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THE SYSTEMATICS OF ERROR: WITH SPECIAL REFERENCE TO THE ERGONOMICS OF OFFICE AUTOMATION

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THE SYSTEMATICS OF ERROR
With Special Reference to the
ERGONOMICS OF OFFICE AUTOMATION

by

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Dedication

This dissertation is respectfully dedicated to Dorothy Haglund, a true professional.

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Abstract

Office Automation is today a pivotal element in the progressive automation of industry, because the mechanization and automation of clerical and secretarial work via computer-based technologies represents a further step in the rationalization of administrative processes, central to organizational communication and control. The phenomenon of office automation also represents a significant instance of the negotiation of technologically-induced change in ongoing organizations, a type of change suggesting that there is perhaps an adaptive, extensional aspect to the phenomenon of formal organization which invites a contribution on the part of social scientists to theorize about methods and methodological issues as they are reflected in practical applications.

At one level, changes associated with the implementation of computer-based technologies in offices imply changes in organization structure and process, in the division of labor corresponding to that process, and in the working environments associated with that division of labor. At another level, the type of problems encountered in the implementation of automated office systems may be representative of a deeper class of problems which involve limitations on the predictiveness of current scientific knowledge and conventional research paradigms when applied in the context of complex, ongoing systems engineering projects.

Problems in implementing data processing and office automation systems--specifically, the inability of designers to account for the

occurrence of "implementation failure" and for "ergonomic" problems associated with the use of computer technologies in offices--reveal major deficiencies in conventional organization theories and methods in complex and ongoing organizational environments. A focus on the models and strategies, theories and methodologies underlying the design of jobs and the reorganization of work entailed in the implementation of office automation technology is facilitated by the use of discourse analytic techniques borrowed from linguistic research. Such an approach makes it possible to explain these adverse outcomes, as well as the findings of the many investigators who have noted little significant impact of the introduction of computers on management as an instance of the systematic production of errors in organizational learning.

INTRODUCTION

Among the most fearsome of the giants with whom the Norse gods had to do battle was the Jormungand, a serpent who was especially dangerous because his body encircled the earth, engulfing everything, including the ground on which the hero must stand to fight. The research here reported has that dangerously reflexive quality characteristic of investigations in that area which Knorr (1979) calls "...the peculiar no-man's-land between a sociology and an epistemology of science, and between the context of discovery and the context of justification." This thesis is an inquiry into the <u>usage</u> of a system of knowledge--Systems Thinking--and the development of a particular technology--Office Automation--which reflects that system of knowledge in action.

The phenomenon of Office Automation represents a significant instance of the negotiation of technologically-induced change in ongoing organizations. At one level, changes associated with the implementation of computer-based technologies in offices imply changes in organization structure and process, in the division of labor corresponding with that process, and in the working environments associated with that division of labor. At another level, the type of problems encountered in the implementation of automated office systems may be representative of a deeper class of problems which involve limitations on the predictiveness of current scientific knowledge and conventional research paradigms when applied in the context of complex, ongoing systems engineering projects.

Problems in implementing data processing and office automation systems--specifically, the inability of designers to account for the occurrence of "implementation failure" and for "ergonomic" problems associated with the use of computer technologies in offices--reveal major deficiencies in conventional organization theories and methods in complex, dynamic and organized environments, limitations that must be addressed in accounting for "The Systematics of Error" in Office Automation.

Office Automation

Equipment

At an immediate, descriptive level, Office Automation is most often identified with word-processing technology, which represents a marriage between the intelligence, storage and communication capabilities of computers and a variety of input and output devices, including typewriters, printers, copiers, and dictating, recording and communicating equipment. Three major components make up an electronic (or automated) office: Workstations, which combine a keyboard and a video display unit with a microprocessor and a number of storage devices into a single word-processing unit; Printers, with graphics, alpha-numeric and microfilm capabilities; and Communications Networks, including local networks, public utilities and radio-communications which integrate the separate pieces of equipment and radio-communications which integrate which can integrate the separate pieces of equipment and facilitate the flow of information-processing. (Seybold Report, June, 1981)

The current state of the art in electronic workstations represents two converging lines of development. On one hand, the trend to increasing miniaturization of circuits in microprocessor technology which characterizes the third generation of computers (introduced in 1964 with the IBM System 360) and subsequent hardware, has made possible increasing capacities in speed and storage relative to cost and to the size of the machines. (Sanders, 1981, p. 31) On the other hand, the development of the electronic, text-editing typewriter as an interface between a variety of office machines and computers, represents a parallel trend which received its impetus from the introduction of the "golfball" typing element first introduced in 1961 with the IBM Selectric I typewriter. The invention of this typing ball--and its successor, the "daisy wheel"--significantly increased the productive capacities of the machine by making it possible to capture keystrokes in 6-bit mechanical "codes". It was the efficiency of this electronic code over earlier methods of electrically powering 44 separate key-bars that enabled the development of the first electronic typewriter, IBM's MT/ST, as the prototype in the merger of computers and typewriters in the design of word-processing equipment. (Seybold Report, February, 1979)

The term "word processing"--textverarbeitung--was first coined in Germany by IBM as a marketing concept to define their line of new office equipment, introduced with the MT/ST (magnetic tape/selectric typewriter) in 1964. Initially "word processing" was defined to mean the use of electronic hardware to process words, sentences and paragraphs, to be distinguished in use from "data processing", also defined by IBM nearly a decade earlier as the manipulation of numbers. (Business Week, June 30, 1975)

As these developmental trends in hardware design converge, the line between small computers and electronic typewriters is becoming blurred, and the early distinctions between word processing and data processing are increasingly being seen as arbitrary and artificial. Indeed, the distinction relies more on the identification of markets on the part of computer manufacturers than on any essential differences in the capabilities of the hardware. Manufacturers of word-processing equipment have been announcing through the trade press that, contrary to well-established expectations, it is "word processing" that will "swallow" data processing and finally usher in the long awaited Office of the Future. (Rockhold, 1981, p. 93)

They may be right. Current trends in systems research lend support to these claims, which are reinforced by the phenomenal growth in the information industry over the past half-century.

Industrial Growth

Growth in the information industry has been so explosive that apologists can assert, not unreasonably, a position of dominance for their industry similar to that enjoyed by the railroads in the 1890's and automobiles in the 1920's and '30's. (New York Times, October 11,

1981) There is considerable continuity in this industry. Most of the larger computer manufacturing firms now producing word processing equipment began as manufacturers of office machines--the famous IBM and the seven dwarfs, among them Burroughts, Wang, Honeywell, Digital Equipment, CDC, NCR, and Sperry-UNIVAC--while other original manufacturers such as RCA and Westinghouse dropped out of the running early on.

The first typewriter was manufactured by the Remington Company, originally a rifle factory and, prior to the invention of the typewriter, engaged in the manufacture of farm implements and sewing machines. Through a series of mergers, Remington combined with Rand and then with Sperry; as Sperry-Rand this firm again had the distinction of being the first company to manufacture computers for commercial use. (Sutherland, 1981, p. 49)

Sperry-Rand's UNIVAC-1 was installed at the Census Bureau in 1951; the first computer installed in a business organization, also a UNIVAC, was purchased by General Electric in 1954. In December, 1954, the IBM 650 all-purpose computer was introduced, and by 1956 over 600 general purpose computers had been installed in U.S. organizations. Observers now estimate that there are hundreds of thousands of general purpose computers in U.S. corporations, at a total installed value ranging from $70-90 billion in 1981. (Sanders, 1981, pp. 31-32)

Estimates of the number of word-processing stations already installed range from 350,000 in 1980 (Small Business Reports, June, 1980, p. 23) to 337,000 in 1981 (Seybold Report, April, 1981) Seybold also suggests that there may be an even larger number of word-processing systems in operation as personal computers, for which even unreliable estimates are not easily come by. In 1973 there were 22,500 word processors installed in federal government offices--a number which was expected to increase to over 81,500 by 1980--and by 1982 this market was exploding. Industry observers have estimated that between 1.5 and 2 million electronic workstations will have been shipped by 1981, and other observers predict a $15 billion market in office modernization by 1984. (Business Week, June 30, 1975; The New York Times, October 11, 1981; Rhodes, 1981, p. 40)

In terms of value produced by this industrial activity, in 1954 the value of the 600 installed computers is estimated (by Sanders, 1981) to have been $350 million. From 1963 to 1967 the value of computer hardware shipped in the U.S. increased from $1.3 to $3.9 billion. (McKinsey, 1968) Most recently, the New York Times (October 11, 1981) reports that major manufacturers in the information industry--among them IBM, Xerox, Hewlett-Packard, Texas Instruments and Wang Laboratories, all included in the 500 largest industrial corporations--are expected to account for approximately 15% of the total national output of goods and services in 1981.

In dramatic contrast with the decline currently experienced in other sectors of the industrial economy, manufacturers in the information industry are continuing to improve their position relative to other types of firms. IBM became the industry leader within one year after introducing the 650 computer; by 1980 IBM ranked #8 among Fortune's 500 largest industrial corporations, and #2 in net income. With sales and assets both in excess of $26 billion, IBM is exceeded in assets only by Exxon, General Motors and Mobil Oil. IBM's most powerful competitor (in terms of financial strength) in the competition to automate the office, the Xerox Corporation ranks 38th among the largest industrials and 28th in assets and net income. (Fortune, May 4, 1981, pp. 324-348) Recent settlement in their favor of long-standing anti-trust suits against IBM and ATT can only have the effect of further intensifying competitive activity in this already volatile market environment.

The dramatic growth which these few comparisons exhibit has been largely responsible for claims that the Office of the Future is Now! Business Week confidently reported in June, 1975, that

> "...(I)n almost a matter of months, office automation has emerged as a full-blown systems approach that will revolutionize how offices work."

And yet it is not reassuring that much of the literature on office automation being written in 1981 is still prospective and prescriptive in its approach, focusing the bulk of attention on details of hardware systems design, rather than on the specification of those details in practice. Given this orientation, there is a tendency to overlook discrepancies between predictions made for office automation and the experience that has accumulated over the past few decades. In particular, although many observers (among them Diebold, 1979; Lancaster, 1979; Rhodes, 1981) assert that, technically, all of the components are now available to permit integration of information processing equipment in an automated, or "total" office system, none are yet willing to claim that systems of the sort they envision yet exist. In order to account for past experience in evaluating the degree to which office automation is currently feasible, it is necessary to clarify what sort of system would fulfill the definition of office automation held by its designers. This definition cannot be provided, however, from information given in a technical description of the components of the hardware alone.

Automation

The key to the automation of office work lies in the integration of office equipment in processing the flow of information (formerly paperwork) through the various stages of recordskeeping and control. Defined in this way, a complete technical specification of the hardware would necessarily specify the characteristic features and capabilities of the hardware in terms of the requirements of that process.

The concept of "automation" refers to a particular type of production technology, characterized by the linking together of separate activities into a continuous, machine-integrated process. (Faunce, 1968, p. 45) This is a specific qualification for the use of the term, "automation". Conventional wisdom and, unfortunately, many technical journalists continue to attribute the term to any use of machine systems in production processes, in which case both the benefits and problems associated with automation come to be attributed indiscriminately to all types of technological change.

According to Faunce, there are four distinct areas within the total production process which can be subject to technologically-induced change: 1) Power technology, which designates energy sources available to drive the system; 2) Processing technology, referring to the methods and instruments which carry out operations on materials input to the system; 3) Materials handling technology, which designates the methods by which materials are transferred through the operations in the process; and 4) Control technology, which refers to the methods for the design and regulation of the production process itself. (Faunce, 1968, pp. 42-43)

Faunce conceives of "development" as a progressive evolution of production technology in each of these components through three stages: handicraft production, mechanization and automation. Development takes place by substituting inanimate for human performance of the functions of each component, and by improving the efficiency of the machinery employed to perform those functions. The development of these components is interdependent. This means that technological innovation and problem-solving can be undertaken independently in each component, but beyond a certain point, further achievements in any given area are dependent on the level of achievement in each of the others. "Automation", as defined by Faunce, thus refers to the "automatic control of an integrated production system", a definition which focuses, not on particular types of hardware, but on the function of continuous control in integrating the production process as a whole, once power, processing and materials-handling technologies have all been mechanized. (Faunce, 1968, p. 49)

This definition is consistent with that of other early investigators in this field. Drucker (1962) defined the "logic of automation" as a continuous and orderly process which contains within it the means for self-regulation; and Norbert Wiener identifies the modern computing machine as "in principle an ideal central nervous system to an apparatus for automatic control" in which input may be in the form of numbers, diagrams or even readings from "artificial" sense organs and motors. (Wiener, 1948, p. 26) In testimony before the 86th Congress, John Diebold (1962) identified that self-regulation or correction with the principle of "feedback" (formally defined by Wiener) which principle allows machines to control their own processes,

> "...so that production processes do not have to be designed to take into account the human limitations of a human worker."
> (Drucker, quoted in Phillipson, 1962, p. 25)

The importance of defining "automation" as continuous and self-regulating (or automatic) control of integrated systems lies in recognizing that it is not the features of new office equipment per se which will distinguish automation from mechanization in office work, but rather those properties together with the ways in which that equipment is used.

Applications

It appears that the state of the art in word-processing applications to date may reflect diverging lines of development of the technology, one path oriented to information processing in a production mode and the other oriented to on-line inquiry in an "analytic" mode. Examples of each of these modes can be provided in a descriptive characterization of two prototype "paperless office" systems being developed in the federal government.

Process Control Systems applications as applied to office work imply a planned production approach to the flow of paperwork, using computer-based office equipment to produce large volumes of correspondence and reports, and to record and process transactions. Installation was begun in 1975 on such a system which linked the Navy's Recruiting Command Headquarters in Virginia with 78 locations throughout the country by means of a network of automatic typewriters and dictation units. The production approach is largely centered in clerical work, and has been most fully developed in bureaucratically top-heavy industries such as insurance and banking, in governmental agencies such as the Census Bureau and the I.R.S. These systems are now used extensively to process payrolls, budgets and subscriptions; for inventory control; and for accounting and transactions reporting of all types.

Inquiry Systems approach the use of computer-based technologies in quite a different manner. Lancaster (1979) describes the development of a paperless office system being implemented in the Central Intelligence Agency, beginning in 1976. Based on the growth in machine-readable data bases which has taken place since 1965, and utilizing technology available in 1978, the paperless office--conceived as an electronic information system--provides a terminal with a video display and a keyboard to each user, who will use that system in an interactive mode to create and build files of information, to search data bases to receive text and answers to factual questions, to compose and edit text and to communicate with other users and data bases. (Lancaster, 1979, pp. 99-107)

Interactive inquiry systems have been used in commercial service environments, such as airlines and restaurant reservations and telephone directory services. In professional service environments, inquiry systems such as that described by Lancaster have been used in advertising agencies and research institutes to manage the volume of editing and printing involved in creating copy; and in medicine, law and law enforcement, and in design and development work inquiry systems are used to search data bases and to facilitate communication among professionals working cooperatively.

The justification for the implementation of office technologies, and the benefits attributed to these systems, differ in the two approaches. Process control, or "transactions processing"* systems are usually sold with the objective of increasing the productivity of office workers, given the increasing volume of paperwork, increasing costs of office work (especially in salaries paid to office workers) and increasing

*Lucas (1974) uses the term "transactions processing systems" to refer to this applications mode. Lucas' definition is too narrow, and I prefer "process control" terminology, which emphasizes the underlying principle rather than any specific or immediate use.

shortages of skilled clerical workers. The ability of word processors to manipulate text speeds up the processing of paperwork, reduces the amount of time spent on revision and correction, and reduces the number of paper files, independent of the skills of the typist, thus simultaneously lowering the costs of producing paperwork while improving its quality. (Small Business Report, June, 1980, p. 23)

Arguments for the need for greater productivity and lower costs in offices are based on data which shows labor costs increasing at a rate of 6% per year, and computer processing and storage costs decreasing at 25% and 40% per year, respectively. (Burns, 1977, p. 60) Improvements in price have been continuing over the past 30-50 years at a corresponding 25% rate. (Branscomb, 1982, p. 755; Allen, 1982, p. 78) Costs of office services have shown the greatest rate of increase as a percentage of total operating costs to firms and their stockholders, office costs representing 20-30% of the total in 1965, and 40-45% in 1975. (Business Week, June 30, 1975) Increases in "people" costs are accelerating, moreover, and Allen claims that hardware costs are already an insignificant percentage of the total cost of an application. (Allen, 1982, p. 78) According to the Bureau of Labor Statistics, secretaries are in increasingly short supply, and the gap is widening. With secretarial schools already graduating only two-thirds of the 305,000 jobs open each year, the demand is expected to increase by 1990 at more than double the rate in other occupations. (New York Times, October 11, 1981) When defined in an office production mode, then, the criterion of value lies in reducing the costs of information processing--and doing so largely by decreasing the costs of labor. Office automation so defined is thus

> "...(A) typically American response to perennial shortages of types of trained workers: the use of advanced machinery."
> (Deutsch, Shea and Evans, Inc., 1979)

The definition of office automation in an inquiry system mode is based upon different value premises--in particular, the need to retrieve and communicate information which is critical to analysis and decision-making. Given these requirements, the accessibility of a computer-based inquiring system and the effectiveness with which it can be used in searching data bases and in generating and transmitting information for a variety of purposes are criteria of value considered more important than speed or processing power. According to Lancaster,

> "The electronic system should not...be justified on the grounds of cost savings, but rather in terms of its considerable benefits. The system will put in the hands of the...analyst a tool vastly more powerful than any available to him at the present time. It will disseminate messages to him more rapidly...and will give the analyst analytical capabilities and intelligence production capabilities that are greatly superior to those that now exist."
> (Lancaster, 1979, p.7)

This comment suggests, as an added benefit, an increase in the level of skill and the quality of information to be obtained from an automated office system, in contrast with the assumption of stable, or even declining levels of skill upon which office automation is sold in a production, transactions-processing mode. It is clear that there are already two distinct modes of application of computer technology in offices beginning to crystallize. The relative benefits and limitations associated with these alternative modes of computerization--and implementation--are less clear, however, and we may ask at this point how well these justifications stand up to a review of actual experience over the past 30 years.

Early Predictions and Research Findings:

Predictions of the "Impact of Computers in Offices"--alternatively the "Impact of Computers on Management" or the "Impact of Office Automation"--have been part of the growing literature on computers from the beginning. For the most part, initial predictions of the impact of office automation have been derived from formal analyses of the concept of automation, defined in terms of the capabilities of state-of-the-art hardware at some moment in time.

Accompanying early descriptions of the fully automated Office of the Future, observers predicted, and in many cases prescribed, a restructuring of office work and a virtual reorganization of firms employing computer technologies. Structural change generated by the introduction of such powerful new technologies was expected 1) to reduce the number of clerical and supervisory employees in firms, 2) to move the locus of decision-making up in the hierarchy of the firm and thus increasingly centralize decision-making, and 3) to reduce the number and attenuate the influence and mobility of middle managers. (Leavitt and Whisler, 1958; Hoos, 1957, 1960; Drucker, 1962; Whisler, 1970) Based upon these predictions, which centered on claims to increased productivity, observers also predicted a growing dissatisfaction among white-collar employees which would be manifest in increasing support for clerical and other "white-collar unions". (Hoos, 1969; Hardin, 19 ; Blum et al, 1971)

It is interesting that, although early observers were often diametrically opposed in their evaluation of the consequences of the trend toward office automation as a boon or a bane to society, they were in agreement on predicting a trend in computerization toward greater centralization of decision-making and displacement of employment. More recently, Daniel Bell (1979) has suggested that

> "...the new revolution in communications makes possible both an intense degree of centralization of power, if the society decides to use it in that way, and large decentralization because of the multiplicity, diversity, and cheapness of the modes of communication."
> (Bell, 1979, p. 6)

The manner in which Bell qualifies his predictions reflects an increasing awareness that the impact of technological innovation cannot be predicted simply from analysis of equipment specifications independent of use, and that formal system specification is in itself insufficient to account for the full range of outcomes of (even) designed systems in action. Confirmation of this position can be found in evidence which is accumulating from field research and users' self-reports indicating that neither the benefits nor the problems prospectively attributed to office automation have yet to be fully realized; notwithstanding that, a number of unanticipated problems have emerged in the interim to render outcomes even more uncertain, and Allen claims that in many corporations computing is at a crisis point. (Allen, 1982, p. 77)

The displacement of white-collar employees--particularly middle managers--and the centralization of decision-making predicted by these early observers have not yet occurred to any significant extent. However, these early observers did not anticipate the range of social and technical problems which are increasingly recognized to accompany the implementation of new office technologies in ongoing environments, nor did they predict a set of ergonomic problems which have emerged in some man-machine environments characterized by the use of these new technologies.

In this thesis I will argue that the conventional predictions of both critics of and apologists for office automation break down on the same points. The points of vulnerability in office automation can be identified with problems in implementation--the management of change--and ergonomic problems--the quality of the working environment. These problems are inseparable in ongoing systems, and are interrelated in time.

Implementation Failure

Based on the technical capabilities already achieved in equipment design, office automation (defined as continuous and integrated control of information processing) was expected to have been in widespread use by now. (Faunce, 1968; Diebold, 1962, 1979; Lancaster, 1979, and others) However, these projections have yet to be fulfilled.

To date, the use of computer-based technologies in office work has largely been restricted to the mechanization of routine, high-volume clerical operations and little progress has been made in the direction of more analytic applications in planning, problem-solving and organizational design. McKinsey (1968) distinguished between technical feasibility and operational feasibility and argued that while the use of computers in office systems had facilitated cutting clerical payrolls, the real operational changes expected to follow from managements' use of computers in decision-making are yet to come. More than a decade later, Lucas (1981) reports that existing information systems in use in U.S. organizations continue to be characterized by a wide and pervasive gulf between transactions processing systems (systems in which no decisions are actually made, and which are largely applied to clerical work) and management information systems (in which information is used to support decision-making and planning.) (Lucas, 1981, p. 39)

Many observers have reported that office systems technologies have had little, if any, impact on managerial decision-making, or on the structure of office work in general. (Whisler, 1970; Lucas, 1974, 1981; Diebold, 1979; Rhodes, 1981) Whisler (1970) found that the effects of office automation on employment varied consistently with organizational level. Although Whisler noted pronounced displacement of clerical employees, he found essentially no relationship between the use of computers and the span of control, which would indicate greater centralization in decision-making. In his study of computer use in local government, Dutton (1978) found that the sophisticated capabilities of computing technology had little effect either on the use of computing by management or on the benefits they perceived to be possible from increased use.

Reporting similar experiences in his survey of U.S. corporations, Diebold cites his recent study to the effect that

> "...(M)ore organizations consider themselves closer to the ADP environment of the 1960s than to the IRM environment of the 1980s. Despite this, practically all felt that they were technologically advanced."
> (Diebold, 1979)

A second finding contrary to expectations is that although there has been significant displacement of clerical employment, neither the relative number nor the absolute number of clerical employees has decreased as predicted, although the proportion of women employed in these jobs has increased. (Jaffee and Froomkin, 1968; Alexander, 1969; Lucas, 1981) Instead, the demand for clerical workers has increased rather than decreased over the past 25 years. According to a 1979 report issued by the Office of the U.S. Comptroller General word processing installations in the Federal government have exhibited a range of problems and have in general failed to demonstrate any increases in productivity as a consequence of implementation of word processing equipment. That equipment--in use for over two decades--has yet to repay the costs of these systems to the government, and thus cannot be considered cost effective. (Comptroller General's Report on Word Processing, 1979, p. 1)

Given the productivity objectives justifying the purchase of new office equipment, this constitutes a serious indictment of computer technology in use in offices. One factor which may account for the failure of these installations to be cost-effective involves the degree to which automated processing can be fully realized in these environments, and the degree to which such installations are conceived and managed as "systems". The U.S. Federal government, through the National Archives and Records Service (NARS) of the General Services Administration (GSA) has officially defined word processing in terms of integrated systems, characterized by

> "...the production of written communication using automated technology, trained people, and system management procedures."
> (Comptroller General's Report on Word Processing, 1979, p. 1)

According to this definition, most clerical systems in operation to date--including those installed in federal offices--do not fit the criterion of continuous, integrated processing required of automated systems. In 1975 the m ǥority of word processors installed in U.S. organizations were stand-alone units, used for separate tasks. (Business Week, June 30, 1975). In 1981 many offices now employing "automated" office equipment still contain a collection of dissimilar machines which may be incapable of integration and in no way reflect systems organization or management. (Rhodes, 1981, p. 49)

However, notwithstanding the limited applications which have so far been implemented, even those basically clerical operations that have been mechanized by office automation technologies are indeed transforming the nature of office jobs and resulting in a virtual reorganization of work roles and relationships, albeit in ways not previously anticipated nor yet fully understood. The uncertainty which accompanies implementation and use of computer-based office technologies is itself emerging as a major source of the type of "people problems" which observers see undermining not only the development of computer applications, but as destabilizing to organizations and individuals working in them.

> "In spite of glowing prospects many businesses today face problems in their use of our wonderful technology so serious that they threaten to jeopardize the bright future not only of many computer professionals but also of the companies that employ them. Matters are at a crisis point in computing for many corporations. Technology by itself is not enough." (Allen, 1982, p. 77)

It appears that implementing new computer-based technologies in offices may present a more formidable problem than designers had anticipated. A clue to the nature of the difficulty may be given in the problems which users cite as reasons for the unspectacular use of computer technology to date:

1. There is a perceived overemphasis on formalization and quantification, which is rejected by decision-makers.

2. There has been resistance to structural changes in office work by employees at all levels. "Managers reject the attitude, common in word processing, that says in order to automate, they must restructure their organizations...." (Rhodes, 1981, p. 44)

3. Conflicts have occurred between clerical, managerial and data processing personnel in carrying out their responsibilities to the total work process. These conflicts can lead to errors in programming and data entry which create unreliability in that process, and which can lead to the disuse of computer-based systems, and/or the retention of

parallel manual procedures as backup systems, which further adds to the expense of computerization. A recent study by the Government Accounting Office reports that "...only 2% of the software contracted for by the government was usable as delivered." (Head, 1981, p. 56) Daniel Couger notes that problems in communication which arise between data processing departments and users are creating morale problems among programmers which relate directly to job design issues. (Couger and Zawacki, 1979, p. 149)

4. Employees' fears that they will lose their jobs, or that their skills will become obsolete have been reflected in resistance to computerization, particularly in its early stages. These fears have been intensified, and in some cases created, by insensitive implementation efforts which can focus attention on, or even create conflicts of interest within organizations. As early as 1975 unilateral attempts to impose centralized word processing centers in such companies as Young & Rubicam, McGraw-Hill and Security Pacific National Bank were recognized to have not only failed to produce any significant cost savings, but to have created serious morale problems in the process. (Business Week, June 30, 1975) More recently, a flood of literature has emerged in the past few years which increasingly attributes these problems in implementation to social, rather than technical, factors concerning the design--and change--of jobs and working arrangements.

5. Certain capabilities and some computer applications are effectively precluded by the manner in which computer-based systems

have been implemented in the past. Reproducing existing structures and procedures with new equipment and techniques, and formalizing routine data-handling procedures in separate areas of the organization is a common practice and one which can create unanticipated problems in coordination and management. (Diebold, 1979) Under these circumstances, the degree of integration required in order to automate information processing will be difficult, if not impossible, to achieve.

6. Implementation problems are often manifest in an "excessively long" period of development which can generate considerable upheaval in the normal functioning of organizations, adding to employees' distress and resistance, and adding to the costs of computerization, which further elicits resistance from managements. A report on office automation in banking and insurance recently issued by Strategic Business Services, Inc. concludes that "...implementation of office automation in most large companies with multiple offices and plants will take seven to ten years." (Rhodes, 1981, p. 40)

7. Finally, computer systems once installed at such great cost have tended to become inflexible and "brittle". These systems are expensive, and changes that must be made later on are beyond the direct influence of users, who must now field them through an internal data processing department with its own established procedures and priorities. Moreover, change is often prohibitively expensive, especially when these additional costs are subsequently charged to users and departments

rather than being absorbed by the firm as a whole as part of its capital investments. Thus, additional costs of tailoring the system to its users constitute a severe constraint on decision-makers to continue to maintain an installed system even if it doesn't meet their needs. This constraint appears to extend to the replacement of equipment. According to the Government Accounting Office survey of computer-based systems in federal offices, most of the equipment now in use is considered obsolescent, with an average of 7 years for most information systems. (Head, 1981, p. 56) We may note in passing that if it takes 7-10 years to implement these systems, and if the equipment becomes obsolete in 7 years, this would be sufficient to undermine the most elegant of designs. Allen notes that at the same time that there is severe competitive pressure in the business environment to computerize their information systems, there are even more difficult problems being now experienced by established users in updating older computer-based systems which have been in use over a decade, and which have become inflexible and costly. These systems are even more threatened competitively by the continuing rapid pace of change, and Allen cites cases of information systems which have become so out of date and--even worse--which had originally been so poorly designed, that "they simply could no longer support the business." (Allen, 1982, p. 79)

The term "implementation failure" was coined by Lucas (1978, 1981) to refer to the apparent failure of installed office automation technologies to achieve the continuous and integrated processing of information at all levels which is the essential characteristic of automated systems. Thus defined, a system in which applicatin is restricted to routine, high-volume clerical work represents a relative failure of implementation, in light of the possibilities represented in currently available hardware and known applications. It may be that Hammer is correct in asserting that "(N)obody wants office automation anyhow." (Rhodes, 1981, p. 40) but this analysis is much too simple. Lucas argues that a large part of the reason for systems failure is organizational rather than technical in nature. (Lucas, 1978, p. 197)

The Report of the Comptroller General cites deficiencies in the processes of implementation as a major reason for inefficiencies and even failure in word processing systems in use in the federal government, specifying the following problem areas:

1) Most agencies failed to conduct feasibility studies from which productivity would have been assessable.

2) Most agencies did not consider alternative equipment configurations in terms of cost-benefit studies.

3) Most agencies failed to plan for or to conduct studies on the impact of the new system on personnel.

4) Most agencies failed to conduct post-installation reviews necessary to comparisons of productivity.

The report concludes that "This lack of evaluation by most agencies caused many word processing systems to fail." (Comptroller General's Report on Word Processing in the Federal Government, 1979, p. ii)

This explanation is consistent with that offered by James Driscoll, who attributes the failure of office automation systems to three factors: 1) the absence of organizational diagnosis, 2) the failure to design the social organization to make maximum use of new equipment, and 3) failure to consider the dynamics of organizational change. (Driscoll, 1981, p. 7) Ultimately, implementation failure implicates the predictiveness of various design and management methodologies, and is reflected in the inability of designers to realize their intended results, a problem worthy of research in its own right, and one which has implications for designed systems beyond computerization.

Computer Ergonomics

Recently a number of complaints have been reported in the press which indicate a growing concern for the health and well-being of office employees working with computers and other new computer-based office equipment. In Sweden and Canada clerical unions have succeeded in constraining the use of computer-based technologies by demonstrating health hazards associated with prolonged use of video display terminals. Over the past decade, research efforts have been undertaken in the U.S., Europe, Japan and New Zealand which attempt to account for some occupational health issues as symptomatic of underlying ergonomic factors present in office environments, which environments are undergoing change as a consequence of the introduction of computer-based technologies.

The term "ergonomics" was coined by the Ergonomic Research Society of Great Britain in 1959 by combining the latin expressions for "work" and "natural law" to refer to a range of human factors involved in equipment design, an area which is becoming increasingly important and is expanding in scope as computers are more widely implemented in a variety of environments.

The concept of ergonomics offers a way of expressing the otherwise intangible costs associated with various production technologies as a measure of the "goodness of fit" between human operators and the human-machine environment in which they work. This relationship can be stated in terms of an "economy of effort" associated with a given set of tasks in a given environemtn, which provides a means of relating the health and well-being of employees to measures of productivity for the organization as a whole.

In his review of recent research findings, Dainoff (1981) reports high levels of visual and musculoskeletal complaints among operators of video display consoles used in computer-based office systems. Much of the research which has been done to date has been initiated in response to complaints and job actions on the part of workers and their representatives, and carried out with the support of governmental agencies, both in the U.S. and abroad. In the U.S., the National Institute for Occupational Safety and Health has begun a program of health hazards evaluation which responds to requests for investigation from employees and their representatives. The findings from these research efforts relate symptoms of fatigue and other adverse physiological problems and expressions of job dissatisfaction to a set of physical attributes.

These physical attributes are characteristic of workplaces employing computer-based technologies--and especially video display terminals--on three dimensions:

1) Ergonomic factors are identified with attributes of task lighting, and anthropometric characteristics associated with the physical dimensions of the design and configuration of office equipment and furniture.

2) Environmental factors refer to physical aspects of office environments per se, independent of equipment configuration, focusing especially on the quality of the air as indicated in measures of temperature, humidity and the presence of radiation or other contaminants.

3) Psychosocial factors relate to the effect of factors such as job demand, work content, and work-and-rest schedules on fatigue and satisfaction.

Field assessments of office environments conducted as part of health hazards evaluations have revealed suboptimal levels of lighting and ventilation in offices using computer terminals, and the use of office equipment and furnishings which are either poorly designed or insufficiently arranged to support the physical requirements of operators working with computer terminals. In addition, preliminary findings from these field studies also suggest a strong relationship between work demand and task structure and reports of visual and postural problems and job dissatisfaction. However,

> "...(E)vidence of causal linkages between specific ergonomic attributes of the workplace, and specific patterns of symptomatology are lacking."
> (Dainoff, 1981, p. iii)

Ergonomic problems indicate another limitation on the predictiveness of engineered systems, in this case the inability of formal design methodologies to account for unanticipated outcomes--or side-effects, such as health hazards--which may accompany the implementation of technological innovations in ongoing social environments. The ability to predict and control ergonomic problems associated with the use of computer-based technologies in offices would be of benefit not only to employees' health and well-being, but also to their employers' attempts to increase the reliability and efficiency of operations.

Recently, union organizers have recognized in the emergence of ergonomic, health-related issues associated with new office technologies a vehicle for articulating the concerns of office workers about their jobs which is more direct and compelling than wage and compensation issues as a basis for recruiting white-collar employees, who traditionally have been resistant to union membership. It may be the case that awareness of emerging "ergonomic" problems can at once be a source and a consequence of problems in implementation, which would increase the uncertainty and limit the feasibility of implementation of otherwise conceivable applications in certain environments.

This reflexivity is a serious and not unfamiliar problem in social science research, and particularly research directed to solving social problems. One characteristic of goal-directed research is that the findings do not always support the original, narrowly-defined research problem, but raise instead totally different patterns which extend beyond the logical and definitional capability of original hypotheses to account for them. In an earlier generation, the Hawthorne researchers "went looking" for human factors which they thought were involved in individual physical and performance capabilities, and found instead social factors which could not be sufficiently accounted for from within a social-psychological model referring only to individuals. Since that time, the hypothesized relation between productivity and satisfaction has yet to be confirmed by test; the needs these researchers identified in individuals have yet to be demonstrated empirically; and, by extension, the recommended methods and strategies derived from these models have yet to elicit the high levels of motivation and associated increases in productivity that were once expected.

Research into the ergonomics of office automation appears to be taking a similar path at present, identifying ergonomics with human factors engineering, a field which may be considered a sub-discipline of applied psychology. Field research has been goal-directed by workers' self-reports and by complaints of their representatives about employees' health and well-being, and justified (to management) because these problems inhibit overall productivity. In "looking for" human factors explanations for the complaints of VDT operators, field researchers have tended to focus their definition of the problem for research on the physical attributes of the working environment, and on the individual man-machine interface. However, the findings they cite, and the changes they recommend in organizations, demonstrate the influence, not only of physical attributes characteristic of different working environments, but of social factors entailed in the definition and organization of work, factors which cannot be reduced to a physical description of the workplace. This may be one reason why a "causal linkage" cannot yet be obtained between descriptions of the workplace and the incidence of health hazards and complaints.

Ergonomic problems represent a composite phenomenon comprised of a number of separate but interrelated factors in ongoing systems. Even as the use of the concept is becoming more widespread, the identification of "ergonomic problems" with physical attributes of the workplace is already too narrow to account for the findings to date. Psychosocial factors cannot be generated, given this definition, by physical arrangements alone, and we will argue that an interpretation which is based on physical factors and yet interprets the results in terms of psycho-social factors is over-extended beyond its definitional base, and to that extent is invalid. A broader--more socially-relevant--explanation for ergonomic problems must be developed instead. We will argue that in order to account for problems in the <u>use</u> of computer-based technologies in office environments, it will be necessary to broaden the concept of human factors--and ergonomics, conventionally defined--to include a set of social factors beyond those which can be described in physical or psychological terms, and we will suggest instead the model of the "socio-technical system" as an alternative framework for research and design.

Organization of the Dissertation

The Research Problem

The first step in broadening the perspective for investigating problems in the implementation and use of computer-based systems is to reconceptualize the problem for inquiry to reflect the full range of contingencies upon which implementation of engineered systems is based. The problem to which this thesis is addressed is the regular--systematic and systemic--occurrence of "error" in the implementation of complex engineered systems in ongoing environments: specifically, the occurrence of implementation failure and ergonomic problems associated with the introduction and use of computer-based technologies in office environments.

Research problems associated with the implementation--as opposed to the design and extension--of formal systems differ significantly from those investigations which are subject to laboratory and/or theoretical control. Office automation is a complex social-technical phenomenon undergoing continuous change brought about by the development and use of new products, markets and technologies. It is also a phenomenon which has been the subject of an increasing volume of literature in business and trade publications, academic texts and journals, and in the popular press for over three decades. Over time, an element of reflexive uncertainty has emerged in the implementation of office technologies which is at least partly a consequence of controversies and changes-in-use of the definitions of basic objectives, concepts and methods from which applications are derived, and from the uncoordinated attempts of numerous "observers"--investors, competitors, managers, employees, consumers and researchers--to influence the outcome of those controversies in their favor by acting within the change situation in such a manner as to alter the conditions under which outcomes were initially predicted. This description of the phenomenon suggests two possible definitions of the problem for research which are not adequate to account for the full range of outcomes:

Technology Assessment: In the early literature in the field, and still in much of the popular literature on computerization, the problem for inquiry is that of "assessing the impact of computers--on management, on organizations, in offices". The objective of technology assessment when expressed in this form defines a problem for research which is at once too broad and too vague to permit of meaningful analysis, and which is thus subject to degenerating into polemics. The phenomenon of computerization is complex and dynamic. However, the form in which the problem for research is stated in "technology assessments" assumes that computer hardware and software can be described as a general type of technology, similar over time and across manufacturers in the U.S. and abroad. Such a problem approach also assumes a (similarly unwarranted) general or formal identity among organizational environments over time, which is based upon presumed functional relationships which are insensitive to change. Moreover, to "assess" a technological innovation, such as computers, not only involves implicit value judgments in performing the analysis, but also presumes (again, without justification) an underlying value consensus among observers of this phenomenon, upon which such assessment is based. We will argue taht this presumption of order is itself part of the problem of implementation. The technology assessment approach to defining the problem for research fails because it is first necessary in predicting outcomes to identify the problem for research in terms of a specific set of outcomes in a specific environment, which may still be highly complex and subject to the uncertainties of decision-making--factors which exceed the limitations of purely "technical" analysis.

Conventional "Scientific Method": The phenomenon of implementation is inherently complex, dynamic and reflexive because of the influence of perception, choice and action which underly the negotiation of technologically-induced change in ongoing environments. Reliable prediction of a set of consequences, once specified, that might follow from the introduction of new technologies is subject to a range of contingencies which are normally "controlled"--i.e., eliminated from consideration--in conventional research designs, in which one or more factors of a complex phenomenon are isolated for scrutiny.

Health hazards investigations are an example of a particularly difficult type of research in which the problem for investigation is an "action" problem--or, worse, an "intervention" problem--the description of which is inseparable from its context. In this type of research, the process of abstracting out of the whole complex phenomenon a few factors considered important for inquiry may overlook many of the complexities which make it difficult to apply technical knowledge in the world outside the confines of purely formal discourse or carefully controlled laboratory conditions, and at the same time introduce an implicit bias in the definition of the problem which cannot be identified (or eliminated) in the research design. In ongoing environments most of the action is not subject to the control of the researcher, who knows in any case that the act of intervening in that ongoing process introduces uncertainties by altering initial conditions.

Predicting the consequences of technologically-induced change in ongoing environments is also a different sort of problem for inquiry than that of constructing a new system (even if complex) from a blueprint in an environment which is not ongoing--i.e., which is unconstrained by the presence of employees, physical plant, clients and customers, suppliers and neighbors. We will argue that this reflexive uncertainty, which we are coming to recognize as a common factor in computerization, is a characteristic of systems engineering in general, and that serious methodological problems associated with the use of conventional research approaches to predicting the outcomes of change in ongoing, organized environments may also themselves be part of the problem of implementation. These methodological problems must be addressed before any adequate explanation can be given for the findings reported to date.

Methodological Problems:

1) Conventional research methods based on statistical analyses of data obtained from surveys and case studies are inappropriate for studying implementation and ergonomic problems in the field, because these problems are dynamic and ongoing, complex, often unique to a specific environment, and often interrelated. These characteristics may contradict the qualifying assumptions upon which valid correlation of findings is based in descriptive statistical research.

Researchers in vision and human factors studies point out that there are difficulties in the availability of instrumentation and in data collection and measurement. For example, there is a lack of instrumentation sufficient to measure low levels of radiation emissions for purposes of health hazards evaluations; likewise, there is a need for instrumentation to measure optical changes associated with eye strain, in order to account for vision complaints. (Dainoff, 1981)

In addition, there is an absence of longitudinal studies, which studies would be necessary to identify chronic, low-level exposure to hazardous ergonomic conditions in the workplace, and possible effects on worker health and well-being as well as on organizational productivity. Current methods of definition and measurement are insufficiently fine-grained to account for any but the most abrupt changes, and therefore gradual accumulation of changes cannot be detected.

2) The phenomenon of interest is undergoing continuous change, which makes valid inference difficult, especially when using conventional

descriptive methods of analysis. Not only do perceptions of the problem differ among observers, but the definition of the problem for inquiry can change as the research progresses, and as earlier problems are "solved". In many cases, computer technology is changing more rapidly than the time required to undertake a full-scale research program at a given site; in addition, the internal composition of the organization undergoing change and observation may be constantly be transformed by turnovers in employment at all levels, by changes in ownership or management or the production orientation of the firm, and/or in the broader environment in which the firm is located. Moreover, in large and complex organizations, it is possible that observations made in one division of the firm, or at one geographic location, cannot be generalized with validity to other sectors of the "same" organization.

3) Most of the field investigations which have so far been addressed to assessing the impact of computer technologies are not theoretically grounded; therefore, not only are they vulnerable to charges of "reinventing the wheel", but the findings derived from measurements of different concepts and variables under different conditions of observational scope and control are neither comparable nor additive. This lack of theoretical grounding can partly be attributed to the multidisciplinary nature of research in the implementation of technology, but often there is not even passing reference to theoretical concepts or principles, nor even to other empirical research efforts--much less to operationally well-stipulated models. Theoretical relevance is difficult to obtain when different

disciplines must contribute to a complete explanation of a complex phenomenon, but the problem must be addressed if sufficiently powerful theories of organization are to be generated to guide such multidisciplinary efforts.

Dainoff mentions as a continuing methodological problem in his review of current field studies that little of the material on ergonomic problems has been published in the traditional scientific literature; therefore, it has not necessarily been conducted nor reported according to the traditional standards of scientific research (for all their limitations) as maintained by refereed journals, and detailed review of methods of sampling and statistical analysis and interpretation is often impossible. In spite of these problems, he notes that recommendations and standards are often imposed as a consequence of these field studies. (Dainoff, 1981) Inasmuch as this type of research project ends, not with an independent confirmation of the findings, but with an imposition of recommended changes, the results and conclusions necessarily become part of the phenomenon for subsequent investigation, but seldom are they so recognized.

4) A similar difficulty is associated with the manner in which the problem for research is defined. Multi-disciplinary research is especially subject to problems in the commensurability of terminology and measurement and also to problems of methodological standards of validity among researchers educated in different disciplines and fields. In this case, the goals and presumptions of researchers are as much a

part of the definition of the problem for research as are the goals and assumptions of the members of the organization undergoing investigation.

5) Finally, the phenomenon of technologically-induced change is reflexive, embedded on the one hand in the development of machine systems and progress in professional and technical fields associated with the design and development of these systems, and on the other hand in the use of the technology within an ongoing environment. The use of technology is, moreover, confounded by choice--specifically, by "managerial prerogative", which may vary even with the same person in decision-making authority over time, and by the various forms of compliance and resistance exhibited by subordinates.

The reflexivity entailed in field research is compounded when that research is initiated in response to complaints or to social and/or political pressures--with, then, the presumption of an implicit "advocacy" mode. Research which responds to social problems is vulnerable to inherent problems of bias and control over the data, much of which consists of subjective reports. These problems are not so severe in laboratory studies and in some controlled studies in the field. However, laboratory studies cannot simulate the full range of uncertainty and complexity characteristic of contemporary office environments; the consequences of the inquiry do not have the same "realistic" effects on the subjects; and conclusions reached under conditions of controlled observation lose their validity when extended to environments over which experimental control cannot be maintained.

Maintaining standards of validity is difficult in conducting field studies, where investigation of subjective complaints embeds the inquiry in a public context where the advocacy of one point of view violates standards of scientific objectivity. When the subjects of inquiry in those environments are also agents of change, the problems of validity are compounded, and it is possible that findings cannot be generalized, even within the same project at two different points in time. Health hazards investigations have a particularly politicised nature when these findings are used as legal grounds for collective action and for the imposition of governmental regulations and standards.

Achieving sufficient depth of analysis for reliable prediction is a formidable problem under controlled conditions of observation, but even more intractable when the phenomenon is changing rapidly, and when that change is at least partly a reflexive consequence of the perceptions and actions of those affected by it. The difficulty of prediction under these conditions has strained the predictive capabilities of conventional methods of research design, a difficulty which suggests a rather different definition of the problem for research. The problem of the implementation of computer-based technologies in offices must be reconceptualized as explicitly complex and reflexive and embedded in a larger social context, within which it might be possible to address these methodological problems in accounting for problems in implementation and use of new technologies.

Historically, the fit between the development and the use of computer technology was closest in the early stages when research scientists were at the same time the inventors and users of new machines and methods. It is possible to see in historical examples a mutual relationship between the potential uses which can be identified for a new technology and the development of hardware, on one hand, and changes in the social structure of the ongoing system defining the context of implementation on the other. However, there is no accepted theoretical position which can fully account for the interaction between the generation of theory and method in ongoing endeavors. We will attempt to provide an explanation for the dynamics of organizational change (as Driscoll has called for) which explanation will reflect the interaction between formal systems design and the implementation of new methods of work. An adequate explanation for this interaction must necessarily account 1) for the identification and selection of goals and objectives; 2) for the development of methods, tasks and processes; 3) for the costs associated with different alternatives; and 4) for methods of inquiry and implementation of change which underlay these activities. Such an explanation could then serve as a framework for conducting organizational diagnosis research, and for designing adaptive and supportive social-technical systems to accompany the introduction of new technologies.

The expanded definition of the problem for research in this study will address these factors as constitutive of a "context of development" for computerization, which context is an essential aspect of prediction or explanation of outcomes for current or future development. Specifically, we will investigate 1) the degree to which the introduction of computer technology into office environments over the past three decades has represented automated technology, by considering the current state of the art in word processing, and teh degree to which automated text processing is now feasible. We will argue that automated text processing as now conceived is not yet feasible, and consider the degree to which this objective is well-conceived--i.e., the extent to which automated text processing represents a "solvable" problem. We will argue that a concept of information processing which does not consider the generation of information as well as the transmission of information is bound to limit analysis to measures which do not cover the question of productivity in offices. 2) We will consider the issue of skill--and learning--as it is implied by alternative conceptions of word processing embodied in the definitions of tasks, jobs and the organization of work and working relationships, and investigate the nature and degree of training implied in word processing applications to date, relative to the requirements of the implementation and use of this technology. 3) Finally, we will consider alternative conceptions of the "systems approach" as applied to the management of technologically-induced change in organizations. We will consider the current state of the art in automated office equipment in the light of currently alternative conceptions of systems management, and of models of business organization and management procedures and strategies in contemporary office environments as a basis for evaluating the degree to which a "systems approach" can be demonstrated as characteristic of the management of contemporary organizations, and, specifically word processing systems in offices.

The Systematics of Error

When large-scale engineered systems become so complex that their own operations can be their only effective test, then prediction of the full range of outcomes associated with the operation of the system becomes urgent. Methodological problems associated with the introduction of technologically-induced changes in ongoing environments limit the predictiveness of formal design models under certain conditions characteristic of the context in which implementation takes place, including: 1) turbulence--especially characteristic of periods of social and technological change; 2) uncertainty--often a consequence of technologically-induced change; and 3) complexity--characteristic of large and highly differentiated organizations.

The limits to predictiveness are reflected in the occurrence of error, which is perceived alternatively as a failure to accomplish some objective, defined as an output in a (formal) model, or as the occurrence of some other outcome(s), which are perceived to some extent to be undesired. To the degree that those limits can be defined and identified with conditions which are a regular occurrence in certain environments, and to the degree that it is possible to demonstrate a relationship between that set of conditions--in the context of a formal design model--and the occurrence of error, defined as some specific problem recognized in terms of that model, then we can speak of such "error" (or problem) as a systematically occurring feature of the engineered project in that environment, which is produced along with the "intended" outputs as a regular by-product of the designed process--hence, the "systematics of error".

We propose to develop this definition of the problem for research by focusing, not on the problems associated with implementation of computer-based technologies in offices, but on the feasibility of implementing automated office systems--as defined--in different ongoing environments. In defining the problem of implementation in terms of feasibility, we recognize that the phenomenon of technologically-induced change is embedded in a context which sets constraints on what is possible in that environment. According to Lucas

> "(T)he probability of successful implementation of computerized systems depends...on the ability of the design and implementation team to overcome the constraints inhibiting successful implementation."
> (Lucas et al, 1980, p. xi)

On the basis of studies conducted in the U.S. and Europe, Lucas argues that the key constraints now lie, not in data processing, but in four areas which are "non-technical": 1) In basic business systems which have not been appropriately formalized; 2) in the failure to involve users effectively, which leads to a failure of effective communication between users and data processing departments; and 3) in external constraints which impede data processing development. Foremost among these constraints, however, is 4) the "human acceptance of the system". (Lucas et al, 1980, p. xii)

Defining the problem for research in terms of feasibility makes it possible to partition the problem into two distinct, but interrelated domains--or contexts. The narrow focus of the problem is addressed to explaining the occurrence of implementation failure and ergonomic problems under varying conditions, including those conditions entailed by the broad focus, which is addressed to explaining the limitations on the predictiveness of formal models in engineering change in ongoing environments.

There are two functional requirements which combine in the implementation of technological innovation--the organization and communication of the information-in-use in the system, and the construction and maintenance of a machine-system to carry out that process. Together, these requirements make up the assessment of feasibility of implementation of new technologies. The development of machine systems is a process at the heart of the "American System" of manufacture--with automation representing the ultimate rationalization of work and highest possible efficiency in producing a return on investment. The organization of information-in-use is based on the development of data bases, of formal languages and procedures defining applications and work processes, an area of study which is considerably less developed. Defined in these terms, the relatively slow development in the use of automated office systems can be attributed to three general problems, or constraints on the process:

1) Computer technology does not yet possess the capabilities that would make possible the kind of inquiry envisioned by those decision-makers who imagine they can "ask" the computer in their own natural

language to find information or to carry out some task in the same manner they would communicate with a human subordinate--on the secretary-as-robot model. As it happens, there are problems in the design of hardware systems, and problems in the design of computer-systems languages and applications which must be resolved before such uses can become feasible, much less acceptable. Particularly important and difficult at this stage is satisfying the requirement that all prior stages of mechanization are fulfilled in the four sectors of power supply, processing, materials handling and control technologies in order to make it possible to automate the process. A major constraint is in developing communications networks necessary to the integration of the hardware at distributed locations, and in designing and managing the workload as it is transferred through this hardware network, so as not to exceed available processing power. Efficiency of communication is largely a consequence of the adequacy and elegance of software, and although the greatest efficiencies are to be gained in the organization of information, the current state of the art in software design and implementation is reaching crisis proportions, which reflect dramatically on the cost of applications.

2) A second, and related problem, is that management decision-makers do not necessarily employ analytic models of inquiry in searching for information, in defining problems and making decisions, and in planning and organizing the workload involved in information-handling. Thus the complexity of many potential applications exceeds the "affordable" capacities of machine systems to perform in the unstructured manner in

which problems for application may be defined. This problem is responsible for much of the current "software bottleneck", and suggests that much work remains in the development of formal languages and procedures, the requirements of which must be rendered compatible with the current capabilities of the hardware and with the information needs of the organization. In particular, the desire to communicate with computers or with other users in an unrestricted mode using natural languages, much less by voice, currently exceeds the capabilities of even the most sophisticated systems.

3) Finally, the ability to use computers in an interactive, inquiry mode as well as in a transactions-processing mode, implies the acceptability of the system to its users, and their ability to use it. "People problems" associated with the acceptability of the system-in-use refer at one level to the ease of use: Is the system accessible--friendly--and human-engineered so that it is not uncomfortable (or harmful) to use? In addition to the ease of use, acceptability also implies a set of requisite skills not yet widely distributed throughout the general population, skills which are in short supply in existing organizations. The choice is to increase the levels of training to computer programmers, clerks and maintenance technicians to meet the growing need and chronic labor shortages, or to continue to seek technical solutions which will obviate the need for those skills. At a still deeper level of acceptability, the relationship between the individual employee and the organization is implied in this choice of alternatives, which is also reflected in efforts

on the part of employees at all levels (including managers) to undermine the new system, or to deflect its potentially adverse personal consequences by attempting to influence the design and implementation process in their own self-interest.

These are not engineering problems, narrowly defined, nor are they problems in data processing and computer science. Because a strictly technical definition of the problem of implementation is insufficient to identify the constraints on acceptability in the designed system, we will argue that engineering solutions are not only insufficient to resolve problems of implementation and use--the conventional engineering definition of the nature of the problem and the methods for solving it may itself be part of the problem of implementation.

Rationale

It is difficult to predict the outcomes to follow from the introduction of some technological innovation in an ongoing organized system because of the implicit process of negotiation and selection which takes place in the implementation of that change. A complete account of the processes and outcomes entailed in the introduction or transfer of new technologies, such as Office Automation, will necessarily require more information than that which can be provided by a complete description of the hardware, its configuraiton and its accompanying methods and procedures. Prediction will depend not only on a formal specification of the technical capabilities and limitations given by the design, but on a specification as well of the organizational context into which that technology is being introduced and of the decision process by which choices are embodied in new system characteristics.

The concept of feasibility suggests a focus on a set of contextual requirements on which projected outcomes depend. For any complex phenomenon, there will be a set of factors contributing to the outcomes, and the constraints which can be identified to affect the occurrence of error in complex systems likely will be defined in several different disciplinary domains. For this reason, we require an approach which permits multi-disciplinary contributions in identifying that set of factors.

In order to account for the influence of a wide range of contingencies on the outcomes of implementation it is useful to consider organizations as "socio-technical systems", thus reflecting a range of possible constraints, not limited to physical characteristics of the environment, and thereby making it possible to investigate the relationship between social and technical requirements in the process of implementation. Moreover, adopting a socio-technical systems approach to organization analysis suggests a reconceptualization of the concept of organization and, by extension, of the available methods of inquiry.

Organization

This study conceives of "an" organization as a social construct, represented as information and embodied in language. In this view, "organizations" are considered less corporeal institutions than conventionally assumed; rather, organizations can be considered as abstract entities, socially recognized and maintained. Organization is something that people do, a set of activities directed to instantiating someone's definition of reality. Thus the concept of an organization as socially-defined reflects at any given time the knowledge which people can bring to bear on relations which they infer among (or conventionally ascribe to) the elements of their experience, during processes of adaptation to the environment.

Given this definition, the key to the study of organization can be found in the concept of Definition as an ordering relation. System definition is a social process linking the organization of ideas and the organization of action, which is ultimately represented in the structure of "an" organization over time. The process of defining--or, in a specific instance, characterizing--"organization" is inherently methodological in nature, and refers to a process in which methods of research and theories of organization are implicitly associated in the instantiation of knowledge in engineered systems. In such systems, the resolution of methodological problems has a direct bearing on the states and outputs of resultant systems, and an indirect bearing on the ability of observers to predict a range of outcomes.

At this point, abstract issues in philosophical empiricism become concrete limitations in systems engineering. The phenomenon of "error" reflects the perception on the part of some observer that the designed process does not "work", relative to some criterion value. If a formal, technically-specified design for some engineered system may be considered logically complete if it is consistent and everywhere well-defined, then in seeking to account for error, a key issue in the implementation of change reflects a major issue in modern empiricism--the problem of Definition. This issue may be restated in the context of office automation to reflect the missing element: What information must be added to formal systems designs in order to take into account their limiting assumptions? And what are the constraints--and supports--for providing that additional information in any given environment? We will argue that the key to the empiricist dilemma is also the key to the information-management dilemma, and that this issue gives us an insight into the nature of the cognitive requirements on which depends the successful implementation of technologically-induced change in ongoing organizations.

Thesis

This investigation is an inquiry into various definitions of "system" that may be entailed in the introduction of computer-based technologies in offices, with the goal of producing a theoretically-relevant and empirically-supported explanation for the systematics of error--specifically, implementation failure and ergonomic problems--in implementing automated office systems in ongoing environments.

The object of this study is not to discover which is the "best" system, nor even the "best" for a given set of conditions, but to present an explanation for the way that people develop and change organized systems, with the objective of decreasing the disruptiveness of organizational change brought about by the introduction of new technologies, and of making the transitions as smooth and as effective as possible. The outcome of the project should suggest a framework for characterizing alternative forms of office automation, and alternative developmental paths of implementation, each of which can be separately characterized in order to evaluate them for their relative impact in terms of acceptability, which we consider the key both to solving ergonomic problems and problems of implementation failure. Such a framework would provide "in-house" capability for grounded organizational analysis, and should be extendable to the study of other types of technologically-induced social change in organizations as well.

We will argue the thesis that: 1) Structural change (morphogenesis) in organizations is a consequence of the instantiation of new technologies in organized contexts recognized in terms of current and historical models representing the (methodological) knowledge or understanding of actors at all levels. This transformation is not a simple function of the specifications of the technology, nor yet even a complex function of the technology as applied in some specific environment. Rather, the transformation is a reflexive function of the responses of actors to the process of (re-)defining the system which takes place during implementation. 2) During change processes a mutually-reinforcing, systemic relationship can arise between the occurrence of uncertainty and ergonomic problems, including stress. This is especially likely in organizational contexts in which structure is defined presumptively, if at all, and in which strategies of implementation are directed to the preservation of structure. We will argue that it is possible to predict and explain certain types of uncertainty and error during processes of implementation with reference to this underlying methodological context--which describes an implicit knowledge base for the organized system--if only it can be characterized in terms precise enough to support formal analysis.

Methodology

The (systems) analytic concept of "model" will be used to identify the context of implementation with a set of assumptions about the nature of "an" organization and its environment. Then the organization in any given system--expressed as knowledge, or "Information"--can be represented as a set of cognitive constraints. These cognitive constraints take the form of implicit definitions of order, or Systematicity, as recognized by different actors and observers. The notion of cognitive constraint thus designates the limits of the model(s)-in-use, alternatively, the boundaries on the "domain knowledge" which we can identify with different actors in systems undergoing change.

If the "structure" in an organization can be seen as embodied in a set of methods during some period of time, then there is perhaps an adaptive, extensional aspect to the phenomenon of formal organization which invites a contribution on the part of social scientists to theorize about methods and methodological issues as they are reflected on practical applications. We will argue that, in addition to the formal specifications derived from the system design, adequate explanation for the outcomes of implementation and for computer technology-in-use will also require an understanding of these systems of knowledge--or methodologies--which organize the information and working arrangements by which systems undergoing change are defined by their members and other observers.

This thesis will demonstrate a systems analytic approach to organization analysis which makes use of combinatorial logic- and language-based methods of analysis to characterize that domain knowledge embodied in models which represent organizations as abstract entities recognized by their members. If we can use these methods to distinguish the assumptions embedded in these models-in-use, then it should be possible to ascertain alternative ways in which automated office systems might be defined--designed and implemented--by comparing different system structures (order-presumptive vs. adaptive) and by comparing different modes of implementation (structure-preserving vs. changing).

On the basis of this type of characterization it should be possible to indicate models which block the implementation of change, models which are incompatible in combination, and models which can support the requirements for automating office work (in particular), as a basis for evaluating the feasibility of implementation of a given technology in a given environment. This type of inquiry we will call Organization Capability Analysis. In particular, we will be attempting to identify incompatibilities in the definitions of new classes of occupation implied in the emerging models for office automation; and in the definition of the problem(s) for which office automation is presented as a solution. This understanding will help to predict the degree of acceptability of proposed systems as a basis for accounting for reports of implementation failure and ergonomic compliants associated with the implementation of computer-based office systems to date.

The introduction of computer-based systems represents a "natural experiment", the details of which consume an ever-larger fraction of the literature in management, social science and computer science. The sources of data for characterizing these alternative models can be found in this body of written literature from which definitions for office automation can be derived. This literature includes information acquired by organization members as part of their education and information presented to the public by manufacturers of office equipment (and other representatives of the burgeoning "information industry"), in the form of advertisements and (prescriptive, "how-to") articles in the trade and popular press; it includes literature which is "out-of-date" as well as that which is current among scholars of organization theory in 1981.

This information will be analyzed to characterize--and distinguish--these implicit models of organization--or System--in three nested representations, each designating a type and/or source of information which can be identified at each level of abstraction. The representation of the system at each level thus provides a supporting and constraining context--or frame of reference--for subsequent levels of specification in the implementation of computer-based office systems:

1) Methodological paradigms are models defined at the highest levels of abstraction, which stipulate the nature and limits of valid (conventionally acceptable) knowledge and inference, and stipulate how information is to be identified, classified and analyzed (processed) in order to reach conclusions. Methodological paradigms may be embodied in a set of research methods which constitute the basis for the organization of inquiry. We will characterize three methodological paradigms at this level: Inductive Empiricism, General System Theory, and Systems Analysis.

2) Theoretical paradigms refer to definitions of systematicity and order, which are held by people as a basis for recognizing the structure of organizations or firms. These paradigms stipulate the nature and limits of (formal) organizations and the role of individuals in them, the nature and basis for the division of labor, and the distribution of information, decision and action throughout that structure. We will characterize three organization-theoretic paradigms at this level: Management Science, Contingency Theories, and Human Factors/Ergonomics.

3) Strategic paradigms imply action, and refer to models and methods of control embodied in a set of decision rules, instructions, prescriptions, procedures and strategies for controlling and managing the implementation of change of the resulting ongoing operations. We will characterize at this level two alternative developmental paths for office automation: Transactions Processing and Interactive Inquiry Systems.

Directionality can be indicated by distinguishing between alternative modes of orientation at each level of system definition. At the methodological and organization-theoretic levels we can distinguish models which presume order from those which do not; at the level of action and implementation we can distinguish between strategic models oriented to the preservation of structure within organizations, and those which presume--and account for--structural change, suggesting alternative paths of development in the implementation of office automation, and indicating which are inherently stressful.

The constituent assumptions making up these three paradigms, taken together in context, represent the knowledge base of an organization observer and/or actor (and, collectively, for the organization as an "entity".) Changes in methodological posture reflected in these models have implications for practical issues in the design and operation of social systems which are organized in some measure to productive ends. The organization of the chapters in this thesis explores these nested relationships with reference to three major methodological issues which bear upon the nature of the relations which can be exploited or instituted among the phenomena of the world as identified by man: Isomorphy, Uncertainty, and Control.

Justification

Office automation is an interesting problem worthy of study because it represents a genuine phenomenon of ongoing social-technical change which presents us with an opportunity to review past explanations and expectations in the light of subsequent events. There is a special opportunity during the process of change still ongoing to identify aspects of organizational life--and especially the nature of organizational change--which are otherwise submerged in "normal operations". There is more than sufficient material available which can benefit from analysis and synthesis, and, in accounting for past difficulties and for processes of technologically-induced change in ongoing organizations, there is also an opportunity to make a contribution on a practical level to the improvement of these systems and, hence, to improving the quality of working life.

This problem is also interesting because of its potential influence on Organization Theory. Demb (1979) argues that the predictions of early observers reflected a wide range of issues which are not well addressed in the information literature, which, she contends, "...offers neither theory nor model to aid in assessing the relative importance of any one variable or in producing operational guidelines for management action". Those predictions, therefore, have not been confirmed or disconfirmed in any systematic fashion, and Demb argues that even if they were, management would still have to select from "an array of issues and concerns which are apparently of relatively equal importance." (Demb, 1979, p. 44)

A generative theory of organization (such as that here proposed) which could account for the development, operation and change of different organizational forms as well as for their formal structure, would better explain and predict the outcomes of change in organized environments than do conventional organization theories. This approach could prove of benefit in decision-making, especially in the identification of incompatible assumptions in systems design and implementation strategies, and thus illuminate at least one possible source of systematic uncertainty and error in the implementation of technological innovation.

It is the object of this thesis to indicate a way in which some of these predictions might be confirmed, and to build a theoretical framework which will account, not for one issue abstracted from the total for scrutiny (the conventionally accepted method in "normal" science) but to suggest a way in which these issues of ergonomic problems and implementation failure might be viewed as related. Should such a theoretical framework be found which could usefully express the range of issues and problems involved in computerization, then we might suggest an explanation for the predictions, and for the findings of field researchers to date.

CHAPTER 1

ISOMORPHY

The concept of "system" is based on a rejection of the traditional reductive analytic procedures in science which describe and explain some complex entity by factoring it into its constituent parts, from which the phenomenon is reconstituted out of relations established among their characteristic properties, whether conceptually or experimentally. In particular, contemporary sociological methods have been described as having concentrated on the "establishment of basic propositions showing that one part or aspect of society is related to another part or aspect; religion is related to voting . . . education is related to class . . . and so forth" (Buckley, 1967). The consensus among system theorists is that such methods result in one-sided empiricism, which takes the form of collection and manipulation of data, to the exclusion of theory-construction, and oblivious to the increasingly evident fact that "mere accumulation of data, although steadily piling up, does not make a 'science'" (Bertalanffy, 1968, 1972; Rapoport, 1971).

The methods of such investigation are predicated upon the assumption that to understand some phenomenon, one first describes it in terms of its observable characteristics, which then can be recombined in the abstract--by a series of hypothetical correlations--to refer to (and thus to explain) any similar phenomenon. Examples of methods based on this premise would include factor analysis and the methods of concomitant variation.

Although the initial objections to conventional social science center on inductive methods of scientific inquiry--particularly in social and biological science--the systems critique is more far-reaching, extending beyond particular techniques to the philosophical presumptions underlying the methods. The methodology of systems analysis could be though of as a "third position" in modern empiricism, standing in relation to and departure from the methods espoused by the Inductive Empiricists and by the Logical Empiricists, with respect to the fundamental problems posed to the conduct of science by the basic propositions of Empiricist epistemology. These propositions are perhaps most clearly expressed by David Hume, as follows:

1. [A]ll our simple ideas in their first appearance are derived from simple impressions, which are correspondent to them, and which they exactly represent. (Hume, 1966)

2. Objects have no discoverable connection together. All events seem entirely loose and separate. One event follows another, but we never can observe any tie between them. They seem conjoined, but never connected. (Hume, 1966)

Moreover, philosophers since Plato, and certainly since Descartes, have generally been agreed that the phenomena of the natural world are known exclusively through the mediation of the senses; therefore, our descriptions of those phenomena are entailed in our observations and in the structure of the language which we use to represent them. How, then, does one understand the world without simply reflecting on one's own prejudices and opinions?

The Inductive Empiricists, Locke and Mill especially, held that if the ground of knowledge is in our experiences, and if our experiences are discrete observations based on the operations of the senses, then the qualities and relations which we perceive must inhere in something, which is not given to the experience. (This is essentially Locke's indirect postulation of Mind, that something--"I know not what"--in which the qualities of our perceptions must inhere.) On such basis, the world should be knowable by systematic observation.

Consistent with this perspective, Mill's methods emphasize the systematic observation and classification of the differences and similarities among the attributes presented to our perception, which are presumed to reflect the order in the natural world beyond our observations. According to Mill, our concepts are formed on the basis of some similarity among a set of objects with respect to some characteristic property, and are related through inductive inference from that "which we know to be true in a particular case or cases" to that which will be true "in all cases which resemble the former" in such respects (Willer, 1973).

The application of this epistemology in scientific inquiry requires some method by which one anticipates future events on the basis of past experiences. Contemporary techniques of statistical inference from such descriptions follow from Mill's joint method of agreements and differences, which holds that if two or more instances in which the phenomenon occurs have only one factor in common, while two or more instances in which it does not occur have nothing in common except the absence of that factor, then that factor in which alone the two sets differ is the cause--or effect--of the phenomenon (Searles, 1968).

This notion constitutes the basis for the search for necessary and sufficient conditions, and is reflected in the methods of concomitant variation developed by Pearson, according to which the variation in one class can be seen to be contingent upon, or correlated with, the variation in a second class. He agrees with Mill that the future will be like the past (the ground for such inferences) but he does not presume that the universe is absolutely uniform. Rather, he holds that what is orderly is its irregularity, and that therefore our representations of it are probable, not deterministic. Fortunately, however, that probability is stable, and thus although all phenomena are contingent and not causal, the problem becomes a practical one of statistically measuring the contingency or degree of association (Willer, 1973).

Pearson's methods are representative of a statistical tradition which is based on the position that we can systematically arrive at the regularities or order among natural phenomena by carefully making observations and then either describing that regularity directly, as Mill suggests, or measuring it indirectly by computing the precise degree to which our observations deviate from the presumption of a lack of regularity. These inductive procedures constitute a "Generalization from Experience," which is justified on the not-so-empiricist presumption of the uniformity of nature, which proposition is thought to be further supported by the various inductions from the particular regularities among our observations (clearly a circular argument).

Such methods have been capably criticized by Willer and by the systems theorists Bertalanffy, Buckley and Rapoport, among others. I should like to point out certain specific problems which arise in describing and defining the operation of complex organizations. An example of the sort of difficulty one could expect is given by Katz and Kahn's attempt to specify exactly what is "the" organization, as an object of inquiry. They ask: On what basis can we establish even its location and identification? How do we know that we are dealing with an organization? What are its boundaries? What behavior belongs to the organization? (Katz & Kahn, in Emery, 1969)

Generally, common-sense considerations lead us to identify the boundaries of the organization materially, according to spatial contiguity of the constituent elements. There is an alternative, which is to consider the organization in terms of its continuous behavior in time, which may transcend physical contiguity, but in so doing, further abstracts the behavior from the behaving objects in such a way as to make the notion of the limits of the organization more, rather than less, ambiguous. The elements and relations identified within the "organization" will be very different depending upon which basis for identification is chosen. It appears that, given the methods of inductive inquiry, it is not possible to simultaneously describe "the" organization both in terms of its material, locational organization and in terms of its temporal, behavioral organization without confusing two sets of terms which are not directly comparable.

This example should demonstrate that the presumption that the "picture" given of some organization is a description of it is fallacious. By extension, an analysis of such a description would be invalid, and not likely to predict anything at all, or likely to predict any number of things, since from false or inconsistent premises, anything is likely to follow. It is thus possible to identify two related sets of deficiencies in the methods of Inductive Empiricism when applied to complex phenomena--problems in representation of the phenomenon, and problems with the inferences which are made on the basis of such descriptions.

1. Description per se is impossible. Each observer sees, not the "thing" in itself, but the representation of the thing which constitutes his observation of it. We know this is true by the fact that it is impossible to devise any reliable way of separating our perception from the image of the "object" perceived.

Moreover, that which each observer "sees" cannot itself be described as a picture of an object, or a single event, but is rather a slice out of an ongoing "stream of action," as it were, in which are represented a whole set of observations which may or may not be consciously distinguished by the observer. This set of observations, moreover, is but a discrete subset of only a few of the possible characteristics and events that might be observed, any one of which might be considered irrelevant from other perspectives.

This argument further supports the assertion that the description that any observer--participant or otherwise--could give concerning even a simple organization is necessarily relative to the observer, in terms of the personal meanings which attend it and in terms of personal purposes and interests. Two such relative descriptions probably would not be comparable, because of the differences in what each "chooses" to "see" in the situation. The relativity in observation thus extends to the observer's intent; what "aspect" is attended, and why?

Thus there is clearly no possibility of getting either a complete or an objective description of all the characteristics of any phenomenon. The term "phenomenon," therefore, stands not for a single object or characteristic of some object, but for a set of observations, which are relative both to the perspective and the interests of some observer.

There is another problem with inductive description. The methods of describing either activities or things in terms of their attributes places the focus on the attributes and disengages it from their references--the objects or events of which they are characteristic--this in such a manner as to preclude the possibility of relating these attributes back to the phenomena from which they were abstracted, or of meaningfully recombining or correlating them in terms of their essential properties. It is exactly this disengagement of reference which requires of us a presumption of the nature of some persistent underlying regularity or substance. If I have compiled a set of categories--race, age, sex--on what basis can I unambiguously refer the values and averages on these variables back to the situation so described? To describe an organization in terms of, say, 45% under 40, 52% female and 16% black is to tell me rather less than I knew by looking at each individual in terms of those characteristics. Thus does descriptive analysis in terms of abstracted characteristics destroy the reference, and thus the meaning, of those characteristics.

Moreover, the relations between the elements of the organization, in which we are really interested, cannot be so described at all. The relation between one object and another is not a property of either object in the sense that some "primary" attribute--such as its weight or color--might be. The notion of a relation between two objects is thus a further abstraction beyond the consideration of characteristics of individuals. Inasmuch as our perceptions are constituted of discrete experiences, none of which gives an impression of relation, then such presumed arrangements and connections amongst the parts, even more than their characteristic attributes, are a further inference, and an opportunity for extending the arbitrary selectivity in our representations.

Thus our methods of inductive abstraction of qualities not only eliminate the references of such attributes; they destroy the representation of the relational arrangements altogether. This difficulty is central to the problems of inference from such descriptive bases.

2. With respect to inference, the inductive method of establishing relations among attributes of objects on the basis of inferred "causal" connections in time obscures the organizational characteristics of the phenomenon by replacing the inferred temporal-"causal" connection. Such inference derives from the perceived order of our observations in time: First was observed this, then that; on the basis of these two precepts, one infers their connection, the consequent considered to be dependent upon the antecedent, and the description thus representing a hypothetical relation between the two which is held to be representative of the whole. However, since the image or description is an abstraction out of the total observational field, the inference of connectedness may be based on a presumed relation in time, and/or a spatial relation--of distance, for example--or it may also be a connection inferred in terms of a further abstraction, such as function or intent ascribed to the objects concerned and indicating yet another sort of connection.

Thus to infer an explanation for a class of events is to refer, first, to a common set of attributes which are related both with respect to their external similarities and to their sequence of observation in time. The attributes first abstracted are considered influential factors in the identification of relations of another sort, which are now impossible to distinguish. Any organization that may have "originally" characterized a complex phenomenon--any relation between the constituent elements themselves together with their attributes, or among the elements as they stand in temporal relations with respect to one another rather than to the observer--is lost, and cannot be inferred by that observer after the fact, certainly not on the basis of their characteristic attributes. Again, it seems we learn more about the observer than about the organization.

Finally, all such inductive inferences, from specific instances to general conclusions of any sort, are invalid. No string of observations, no matter how extensive, entails any other observation, nor any general conclusion to the "whole."

Implicit in the epistemology of Inductive Empiricism is the presumption that our descriptive constructions are isomorphic--or identical in structure--to the order in the universe, which is presumed. It should be clear from the foregoing discussion that an isomorphism between a descriptive causal representation and the objects of such representation is in principle impossible of discovery, and is to that extent meaningless. Moreover, it is the position of systems theorists, in particular, that such descriptions are not even isomorphic to the set of observations from which conclusions are abstracted and generalized. In this view, the modern variants of the inductive methods of observation of similarities and differences are destructive of the very relationships--the organizational structure--which the systems analyst would consider the most important characteristic of complex phenomena.

Representation and Modelling

The first principle of a systems approach is to abandon altogether the notion of description and examine instead the idea of representation --or modelling--making explicit the relative and constructive nature of experience and observation.

Ashby, for example, suggests that we forego any ambition to know the whole system and attempt to achieve partial knowledge which is complete within itself and sufficient for one's practical purposes. A model, he says, is "seldom isomorphic with a 'real' system, but is a homomorphism of it. Some aspect of the model is related to the real system; thus . . . two systems are so related that a homomorphism of the one is isomorphic with a homomorphism of the other," which is a symmetric relation (Ashby, 1956).

A "system" can thus be considered a general model for expressing the character of any set of observations; the phenomenon is presented as a conceptual system, which is a homomorphic image of the "natural" environment, constructed symbolically out of some of its total set of characteristics by an observer, selected according to his interests and intents, to which it is isomorphic. Thus "we construct a map or model which is homomorphic to the environment and isomorphic to the process of interest" (Haralick, 1977). By so identifying both our observations and the basis for expressing their similarities, we avoid the impossible problems encountered in attempting to prove or presume an identity between our descriptions--which can now be seen as models, or representations out of the ongoing set of observations--and that "reality" which they propose to describe. Furthermore, we have good grounds for unambiguously establishing an isomorphic relation between two systems, not on the basis of their external characteristics--which may represent nothing more than shallow analogy--but on the basis of their formal expressions or representations.

By referring our observations to a model, we can assert that that in which qualities inhere is not immutable substance, but a mode of expression, which provides a basis for the precise and complete specification of some phenomenon.

Before considering "systems" as models, it is necessary to consider several different possible interpretations of what a model is. According to Willer, "A model is a conceptualization of a group of phenomena constructed by means of a rationale, where the ultimate purpose is to furnish the terms and relations, the propositions, of a formal system which, if validated, becomes theory." The model abstracts those aspects of the phenomenon which are of interest to the observer and provides for them a definition in terms of their meanings and scope of application, which set of definitions taken together determines the mechanism, or structure of their relationships (Willer, 1967).

Willer considers a theory to be a validated formal system expressing the relationships generated by the model in a one-to-one correspondence; thus the theory is isomorphic to its model, and not to the phenomena of the world as such. "That the model should be isomorphic does not mean that it should (or indeed could) be identical with its phenomena. . . . A scientific model is a model _for_ phenomena, intended to represent its structure or behavior, not a model _of_ it intended to simulate its appearance" (Willer, 1967). Thus, this conception of "model" is functionally identical to the models developed in systems engineering and analysis, as defined by Haralick and Ashby; the model stands in the same relation of isomorphy, not to the "objects" themselves, but to their representations.

There is a difference, however, in the relation of a model to a theory, indicating that there may, perhaps, be more than one type of model implied, which can be differentiated on the basis of the modelling process itself. According to Haralick, model building results from a process of abstracting or generalizing from the phenomenon. "The process of abstracting or generalizing is an adaptive translating process from the set of observed measurements to a formal language, a set of principles which describes these measurements." This set of principles contains the "irreducible" characteristics of the phenomenon, definitions of concepts involving these characteristics, and the axioms or theorems which describe the assumed relations among them (Haralick, 1977). Thus the model is a model <u>of</u> the relationships of the phenomenon.

The modelling process just described would corresond to what Willer calls an "iconic" model, which is constructed to "directly resemble a property or set of properties of a group of empirical phenomena," and which may be homomorphic to the phenomenon in terms of scale or with respect to certain included characteristics. This abstraction represents a certain danger in social science; since the mechanism--or relational structure--of the iconic model directly represents the behavior of some phenomenon, the strength of these relations is strained once the degree of abstraction becomes too great (Willer, 1967). Thus a truly abstract representation of a group of phenomena based on this modelling process would probably fail to demonstrate such relations unambiguously, if at all.

It could be argued that the reason for the greater success in adopting or identifying a set of axiomatic relations among the phenomena of physical science can be attributed to the presence of broad-based, extremely well-confirmed theories in these fields, which provide both the relations and the basis for their proper application. The social sciences, in the absence of a strong theoretical tradition, cannot now draw upon such generally accepted axioms. For this reason, Willer advocates the construction of symbolic models, in which the structure of relations is constructed from the definitions provided for the constituent concepts representing elements of the theory, which designation he declines until those relations have been confirmed by prediction and test.

It would thus appear that Willer's use of the term "theory," in this methodological context, corresponds to the term "model" as used in the physical sciences and mathematics. The disparity in terminology may perhaps be alleviated by demonstrating an isomorphic relation between model and theory in two respects: the conceptual model for a theory, and the construct model of a theory. The latter presumes a set of reliable axioms of relation; the former attempts to generate them.

The conceptual model would be a representation of a certain set of characteristic elements abstracted from a total set of observations; it stands for that phenomenon and provides a clear expression for analysis into its constituent relations. These relations are then exhibited symbolically--either mathematically or logically or by some other formal system--which provides a general expression of relation valid for any phenomenon whose characteristics fit the definitive conditions of the formal system, now a putative theory. When a particular phenomenon is represented in terms of those characteristics and relations as defined, by identifying observable characteristics of the phenomenon with the elements of the theoretical expression, or formal system, we have provided values for the terms of the theory and in so doing generated a specific model--a construct--which exhibits those relations unambiguously for test or confirmation. Thus the conceptual model, the theory, and the construct model all are isomorphic systems with respect to their relational structure, but not necessarily with respect to the particular external characteristics. A conceptual model is constructed to represent a set of observations for purposes of definition and derivation of a mechanism; a theory is a formal system, or model, consisting of a set of defined elements standing in determinate relation, considered putative until the mechanism shall have been confirmed to some degree. A construct is a specific representation of a particular instance of that phenomenon, which provides actual values on the variables or elements related in the theory. Thus a construct is a model of a theory, which it validates, at least for that case; the conceptual model is a representation of a set of observations, or a model for a theory, and is never confirmed.

As a particular representation which stands for a set of observations and which provides a basis for their comparison and combination, the concept of "system" is a model which stands in a deductive, rather than an inductive, relation to particular observations and is thus better able to represent the structure of complex phenomena than are inductive generalizations from it. Moreover, the concept of "system" properly permits the investigation of isomorphic relations among phenomena on the basis of their abstract representations, rather than on the basis of their direct observation. It is now possible to distinguish systems analysis from systems theory by drawing a distinction between formal system models and the model of the General System.

General Systems

Beginning with the admonition that "the subjects of investigation in every experimental science are systems, not objects" (Klir), a system can be considered to be a set of objects together with the relationships between them and between their attributes, or characteristics. The objects concerned are simply the parts or components of a system, and the nature of these parts is unlimited in variety (Hall & Fagen, in Buckley, 1968). On the basis of just such general definition, Bertalanffy asserts that there can be seen to exist "models, principles, and laws that apply to generalized systems or their subclasses, irrespective of their particular kind, the nature of their component elements, and the relations . . . between them" (Bertalanffy, 1968).

The definition of "system" and the principles pertaining thereto are considered to be universally applicable by systems theorists, and, in fact, form the basis for establishing isomorphic relations among phenomena in widely disparate disciplines. It may be asked of such a definition: To what does it refer? What are the "objects" designated?

James Miller identifies three "types" of system--concrete systems, abstracted systems, and conceptual systems. Concrete systems are "continuous bounded regions in physical space-time containing a nonrandom accumulation of matter or energy organized into a set of interrelated subsystems" (Miller, 1965b). The "objects" in question, or components of concrete systems, are themselves physically concrete, and refer to properties of units or relationships within the system which can be recognized by an observer; thus the parts of the concrete system are variables which can be distinguished from other nonorganized entities by their physical proximity, their similarity, and their common fate, and by the distinct or recognizable patterns which the units constitute, all of which are empirically discoverable.

Abstracted systems are "sets of formal relationships within or among concrete subsystems." The objects or components of the abstracted system are relationships which exist in the observer's mind, but which refer to concrete relations which are observed in concrete systems. Conceptual systems are formal systems whose components are logical or mathematical terms which, taken together, may be made to represent either abstracted or real systems (Miller, 1965b).

Structure, according to Miller, consists in the arrangements of subsystems and components in three-dimensional space at a given moment in time; "structure" thus refers to a static arrangement of physically contiguous units in a concrete system. Process, which includes the ongoing function of a system, is a dynamic change over time in the matter-energy or information of a concrete system. The components of a system are the specific local and distinguishable structural units; the subsystem is the totality of all the structure of a system which carries out a particular process, and which exists in one or more identifiable structural units of the system (Miller, 1965a).

Let us raise a few questions concerning the foregoing definition of system before considering the concept of isomorphism as an application. Miller's definition of system exhibits an ambiguity which is characteristic of systems theorists: the tendency is to lean strongly in the direction of a description of concrete "objects" in the manner of the inductive empiricists. He identifies the objects and attributes of a concrete system descriptively and physically, and argues against the use of abstracted systems in favor of concrete systems, for the reason that "since one can conceive of a relationship between any concrete system and another, one can conceive of many abstracted systems which do not correspond to reality" (Miller, 1965a).

His argument is unsound. Even could he successfully identify concrete referents for the components of the system, the concept of processes carried out by living systems requires the postulation of observed relationships, which suggest either that such relationships are parts of abstracted systems, or that the abstracted system is in this regard indistinguishable from the concrete system. The description of the "concrete" system appears to require the coupling of the abstracted system to make sense, for even if his assertion that relations are observed is accepted, "functions" are not. The concept of function is strictly a conceptual term, having no physical, concrete designation; processes, and thus subsystems inferred from the functional analysis of the system, appear to have no identifiable reference, particularly since so little consideration is given to the conceptual system. Do the "words" in the conceptual system refer to objects, or to roles, or both?

Miller suggests that "the concept of subsystem process is related to the concept of role. Organization theory usually emphasizes the functional requirements of the system which the subsystem fulfills, rather than the specific characteristics of the component or components that make up the subsystem . . ." (Miller, 1965a). Confusion of reference of this nature can lead to absurd consequences for the construction of systems theories, particularly in social science, where we are offered a description of an organization which "is deemed to have basic needs," which "reaches decisions, takes action, and makes adjustments." Consistent with the type of definition given by Miller, "structure" may refer both to the relationships within the system, and to the set of "needs and modes of satisfaction which characterize the given type of empirical system" (Selznick, in Emery, 1969).

The objection here raised is twofold: On the one hand, a general term, such as the "General System," can be considered not to refer at all. There are no general objects which could be observed to correspond to such terminology; at most, all that could be designated by the term "general system" would be the words in a language or written on a page. The term is logically equivalent to "the average man," or "the meaning of 'a' word"; we understand what is meant in the definition, but can identify no possible objects other than the word standing for itself, which such terms could designate; therefore, it would not be out of the question to consider the "general" system meaningless. Moreover, if the relative nature of the perception of all phenomena is taken into account, so that we recognize that the "system" is defined from the point of view of the observer, it could further be argued that instead of a general system, there are to be designated as many specific systems as could possibly be expressed by all possible observers. In this case, the concept would be meaningful, and useful, but certainly not identical.

A second question can be raised regarding the structure, or mechanism, of the general system. That the "system" consists of elements in mutual relation does not tell us anything about the form those relations actually take in a particular case, nor indicate how to describe them. Members of a system don't become constituents by means of their inherent qualities, but by their positional value in the system; what is significant is not their attributes but their arrangements, which is the definitive characteristic of any particular system (Angyal, in Emery, 1969). A definition of structure in general terms implies that, as with the designation of a general term, there is no such meaningful "object," the concrete reference notwithstanding. On the other hand, should such references be produced, the general definition of structure would lead one to believe that there is a single structure or mechanism throughout all phenomena represented as systems. Even Buckley (who vacillates on the concrete versus conceptual nature of "systems") asserts that there is no specific structure that is normal for every society, and that the specification of conditions essential to the maintenance of society tells us nothing useful concerning the particular structures they will or have developed to meet these conditions (Buckley, 1967).

Therefore, "general system" theory must be defined for particular phenomena, but it would be a mistake to do so by translating general system properties into particular system properties, because it is not the characteristics of the elements, but the arrangements of elements, which is crucial to an explanation of that system. The arrangement of parts is not some general property of systems, but must be established for each system. "Organization" does not imply <u>an</u> organization, but rather a large number of possible arrangements; for a specific system, this is an empirical, not a definitional, question.

Bertalanffy's response to Hempel's criticism that general system theory has no predictive force is relevant to this argument. He asserts that in every hypothetico-deductive system it is necessary to introduce special conditions according to experience in order to apply it to concrete phenomena, and that general system theory is no exception (Bertalanffy, 1972). It can be objected that, if it is necessary to develop special "laws" for particular systems such that general system theory must be coupled with existing theories, the system conception may be merely gratuitous.

The discussion of mechanism with regard to General System Theory is directly involved in Bertalanffy's conception of isomorphism.

> There are principles which apply to the entities called "systems" in general, whatever the nature of their component elements and the relations or forces between them. The fact that all sciences mentioned are concerned with "systems" leads to a formal correspondence or isomorphy in their general principles. (Bertalanffy, 1958)

Bertalanffy initially defines isomorphism as a structural identity between two systems as represented mathematically--i.e., formally. This isomorphy is based on the appearance of similar system laws in the various sciences, a similarity which holds between the principles governing otherwise dissimilar phenomena. However, he then gives two different reasons for that similarity. On the one hand, such an identity of mathematical expressions available to describe phenomena, and thus such isomorphism, would be a consequence of the application of the same equation to two different phenomena. On the other hand, he further asserts that "these laws and schemes would be of little help if the world (i.e., the totality of observable events) was not such that they could be applied to it" (Bertalanffy, 1968). Thus the structure of reality is such as to permit the application of our conceptual constructions, somewhat reminiscent of Mill.

But later on, he again equivocates, affirming that all scientific expressions are merely abstractions and idealizations expressing only certain aspects of reality and are thus not identical with it. But that every science relies upon such schematized pictures of realities he considers only possible on the condition that order "really" exist in reality itself. Finally, he escapes this circularity by asserting that the parallelism of general conceptions in different fields is a consequence of the fact that these are concerned with systems and that certain general principles can apply to any system, irrespective of its nature. Thus the isomorphism found in different fields is based on the existence of general system principles--his conception of the General System is thus the ground for uniting the structure of the "world" and the structure of our models of it (Bertalanffy, 1968).

General System Theory appears, especially in Miller's definitions, to be an empirical generalization from a diverse set of phenomena, related in terms of formal mathematical models--particularly exponential laws relating rates of change of one variable with those of another. Such mathematical expressions are constructed to represent the structure of specific phenomena; if such expressions are applied across the board to other phenomena whose internal arrangements have not been empirically established in advance, even if the expression "works," it cannot be used as the basis for asserting a fundamental structural isomorphy between the two "natural" systems, especially if such isomorphism is established to justify the use of the expression.

The circularity of the notion of isomorphism in General System Theory indicates an essentially "inductivist" ambivalence with respect to the phenomenon, in which an isomorphism between the concept and its "object" is implied as a "structural-functional relationship between a living prototype and a model" (deGreene, in Beishon, 1972). It should be clear that to establish or confirm an isomorphic relation between a "living prototype and a model" is in principle impossible, inasmuch as it presumes the very order in the natural world that it seeks to uncover and neglects the relative nature of observation. To the extent that General System Theory defines general principles characterizing concrete systems, it can be seen to be an empirical generalization, which, since it claims to represent all, rather than any, phenomenon, is false.

Finally, to the extent that General System Theory is general, it may be said to refer to no particular case, and thus would be--not false --but meaningless. Being "general," it is not a theory at all, having neither specifically defined elements, nor a determinate mechanism. One must conclude that General System Theory, therefore, represents a normative orientation to the phenomenon, at best a rationale for the construction of a substantive theory.

Organization

The concept of isomorphism entails a comparison of two systems with respect to their organizational structures, and thus presumes a clear identification of such structure in each case. According to Bertalanffy, "The unifying principle is that we find organization at all levels" (Bertalanffy, 1968). The concept of the General System was intended to be a model of a very particular sort; it was conceived as representing explicitly the organization of any system-phenomenon--the set of relations or mechanism itself, independent of the characteristics of the particular elements so related. That which unifies the various system "laws" and which underlies the notion of the General System itself, is the ubiquity of organization per se, the arrangements and relations and processes which are a part of all phenomena.

We have seen that this objective failed for the General System theorists in its inductive generality, which implied either the construction of a model of an essentially unbounded set of systems, or the presumption of some physical entity and/or some theory from which the specific characteristics and relations are derived. In the absence of a theory from which the formal identities or isomorphic representations of two "concrete" systems can be identified, such isomorphism as postulated by Miller and Bertalanffy reduces to "mere" analogy. Thus it is important to distinguish analogy from isomorphy for purposes of analysis.

On the basis of a set-theoretic identification of system elements, Bunge identifies representation as a subrelation of simulation; if x represents y then x simulates y. Given a universal set, a member x is substantially analogous to another member y if the two share several objective properties--if they are equal in some respects. A member x is conditionally analogous to member y if there is a correspondence between the parts or properties of the two. If both conditions hold, the analogy is considered a homology (Bunge, 1970).

Analogy can suggest equivalence, but does not establish it. In order to determine a relation of identity between two systems, it is necessary to establish a correspondence between those two systems--represented as sets--which maps each element of set x onto some element of y in such a way as to preserve the relations and operations in x. If it is possible to do so, we may consider the set x to be homomorphic with the set y. The set x is to be considered isomorphic with set y only if there is also a homomorphism from y into x as well--in other words, perfect analogy. Therefore, although identity between the two sets establishes their equality, and the equality entails equivalence, which further implies similarity, the converse implications do not hold. Thus the specific criticism leveled against formal identities established among "concrete" rather than conceptual systems is that the confusion of analogy with equivalence "has given rise to the classical yet mistaken belief that analogy is the source of induction, in turn wrongly supposed to be the method of science" (Bunge, 1970).

Bunge concludes that in order to establish or identify an isomorphic relation between two phenomena it is necessary to produce a relational system--a conceptual system, or theory--which represents the structure common to the two concrete systems. Given a theory, we may then regard the specific systems to be related as two among many physical models--what we have termed construct models--of the formal system.

Moreover, as realizations of the formal system, each system is now formally analogous to the other--with respect to that theory. Thus he concludes: "Two systems (concrete or conceptual) A and B are isomorphic with respect to a third system, the relational system F, just in case A and B are models of F" (Bunge, 1970). In order to establish an isomorphism between two systems, it will be necessary to produce a theory which subsumes them both.

In order to establish an isomorphic relation between any two systems strictly on the basis of their structural configurations and irrespective of the particular characteristics of their constituent elements, it will be necessary to produce a general theory--not of systems--but of organization, which is the only phenomenon entailed unqualifiedly in all systems, conceptual and concrete. Ashby's definition of "organization" reflects the relative nature of identification and comparison of two systems with respect to a third, which is exhibited in Bunge's above treatment of analogy. "The hard core of the concept is, in my opinion, that of 'conditionality.' As soon as the relation between two entities A and B becomes conditional on C's value or state then a necessary component of 'organization' is present" (Ashby, 1962).

A formal theory of organization would be more plausible as a relational system than a general theory of systems, in that the concept of organization need imply no particular structures, nor substantial systems at all, but may be restricted without ambiguity to the patterned nature of elements in any system, regardless of their specific descriptions. The concept of organization can then be taken to refer to both a process of relating and structuring separate, individual components to form a whole, as well as the product of such a process--the organized "entity" made up of those constituents, which, once related, exhibit a structure of relations which not only characterizes the whole organization, but adds to the individual properties of the constituent elements positional characteristics relative to each other and to the whole.

It is on this basis that, according to Buckley,

> the newer systems view is building on the insight that the key to substantive differences in systems lies in the way they are organized in the particular mechanisms and dynamics of the interrelations among the parts, and with the environment. (Buckley, 1967)

Even in the purely material realm, the differences between organic or inorganic, living or dead, are due, not to the intrinsic natures of the composite materials of each, but to the different ways in which the materials are organized (Buckley, 1968).

This viewpoint is consistent with a major tradition in sociology, represented by Mead, Park, Cooley, and Marx, among others, which has long emphasized the processual nature of social phenomena, recognizing "structure" as a construct, some"thing" abstracted from one's observations at a moment in time. The notion of process has focused on the actions and interactions of components in an ongoing system, from which perspective "structures" can be seen to arise, dissolve, change, and to persist. But "a" structure, or "an" organization is certainly not an essential description of any phenomenon, least of all society, which can be seen as a "complex . . . interplay of widely varying degrees and intensities of association and dissociation" (Buckley, 1967).

However, since we have established that the mechanism, or structure is, for each specific system,an empirical question, to assert that a system is organized is not to say what that organization is. We may believe that a system is organized, but we can neither presume nor directly perceive its structure for two reasons: First, organization exists in the way we think, in the syntax of our language(s) as well as in the socially determined and verified meanings which we attribute to our experiences. In addition, even our "mere" observations take on a certain temporal structure in our minds because of the fact that our perceptions occur in contiguous space and time frames, requiring that we infer, rather than perceive, any connectedness amongst the phenomena.

Hume suggested early on that one not place too much faith in inferential reasoning--or our believe in causal connectedness among our impressions. The basis for such notions as cause and effect, and order (or organization) in the universe, is to be found in our habitual awareness of repetitious experiences--or constant conjunctions. "All inferences from experience . . . are effects of custom, not of reasoning" (Hume, 1966). In this view, the connectedness and organization in which we believe refers not to the external world, but to the way we think about it--in other words, to the redundancy in our experience as we are aware of it. Were there no awareness, and/or no repetition of our experiences, there would correspondingly exist no notion of order.

The implication is that the organization that Miller, for example, thinks he observes among concrete objects, or the organization that Mill or Pearson infer on the basis of similarities and differences in perception refers to associations among ideas in the minds of people, beyond which one might well be simply agnostic. While there certainly may be order in the universe, what we are aware of is order among our ideas, which is associative rather than merely reflective, as can be demonstrated in the phenomena of hallucinations, dreams, and memory. Thus the stumbling block to a theory of organization, per se, would be the organization among the ideas in the mind of the observer, as manifest in his expressions about his observations. We know such expressions are relative to the particular experience of that observer; is there any way in which the expression may be related to the objects designated by that expression?

In constructing models we are establishing a homomorphic mapping of the external environment present in our observations into a language of expression, within which isomorphic relations can be established between two such representations by comparison of their structures as exhibited in that language. Thus all our discussions are of conceptual systems; but it is possible to recognize and express the organization entailed in such conceptual systems in various ways, according to their designation. The word "system" may refer to an ordered collection of objects in the world, or words or statements in a language, or to a collection of sensory impressions existing either in the mind or in the brain of a human observer.

That which could characterize both the presumed organization in "natural" phenomena and the organization in thought and thinking could only be a formal conceptual system in which the terms and relations represent, and thus designate, both the objects of observation and the objects of our language. On the basis of such a formal system one can establish determinant relations between the language of expression--the conceptual system--and the observations expressed in it--the concrete system--by defining a mapping, or set of rules for translating those observations into formal representations which can then be transformed within the language. Thus, although we may never be able to directly attribute any given structure or organization to the objects of our perceptions, it is possible to define a conventional order to the process of representing our disparate observations as systems.

We may then consider any organizational configuration--regardless of its reference--as a code standing for the structure of that phenomenon to which it refers, a code which can be expressed formally and independently of its coded elements. Given a sufficiently flexible language of expression--set theory, for example--it should be possible to translate, or map, the order represented by one system into that of another (either on a comparative basis by determining whether there is an isomorphy, or on an "effective" basis, by which that isomorphy could be imposed in translation).

To the extent that this process of representation is feasible, all phenomena become commensurable, with respect to the language of expression. We have thus a sound basis for comparing and combining even the most dissimilar phenomena by representing their organizational configurations as formal codes, translatable by rule-guided procedures. On this view, if two complex systems are definable, then they can be made commensurable, regardless of the nature of their constituents, by so translating them. It should thus be possible to represent both the spatial and temporal configuration of some phenomenon in a single expression, from which we may infer without obscuring the relations we presume (or intend) to obtain within that system.

Finally, this commensurability provides the basis for establishing the isomorphy of two systems with respect to the formal relational structure in which they are represented. If the formal conceptual system is fully specified, any phenomena which it designates may be considered to be isomorphic with respect to their structures.

Perhaps the most dramatic instance of the application of such isomorphies can be seen in the recent attempts to identify a "mechanism by which experience can be experienced" (Pribram, 1969). Numerous physiological studies have been undertaken in recent years to attempt to identify the coding of environmental information in the neural structure of the brain. McCullough and Pitts have developed a model of the "anatomy" of the brain as a "formal neural network" which is isomorphic with the general representation of information--or signal elements of whatever designation--as expressed in the language of symbolic logic. This isomorphism is established by recording the behavior of complicated neuronal nets in the notation of the symbolic logic of propositions (McCullough & Pitts, in Buckley, 1968). The importance of this representation, according to von Neumann, is that the functioning of the brain in a physiological sense can be identified in a formal neural network, and thus a relationship can be established between a physiological process and a formal linguistic expression (von Neumann, in Buckley, 1968). The focus in establishing a relation between thinking and thought then centers directly on questions of configurations; as Pribram argues, we turn our attention to the substitution of one configuration for another by operations of the nervous system. Research into the physiology of memory so far indicates that coding operations take place in the nervous system continuously, whereby physical "signals" are sensed by receptors and transformed into nerve impulses (Pribram, 1971).

The key to establishing an isomorphic mapping between physiology and language is the notion of redundancy, or repetitive configuration of signals. This concept of redundancy is central to the expression of organization as information; the fundamental principles of Information Theory equate the amount of information which can be conferred by a configuration of conditionally constrained elements in reducing uncertainty of the observer with the organization of the system itself. The notion of redundancy is also reflected in Hume's contention that the connectedness we "perceive" in the world reflects our awareness of the redundancy in our experiences. In both cases, the configuration exists in the experiences and expressions of the perceiver; but the redundancy or repetition may implicitly designate a source of information external to the observer, in terms of which configurations may be inferred in the environment.

Pribram identifies two major classes or modes of organization into which redundant experiences are coded: in the biological sense, encoding refers to the process which distributes information in the brain; decoding refers to the process by which that information is "read out," or utilized by the organism (Pribram, 1969). On the basis of a model such as that of the formal neural network, it should be possible to define a mapping from an expression of information as a configuration of signal elements designating physical referents in the environment of the organism, to an identification of the physiological processes operating within the brain itself, to a further transformation in the expressions of the subject of that information, in memory and in other "psychological reports." Thus is proposed a neural model of the environment which is produced in the nervous system, and which relates the objects of our observations to the expression of our observations in physically designative terms.

Given the expression of our observations as conceptual models, it is possible to relate our observations and our theories, and it is possible to relate two separate theories on the basis of a formal system, which may be constructed either by: (a) producing a formal language into which the terms and relations of both can be translated and thus made comparable, and/or (b) producing a theory--a fully specified and determinatively related conceptual system, such as Information Theory--which would subsume both the specific models under consideration. Logical systems represent the objects of systems as elements of sets, or terms and propositions in formal logical languages; Information systems represent the objects of systems in configurations of signal elements which may be either physically or conceptually designative. The organization in information systems is expressed indirectly, by statistical inference to the amount of information conveyable by the constraints entailed in the configuration of elements. Either method provides a mode of expression which permits us to represent without distortion any set of observations as a "code," and to transform that code in an orderly fashion without loss of information--in other words, while preserving its distinctive configuration. The discussion that follows will focus on methodological issues specific to a number of logical system models, which are based on set-theoretic, rather than information-theoretic representations of systems.

The systems analytic methods of representation and transformation depend on the construction of formal logical languages by the early logical empiricists, particularly Russell and Wittgenstein. Since the focus is explicitly directed to systems conceived as formal linguistic expressions, the constituent elements or "object" of systems are elements of the language. Calculation within the system consists of successive tautological transformations of expressions in the formal language, all of which can be seen to be isomorphic with respect to the language of expression.

Logical Analysis

The analysis of a formal logical structure is an analysis of the expressions in a language. Historically, logical analysis developed out of an early attempt to identify the structure of the natural world with the structure of a precise language in which observations were expressed. Responding to the tenets of empiricist epistemology, the logical atomists were agreed that the basic elements of science were not material things, but facts--logical, rather than physical "atoms." Thus Wisdom claims "An account of the world in terms of things, an account of the world in terms of facts, and an account of the world in terms of events is just an account of the world in three languages" (Wisdom, 1931). Inasmuch as the basic elements of experience are thus facts, and not objects, the proper route to understanding of the objects of experience lay in the logical analysis of our expressions of those facts. Thus, propositions of existential import were held to have an exclusively empirical reference, but that empirical reference could be conclusively demonstrated by logical analysis.

It was assumed that there was a structure to the world of nature, and that, moreover, one could know it, not by a rigorous examination of experiences and observations, but in terms of the examination of the expressions in the language of science itself. The presumption was that the structure of scientific laws would reflect the underlying structure of thought, expressed in its clearest form by the mathematical logic, which logic was held to reflect the structure of the world of nature.

Russell's Thesis of Extensionality is primarily an account of the correspondence of language with the sensory experiences out of which it derives. According to this thesis, language is truth-functional; every statement made is either a logically simple statement or a truth-function of logically simple statements in conjunction. Language, defined in these terms, is a collection of simple, atomic propositions, each of which can be empirically confirmed. If we presume, with Wittgenstein, that the world as observed consists of an "indefinitely large number of atomic fact" to which the true atomic propositions in a language will correspond, then if these atomic propositions are conceived as being logically independent, by extension, such atomic facts must, therefore, be conceived as being metaphysically independent (Wittgenstein, 1922).

In this view the structure of the world is considered to be isomorphic with the structure of a language such as that developed by Russell in the Principia. The form of a well-constructed symbolism would be a faithful indication of the structure of the world; thus inquiry into the structure of the world must begin with analysis of the statements representing our observations, or atomic facts, by replacing all descriptive phrases and logical constructions and otherwise "incomplete symbols" with the names of possible objects of observation or reference.

Although the logical empiricists laid the groundwork for the precise representation and logical manipulation of systems as formal languages, their methods ran aground on the strict requirements placed on definition by the Verificationist theory of meaning, originally posited by Wittgenstein. According to this theory, if the atomic propositions of the truth-functional language were to accurately stand for, and thus provide a basis for inference to, the "objects" of the natural world, then in order for a term or expression in the language to be meaningful it must be possible to specify the simple observation of sense datum that corresponds with, and thus unambiguously defines the term.

A host of problems arose to render this attempt at definition impossible and it was eventually abandoned or modified by its original proponents. It was not so possible to verify the meaning of a statement, because the number of empirical instances required to be produced is at all times infinite; thus, although it was possible to reduce complex propositions to sets of simple propositions, it was not possible to break down all our complex observations to simple sense data.

Two "methods" were advanced in an attempt to define the elements of the formal, logical language by identifying them with observations, each of which can be seen reflected in the methods of modern systems analysis.

The method advocated by Carnap was to split the language of science into two parts, the Observation language and the Theoretical language, which were then to be connected by a chain or partial, or conditional, definitions called reduction sentences which functioned as a set of correspondence rules linking the two "languages" or frameworks of discourse.

According to Carnap, Wittgenstein's restriction of empirical knowledge to that which is actually observed reflects an exclusive identification of the language of science with what he calls the "material mode," which results in a certain absolutism and neglect of the fact that the thesis of verifiability is relative to the chosen language system. The mistake is in formulating the epistemology of such statements as an assertion instead of as a suggestion concerning the form of the language, which should instead be considered a matter of choice or convention (Carnap, in Feigl & Brodbeck, 1953).

Carnap advocated instead the use of the formal, rather than material, idiom, both of which can be translated directly into a propositional language. Thus, out of the natural languages it is possible to construct a "technical" language, which is validated externally by its usage and pragmatic value; internally, propositions in such a language or framework are evaluated according to the rules, or syntax, of that language system. This syntax can decide on any question regarding the elements of that language system except: (a) the question of whether an element of the language "exists" independent of, or external to, that language, and (b) whether the language framework as a whole exists, or is "correct" in some sense (Carnap, in Linsky, 1952). It is possible to equate this exclusion with our earlier criticism of the attempt to identify an isomorphic relation between the objects of the world and their models.

Within the framework of a given theoretical language or observation language, the meanings of terms can be specified according to the rules of that framework--observation terms, for example, could eventually be specified by ostention--and the question of designation or reference for those terms is referred to the framework in which the term is defined. A proposition is thus defined within the language in terms of the syntax of that language and the designation of the term; in representing the formal and material idioms in the propositional system, the differentiation between the two thus falls upon the designation of the terms or propositions (Carnap, in Linsky, 1952).

There are some striking similarities between this position and various set-theoretic methods of system-definition which draw upon the direct representation of observations in a formal language, which constitutes a framework bounding the system. This framework, or universe of discourse, makes it possible to precisely distinguish among the levels of organization--or abstraction--of the system-phenomenon, and to precisely distinguish the system from its environment. As an example of this approach, Toda and Shuford have developed a set-theoretic method of constructing and decomposing systems, which is not only consistent with Carnap's conception of formal languages constructed as frameworks for analysis and synthesis, but which attempts to specify as well the position of the observer in the "construction" of such systems.

Their approach focuses on the consideration of structure as residing, not in the "system itself" but in the rules by which a complex phenomenon is composed out of its elementary relations, which rules are a function of the observation of the system. If the system can be considered to be a set of objects together with the relationships between them, the question immediately arises as to which objects, and which relations are identified out of all the possible characteristics that could be abstracted. Hall and Fagen are explicit about the relative nature of system definition; they take the position that the relationships to be considered in the context of a given set of objects depend entirely on the problem at hand, the decision as to which relationships are important residing ultimately with the person who defines the problem (Hall & Fagen, in Buckley, 1968). They do not indicate, however, how two such idiosyncratic systems can be compared.

The intent in Toda and Shuford's method is to provide a precise, but relative, specification of the system, by focusing on the process of decomposition of the observer's idea of the system. To decompose a system is to break it down into a set of subsystems or parts, which can themselves be further resolved into subsystems progressively identified by the observer until the point is reached at which further decomposition results only in an identity; the transformation results in the set itself. The D-set--or set of subsystems obtained by decomposition on a system, is thus a function of both the particular deomposition applied as well as of the particular system to which that decomposition is applied.

Thus is analysis relative on two counts: first, the structure is related to the set of parts considered, which will differ according to the particular decomposition; thus for any given system, there are as many D-sets or structures as there are ways of breaking it down into subsystems. Second, each decomposition on a system generates another set of systems--previously subsystems--each of which can be further decomposed. Every decomposition is unique, and every transformation is made on a single system, resulting in a single D-set. In this way the authors specify the notion of the hierarchy of systems. The levels of organization in systems can now be identified with the successive decompositions on a single system, which are relative not only to the major system but also to the particular succession of decompositions identified by the observer (Toda & Shuford, 1965).

The system identified by a particular decomposition constitutes a determinant framework for analysis. James Miller's procedural rule that all discussions of systems should begin by identifying the level of reference can be seen to restrict, or to contextually bind, comparison and combination of particular elements to the universe of discourse specified by the identification of the "system" at whatever level of decomposition. Thus the "system" is the set of elements under immediate consideration; organized structures above that level of reference in generality are suprasystems, and those below are subsystems (Miller, 1956b).

Given this relative definition of "system" and following Hall and Fagen's definition, the universe of all "things of interest" can be subdivided rather arbitrarily into two sets, system and environment. The system consists of a set of related elements identified by a particular decomposition; the environment consists of everything not entailed by the framework of the system under consideration (Hall & Fagen in Buckley, 1968).

The black box concept, to be treated in greater detail later on, is a type of operational "primitive," the undecomposed conceptual element of any system. As we have seen, each decomposition identifies a set of different elementary parts, which are taken to be its basic units, and are thus not further analyzed or decomposed into constituent subsystem elements. Thus, for example, a biological decomposition may be resolved into cells as the elementary units; another decomposition could conceivably further resolve the cells as systems into chemical subsystem constituents. However, to identify the cell as the basic unit is to treat it as a black box with respect to that conceptualization; its behavior can be recorded but finer decomposition into elements is not, or cannot, be made.

Toda and Shuford assume that the number of subsystems generated by decomposition is finite; that is, at some point the decomposition will culminate in an identity set within a finite number of steps. In order for any decomposition--material or conceptual--to be meaningful, "there must be some means to obtain information about the state of each subsystem generated by the decomposition" (Toda & Shuford).

The state of any system is defined as the total set of characteristics of that system at some time; the atomic state function is the ability of the observer to specify a set of alternate states for each atomic system generated as the terminus of a series of decompositions on a system. The "System" is formally identified as a Formal Structure Set, which consists of two components: the System itself to be so analyzed, and the Observer, who is identified jointly with the atomic state function--the alternate states which can be defined for each atomic system--and with the total set of decomposition families--all the possible decompositions which generate the same set of terminal, or atomic, systems from a given major system (Toda & Shuford).

The relations between elements of a system are defined in terms of the states of such systems; the relation between two systems consists in the totality of constraints on the possible combinations of states of the two systems. The constraints can either be identified quantitatively, in which case each constraint represents the conditional probability of two systems jointly being in the same state, or in relatively identifiable states; or the relations can be identified categorically. Since quantitative relations can only be given between classes of states in two systems and not directly between the individual states of those systems, categorical relations are in fact the basis for determining quantitative probabilities (Toda & Shuford, 1965). However the relations between states of a system are determined, the structure of the system can thus be identified as the totality of relations holding among subsystems of a given D-set.

The structure of a system is a product of the relations between the states of the subsystems of a given system, at each level of decomposition, and for each major system identified. The structure changes if the relations among the subsystems change or if a subsystem is removed and not replaced. Structure is also a function of the particular decomposition which the observer <u>can</u> bring to bear on a system and the extent of the decomposition of which the observer is capable--how far can a major system be broken down before a terminal set of subsystems is reached which can be decomposed no further? It is also dependent on the identification of alternative states of which the observer is capable, including the possibility of assigning conditional probabilities to these states in conjunction. Thus we can see in this rather involved discussion of structure, that the concept is not even meaningful without specifying which of the many possible D-sets is selected, both with respect to the dimension selected for decomposition, and with respect to the level, or particular system, which is being decomposed. We can, therefore, conceive of the structure of a system in a number of ways, because there is always more than one way of decomposing a system (Toda & Shuford, 1965).

Finally, whether a system is closed or open can also be seen to be a function of the agency of the observer rather than an independent attribute of the system in itself. The system is considered closed if the Formal Structure Set to which it belongs is complete, which means that the observer can assign a probability distribution over the domain of alternative atomic states that can be identified for the terminal systems. As with the concept of structure, the closedness or openness of a system is meaningful only with respect to the observer--it is not that a system _is_ closed, but that it can be considered closed in the context of its observation. If the observer can assign a probability distribution over the states of the subsystems making up a system--if the structure involved in the relation between two subsystems with respect to a larger system of which they are constituents can be identified--then the system is "well-determined" and can be considered closed within the Formal Structure set to which it belongs. If the observer cannot do so--and the authors agree that it is finally impossible to assign nonmarginal conditional probabilities strictly on the basis of the subsystems identified without also being able to specify the probabilities for the external systems to which any open system is subject--then it is possible to make an open system well-determined, or closed, by discovering a closed supersystem of the open system.

The ability to close a system depends upon the composition of a system out of a set of constituent subsystems identified by some decomposition. Toda and Shuford identify a C-set as a set of systems which can potentially be a D-set, and further identify two types of composition of a system out of a set of systems. A system may be materially composed by a combination of a set of systems with relations they did not have before composition; consistent with the earlier discussion of decomposition, it can be seen that there are as many ways of materially composing a system as there are alternative structures that can be put into the C-set. However, the authors reject material composition in favor of "conceptual composition" by which they mean to

> simply regard the whole C-set as a single system. Then the supersystem conceptually composed from a C-set is unique, and the relations held by the systems of the C-set (which now constitute a D-set of the supersystem) are identical to those held before the composition.

Thus as long as we restrict usage to conceptual composition and decomposition "it is natural to assume that there is a one-to-one correspondence between the whole set of decomposition and the whole set of compositions" (Toda & Shuford, 1965). On this basis the authors postulate a composition which is exactly the reverse of the original decomposition applied to the system.

Now we can see how the observer can close a system by a process of composition; by adding to the system under consideration a number of other systems which have been identified in terms of their atomic systems and states, a composition can be undertaken on the "new" C-set to form a new, possibly complete, and unique supersystem, which, taken together with the "observer," constitutes a new Formal Structure set. "A closed system is well-determined. An open system can be made well-determined if a closed supersystem of the open system is discovered" (Toda & Shuford). Closing a system depends on being able to compose a unique supersystem by adding to the original system other systems of the same modality, thus creating a new (super)system with the original as a subsystem.

Toda and Shuford's set-theoretic "system" is a method for defining a system for purposes of analysis by precisely specifying its constituents within a frame of reference provided by a logical language. As such it has several advantages.

This method not only recognizes, but attempts to specify, a process by which the system is explicitly identified with the particular classificatory scheme of a given observer. To the extent that it is possible to identify the relative element of system definition with the decomposition and specification of alternate states that the observer brings to bear on a situation or a group of data, it is possible to compare two different systems unambiguously, and/or to extend any system definition clearly by extending the decomposition, or by adding to the alternate states identified.

In constructing a formal system by this method, we have established, in Bunge's sense, a conceptual relational system within which specific systems can be identified and compared. By making the classification system an explicit contribution of an observer, the relational system can specify the relative component of definition, such that, if one can make the observer's a priori schema explicit, it is possible to account for it in analysis and thus to render it eliminable. In practice, if the schema--or organization in the observer's thinking about the phenomenon--is made explicit, the system identified by two observers can be compared not only with respect to its elements and relations, but to the structure imposed by observation as well.

Such specification of the structure of the system makes the concepts of system boundary and system level both meaningful and specifiable for any system and for any observer. This method is thus valuable for preventing category mistakes in identifying the boundaries of systems, which often remain vague because the level of decomposition by observers is not consistent or in all cases clear, and which makes comparison of systems or the extension of a system model impossible.

However, although such explicit identification is advantageous, given the manner in which Toda and Shuford define the Formal Structure set, a particular system is only in principle so definable, because the number of decomposition families or atomic state functions that _an_ observer _can_ identify is unlimited, and is certainly not specifiable in advance.

The problems of system specification in terms of its observation are reminiscent of the difficulties of positivists such as Carnap in attempting to rationally reconstruct a phenomenon from its "atomic facts." Carnap did not "solve" the problem of identifying the relation between the theoretical and observational languages. In order to refer to the objects of the observation language, a proposition must ultimately be decomposable into atomic sentences which are full expressions of atomic predicates, which are either primitives--direct "objects" of sense perception--or are introduced by atomic chains which designate individual constants as space-time points (Carnap, in Feigl & Brodbeck, 1953). The question centers on the formation of such atomic sentences by the specified operations; what is the relation between the form of the atomic sentence and its content is a question that may be asked of Toda and Shuford's treatment as well.

In Carnap's method of specifying the empirical meanings of theoretical elements by constructing sets of reduction sentences, it is the confirmation of a sentence which is reducible to a class of propositions which themselves are taken to be true unconditionally--without further reference. Nonetheless, the rules of correspondence between the two languages could only give a partial interpretation of the theoretical framework in observable terms. There still remained concepts which could not be defined by means of nonquantitative observation terms, among them general terms and descriptions and dispositional concepts which referred, not to observable properties of "objects," but to relations between concepts, or to "behavior" (Carnap, in Baumrin, 1963).

Thus, for essentially the same reasons, neither Carnap's nor Toda and Shuford's system is ultimately fully specifiable in terms of empirical observables. For the latter, the advantage of being able to include the position of the observer in the specification of the system is overshadowed by the impossibility of specifying all of the possible decompositions and all of the possible alternative state descriptions and conditional probabilities that he can apply to a given phenomenon. In order to specify such a set of possibilities, it would be necessary to include an immense collection of potential classifications; if we are trying to specify in advance everything an observer could say about a system we could be required to specify the entire language--potentially the entire culture--that the observer could bring to bear on a problem. Moreover, this process of definition also requires the observer to decompose the initial system into its atomic subsystems, which are necessarily specifiable in terms of observable state characteristics. This is a reiteration of the positivist's requirement that terms--subsystems of the conceptual system--be identifiable in terms of sense data, but the method at hand extends this specification to all the possible characteristics.

Moreover, as with the problems of general system specification, any schema or set of decomposition families which can be produced does not specify that which actually is produced for a given case. It tells us too much for a given system; we may be interested in complete knowledge of a system, but not of any system. Focusing on the alternatives which an observer can identify not only entails specifying the universe in advance, but still does not identify the structure in the particular system at hand. The confusion of possibility (the decompositions and states that can be identified) and actuality (the particular constituents of a given system) is reflected in Toda and Shuford's treatment of composition, which does have the merit of reflecting quite clearly the problems with inductive reasoning.

The process of composition which the authors advocate comes to no more than stipulating that such composition is equivalent to the initial decomposition effected on the system. This is an unwarranted assumption in terms of the admittedly numerous possible structures which could be identified for any given system. Composition and decomposition are not symmetrical. A decomposition actually generates a single set of constituent subsystems on an identified system; composition refers to a number of possible combinations that could be established among a set of systems, but one has no reason to suppose that those possibilities reduce to the single decomposition actually performed. Unless one knows a rule, or procedure, for relating systems to form a supersystem, it is not certain which possibilities are actual, given a decomposition on a system.

Thus, although their method of system closure is attractive, it is essentially unworkable. Being able to identify a system as closed--as everywhere well-defined--is necessary for purposes of analysis. Toda and Shuford's definition of closure not as a characteristic of any material system, but as a function of the explicitly identified observational context for a system, enables one to view system closure as a characteristic of conceptual systems and a function of the observer. A system may always be considered closed, only with respect to a broader system which the observer has in mind.

However, to so close a system requires us to specify a particular classification schema and not merely a potential set, in order for the boundaries to be meaningful or useful. The application of Toda and Shuford's process of composition is inadequate to the purposes of closing a system, inasmuch as the addition of a number of new systems to the original to form a C-set constitutes a different set than that generated by the decomposition. Thus, even if we were willing to accept the symmetry of composition and decomposition, the symmetry is dependent upon the identity of the two sets of systems, which is not satisfied in closing an open system by adding "systems" to the C-set.

Essentially, we are dealing with the problem of induction--the possibility of inference from a set of parts to a whole. A given set of "elements" does not entail any particular structure of relations or any future consequences materially or logically, and the selection of constituents is external to the process of composition anyway.

In the absence of a decomposition on a previously identified system, the possibilities of combination are limitless because the possible constituents to the C-set are limitless. Because the constituent elements of a system are defined not only with respect to their individual characteristics (states) but also with respect to their relative positional characteristics (relations), those constituent elements themselves are only partially defined. Thus, neither Carnap's nor Toda and Shuford's methods result in a fully defined conceptual system. Definition of the system is incomplete without specification of the organization of relations characterizing it; however, it is not possible to discover those sets of relations by analysis unless the system is well-defined. Thus a system cannot be fully defined by a composition of its elements, and the conceptual system therefore cannot be closed.

For this reason it is not meaningful to attempt to specify functions and therefore roles in general systems--such as "society"--in which specific boundaries or "ends" cannot be given. Society has neither clear-cut boundaries which distinguish it from other systems in its environment, nor does it have an end--or final state--from which such functions could be determined.

Finally, Toda and Shuford's stipulation that composition is merely conceptual renders the entire system empirically undefinable in much the same sense as isolating the observation language and the theoretical language--or conceptual system from a "concrete" system.

Although translating our observations into a formal language, such as a logical or set-theoretic representation, enables one to represent a system coherently relative to its environment and to a given frame of reference or observation, we are left with a number of problems crucial to the identification of actual, rather than potential, relations among the constituent elements of a specific system. We are not able to fully specify the characteristics of "atomic" states empirically, which in the preceding method is necessary to effect closure of a system for analysis. If it is going to be possible to infer a mechanism, or principle of relation, underlying the spatial-temporal organization of a system, such inference will depend both on the conceptual closure and the empirical reference of the system to be so analyzed. Finally, if it is going to be possible to specify the contribution of the observer--which we have seen would be a great advantage in increasing the precision of our models--it will be necessary to specify either the actual composition, or a rule for its construction, since the conceptual system the observer can employ is richer than the actual system employed.

The problem of induction could be summarized as follows: Given a system, we may be able to infer a set of constituent parts for it which will be exhaustive; but given a set of parts it will not be possible to "rationally reconstruct" a particular structure of relations from the parts alone. We have seen that organization is a property of the whole and not of any of its subparts; therefore, it cannot be constituted of any number of parts, but requires--as Bunge suggests--a relational system with respect to which the subparts are related. This observation has been variously put by Russell and others; essentially, it describes the relation between particular and general expressions such that "One more level is needed to constitute an organization than is contained in its parts and subparts" (Feibelman & Friend, in Emery, 1969). Consequently, the conclusion of an inductive explanation--in this case the system itself--will refer to at least one thing not entailed in the premises, and is thus not truth-functionally entailed by them.

Does this mean that something is added in composing or inferring an organized system? To answer this question, it may be helpful to analyze the meaning of two characteristic systems concepts--emergence and equifinality--to ascertain what, if anything, is added.

Induction, Equifinality and Emergence. In the phenomenon of "emergence," the characteristics of the whole complex system as compared with those of the elements, appear as "new," as somehow not a consequence of the composition of the parts alone (Bertalanffy, 1968). This fact is expressed in the adage, "the whole is greater than the sum of its parts," and can be recognized in the disappearance of some parts at a lower level of organization. For example, a role can be considered a component of a social system, but not of an individual, psychological system. Is the role added when the individual joins a social group? More to the point, can the role be attributed as a characteristic property of an individual along with color of hair, or height? If we say no, that the concept is only meaningful when applied to a social group, and if it is not a property of individuals, we may ask, then, "to what does it refer?" Is the social group, beyond the individuals in it, an epiphenomenon--meaning that its designation of something other than the set of individuals is of some nonobservable and thus "less real" component, undefinable and thus not eliminable from any explanation, which should be reduced to psychological explanation of the behavior of individuals? It seems this is giving up too much, but the alternative explanation that something is indeed added--some collective conscience, for example--is presuming too much.

Perhaps a more fruitful explanation is that the qualitative change (the emergence of roles, or nations) comes in the change in perspective or representation of the system at each level of organization. The newness is a consequence of a shift in the frame of reference (in this case, from the individual to the group) and therefore in the base unit of definition of the system at that level. Thus the social unit of "role" is lost when the frame of reference is shifted to the individual. We could conclude that emergence is then not a "property" of systems, but rather the discontinuity is in our perceptions and expressions.

Anticipating the problem of equifinality, one may argue that what is added in moving from the personal to the social level of organization is relationships. Herein lies the rub. The difficulty of attempting to represent the structure of a system arises from the fact that the relations between events and elements in a system cannot be "defined," or stipulated, because the "relations" are not observed, but inferred, by an observer. Specification of relations in system definition is therefore contingent upon the context (or the environment of the system), on the classification the observer employs in defining the system (in agreement with Toda and Shuford), and on the time frame in which the observation takes place.

Given our earlier arguments, it transpires that both the initial and final states of a system are arbitrary; they are strictly a factor of the temporal circumstances of observation, which must be stipulated in order to close the conceptual system. We have seen that a system can be considered closed only if a context can be fully specified for it; this is possible only in the case of a conceptual system, which makes explicit a restricted, but complete, frame of reference with respect to which constituent terms are defined. The question of materiality is irrelevant. Closure is a consequence of observation; all phenomena are potentially interrelated--i.e., open--relative to some context, or dimension of observation.

Thus it is not the case that some systems are open and some closed. Rather, in the absence of a theory which presents a formal context for a system, it would be necessary to specify the entire environment, a residual and therefore intrinsically unspecifiable concept which includes all other system-phenomena, future as well as present, negative as well as positive. Let us now consider the concept of equifinality from this standpoint.

Whereas in a closed system, the initial conditions determine the final state, equifinality in open systems means that the same final state may be reached in a variety of ways and from a variety of initial conditions (Bertalanffy, 1968). Different sequences can lead to identical configurations, which configuration--or system state--does not entail any one process in the sense that one could trace a single sequence back to a set of elementary conditions through a specific sequence. That two identical sequences could result in different ends, or that two different sequences could result in the same consequences, obviously is a problem for scientific prediction, but is understandable in the light of the number of alternate compositions that could be taken from the same set of initial conditions.

Thus it seems that equifinality, like emergence, is a property of the framework of observation--which is always involved, either implicitly or explicitly, in a prediction. The conditions of observation--time intervals, choice of initial and final states, classification schema--are all equal parts of the system along with the state characteristics. Therefore, a change on any of these conceptual variables will result in different consequences, which will be unpredicted if they are not made explicit and controlled for. Equifinality of our observations can then be seen as a consequence of the fallacy of equating decomposition and composition, and of neglecting the conceptual ordering of the observer.

Finally, then, in answer to the question of whether the whole (system) is actually greater than the sum of its parts, in the sense that something is added in composition, we can agree with Ashby that it is not. According to Ashby, we define a set of parts as organized when some connection exists between them, which connection implies some constraint, or correlation, between what happens at A and what happens at B. "Thus the presence of 'organization' between variables is equivalent to the existence of a constraint in the product-space of the possibilities" (Ashby, 1962). He goes on to assert that while biologists especially have tended to think of organization as something extra, as some thing added to the elementary variables, the current position, which is based on the logic of communication, is rather to consider organization in terms of restriction or constraint.

Thus the behavior of the whole system is not richer than that of its parts; no epiphenomena materially emerge. However, the explanation of a whole system is certainly richer than that of its parts, for not only must the parts be defined, but the relations among them. Since there are, in effect a number of systems that could be constructed from the parts, given a specified interval of time, the conceptual system is richer than any actual system corresponding to it.

Uncertainty of prediction, therefore, resides in the multiplicity of contexts that can be specified in defining a system. Ultimately, this uncertainty is not eliminable, because it is impossible to capture the total referential context--all the possible decompositions--simultaneously. Each decomposition identifies a different system, and composition is not single-valued. The lack of reliable prediction is thus understandable, but should, therefore, be remediable.

Thus it appears that the problems of system definition and of valid inference and prediction are not separable, but must be tackled jointly. If it is not possible to infer the mechanism--or structure of relations--without fully specifying the system in advance, can we not instead merely postulate a mechanism and then confirm that presumption by prediction to some--observable--outcome? This method has a history as well.

The definition of structure--or connections among the phenomena--by postulating a set of relations on the basis of some predicted outcomes can ultimately be attributed to the hypothetico-deductive model advocated by Hempel and others, which was essentially an alternative to Carnap's method of connecting the (deductive) conceptual system and the objects of observation.

The Hypothetico-Deductive Method

According to Hempel, "There are no generally applicable rules of induction" by which hypotheses or theories can be mechanically derived or inferred from empirical data. Rather, scientific theories are invented in order to account for observed facts, and are not derived from them. One invents hypotheses for the relations among objects or attributes of objects and then subjects these "guesses" to the observational confirmation of events. Thus does he claim that scientific inquiry is "inductive in the wider sense," according to which any rules of induction would have to be considered "canons of validation rather than of discovery" (Hempel, 1966).

Again, the implication is that structure in the world of nature can be approached indirectly, not through statistical measurement, but by careful observation and experiment in terms of test implications that can be derived from general hypotheses. Although specific elements of the theoretical system may not be considered fully definable in observable terms, if the world is regular in the fashion that we think it is, then some testable implication would follow from a statement of regularity among our observations that will indicate to us the adequacy of our hypothesized relations.

According to this method, the phenomenon to be explained is subsumed under a set of general laws and specific conditions, which must meet several criteria. First, the sentence describing the phenomenon to be explained--the explanandum--must be a logical consequence of the general laws and specific conditions cited in the explanans. Explanation and prediction are, therefore, logically equivalent; if the event has occurred, the explanans explains it; if it has not occurred, the explanans predicts it, and the confirmation of such prediction refers back to the general statement of relation. In addition, the explanans must fulfill two conditions: It must contain general laws which are capable of supporting any conditional statement, and the explanans must have empirical content--it must be capable of test by observation, at least in principle (Hempel & Oppenheim, 1969).

The most attractive feature of the hypothetico-deductive method is the symmetry of explanation and prediction. This is the basis for Reichenbach's concept of "postdiction," which suggests the possibility of a logic of discovery as well as of confirmation. The logical equivalence of explanation and prediction can be expanded to suggest a way of inferring a mechanism for a system-phenomenon from an analysis of the explanandum, E, as an outcome of the system. Although Hempel denies a meaningful status to the logic of discovery, as do most neopositivists, this possibility does arise with the cybernetic analysis of systems in terms of their outcomes, and the validity of such analysis would derive from this equivalence.

However, there are also many shortcomings in the hypothetico-deductive method as it stands, deriving mainly from the use of the two-valued, if-then logical form of inference which results in a series of conditional statements of the same type of causal-temporal inference as postulated by the inductive empiricists. If, instead of inferring to the causal relations among objects, we postulate causal relations (still on the basis of perceived temporal contiguity) and then confirm the adequacy of the hypothesis by predicting to some later empirical circumstance, for the same reasons as other, basically inductive, techniques, such a method may be presumed to inundate any particular configuration that is the object of study.

Moreover, it has been noticed that such explanations tend to become elliptical; they never give a complete or final answer to the "why" question. The explanation for an event E is given in terms of certain principles L_1 and conditions C_1; but one can then question these conditions, which are themselves explainable given principles L_{11} and C_{11}, and so forth in an infinite regress. It can be seen that this difficulty parallels that involved in attempting to achieve closure of definition of logical categories in empirical terms. Official "answers" often have this character when organizational decisions are being questioned; dozens of contributing "causes" can be cited, none of them necessary, all of them sufficient--the investigator can take his pick of attractive "reasons."

Such explanations also reflect the equifinality of system predictions, which is seen in the irreversibility of a causal sequence; one event clearly followed another in time, but the particular sequences preceding the event in question are many. Thus an explanation could begin at any point and end at the event to be explained, but not the reverse. Without full definition of terms, then, even if given the event, it is not entirely possible to reason backward to the "cause," first because there may be no end point in the sequence that does not take in the whole world, and second, because the number of possible sequences leading up to the event in question must also be taken into account.

Finally, this method cannot handle the particular case in which definition and inference are most strongly interconnected--that of the status of dispositional concepts, which do assert a relation between two events, and are, therefore, not fully specifiable in sigular, primitive, observational terms. Dispositional terms seem to resemble covert hypotheses more than the assignment of attributes. For example, a juvenile delinquent is defined with reference to attributes which express, not a characteristic of the person, but of something that the person did, and that, moreover, under rather special conditions of test--being arrested. The basis of dispositional definition is its conditionality; the term is variable according to its context. A phenomenon so defined, according to the methods under consideration, ceases to be defined when the definitive conditions are not satisfied--when it is not actually being tested. This is the problem of the paradoxes of material and strict implication. In the first case, given an expression $(P \supset Q)$, if P is false (is not satisfied) then logically anything follows; in the second, given the expression $(-P \supset -Q)$, if P is true, anything follows. Thus the meaning of a dispositional concept of relation cannot be fully defined according to any of the methods of logical empiricism, even under the liberalized thesis of the empiricist criterion of meaning. Since dispositional terms can be seen to designate relations in, or behavior of, systems, if their meaning could be specified unambiguously it would constitute a real contribution to the analysis of systems.

This objection is related to the general problem of definition engendered by the requirement that the explanans have empirical content, established at least by prediction to observable events. On the positive side, it is not necessary to define directly all terms by reference to observations; it is sufficient that observation is possible, and can be provided indirectly by prediction and test to observable events. However, this method does not fully specify--any more than did the postulation of correspondence rules--the relation between deductive "laws" and empirical statements.

The problem with definition in the hypothetico-deductive method has been attributed to the fact that the general laws are logically independent of the characteristic conditions or descriptions of the objects to be so related; there is always implied some external constraint on the objects beyond their descriptive conditions--and thus the explanations given in terms of such general, substantial laws, are never "complete." On the other hand, theoretical laws are not independent of the defining conditions of the objects to which they apply. Moreover, an empirical generalization, "theoretically reconstrued," becomes a deductive consequence of the conditional descriptions of its constituents and thus loses its "law-like" universality (Cole, 1977). There is an apparent paradox entailed in the postulation of deductive scientific theories and laws: To the extent that the law is universal, and thus deductively entails all specific instances, it cannot be empirically specified or defined; to the extent that the law has the appearance of a generalization, and thus does have empirical content--such as a well-confirmed dispositional definition--it ceases to be universal, and thus does not support certain conditional statements, particularly counterfactual conditionals. As previously noted, such definition also obtains only under actual conditions of test, when the conditions are fulfilled--when the conditions of test are not satisfied, anything at all can follow from the "law" in the sense that anything can follow from false premises.

Because the descriptions of the antecedent and consequent events in covering law explanations are logically independent, a logical analysis of the expressions will fail to account for the application of the law to particular cases; there is always a nonlogical component in the definition of empirical objects in the terms of the theory which cannot be formally accounted for, which creates a certain pressure to elevate hypothetical relations to the status of definitions (Cole, 1977). This is the reason why the program of the logical empiricists to identify the structure of the world of nature in the logical construction and transformation of its precise linguistic expression failed on the problem of empirical "definition" of logical terms. As a consequence, the logical empiricists, as the inductive empiricists before them, were forced to presuppose the existence of the "objects" and "relations" in nature that they were attempting to identify. Thus, none of the empiricists was able to shake off a presumptive ontology which was subsequently reflected in unsolvable definition problems.

Cole offers an alternative solution to this problem which is consistent with the methods of analytic representation. He suggests that one explains a fact from a "model" such that if the model applies, then the fact obtains. If the prediction advanced does not hold, the theory is not falsified or abandoned; rather, it is not considered to be applicable in that case. This proposal constitutes what Cole terms a "substantial cause theory" of explanation, according to which the laws in a hypothetico-deductive explanation can be considered to constitute conditions which are satisfied in correct identification of particular instances of the general expression, which itself may be recharacterized without abandoning the law, even if the prediction fails (Cole, 1977).

That such characterization is possible depends on the development of formal logic beyond the theory of truth-functions, so that an expression can take into account the variable nature of its predication. According to the theory of truth-functions, statements are seen to be compounded of other statements, and thus a compound statement is a truth-function of its parts, which can be so reduced only to the particular atomic statements, which are not themselves truth-functional compounds, and thus are essentially undefined. The contribution of quantification theory (credited to Russell) lies in the extension of the analysis to the atomic--and therefore unanalyzed--statements of the theory of truth-functions. Truth-functional analysis of expressions is preserved, but the atomic statements can be further analyzed into logical subjects and predicates. A given statement can be said to designate three types of logical subjects or expressions: Individual names, which are singular designative expressions; universal selectives, "for all x,...x..."; and particular selectives, "for some x,...x... ." Statements in ordinary language can be translated into the quantificational structure, which renders the statement of relation weaker, but specifiable. The statement "All A's are B's" can be translated into its equivalent formulation, "For all x, if x is an A, then x is a B," which can be specified in terms of particular observations. The statement "All A's are B's" is untestable in principle (and thus undefinable) because it is impossible to enumerate all the A's or B's; but its quantificational reformulation is testable and holds for each specific case which fits the conditions cited.

This modern, quantificational logic makes possible the expression of complex relations without having to presume the existence or nonexistence of the items involved in the relation; rather the existence of that item itself can be taken as a variable to be established for a particular case. Thus a scientific law can be an analytic statement, expressed as formally deductive, rather than general, and yet still be specifiable in empirical terms.

In Cole's substantial cause theory of explanation, the soundness of the explanation depends only upon the identification of instances to which it may properly be applied; the contingent element is not the definitional assignment of properties to designated kinds of "objects," but the identification of specific cases to which the general statement may properly be applied. In this way a theory can generate a number of specific statements concerning natural objects, which can then be tested. However, contrary to Popper, the theory is never verifiable or falsifiable in experience; it is either applicable or it is not (Cole, 1977).

Thus, although an explanation in this view is a deductive argument from a universal proposition which does not refer to any actual entities--it is, in other words, a fully conceptual system--the explanation is related to the "material" world via an instantial premise which constitutes a claim that the universal statement is instantiated by some natural object which fulfills certain conditions for identification under the law. If the deductive consequence of such a premise is false, the object in question is not instanced, but the theory or law is not thus falsified (Cole, 1977).

The concept of instantiation appears to resolve the insurmountable problems involved in identifying empirical cases with the formal, analytic statement of the model or theory; and does so in such a fashion that the theory or model can be considered to be a valid representation even when its constituent conditions are not fulfilled, thus circumventing the problems of material and strict implication. A system can thus be formally--and hence deductively--represented but still be empirically relevant; it may be analyzed logically <u>and</u> designated empirically.

Such an explanation can be "closed" to the infinite regress involved in elliptical descriptions which follow from sequences of "causal" dispositional definitions. The instantial premises restrict the explanation to the conditions cited, and close it to wider descriptive contexts involving conditions which are not part of the "law." On all other factors, save those identified in the model, the explanation is silent; thus the failure of a prediction due to the influence of such "external" factors does not falsify the relation, nor does it render the explanation only probabilistic. Rather, application of the explanation is restricted to cases which fulfill the conditions; other conditions may then suggest extension of the model to include such factors, a feature not gained by merely disconfirming the theoretical statement (Cole, 1977).

Finally, instantiation can be seen to constitute an equivalence relation among the objects which fulfill the conditions of the model; each system-phenomenon which can be so designated according to the conditions is isomorphic--with respect to the formal system model--to every other phenomenon so instantiated. The logical relations are preserved, but the theory is rendered empirically meaningful. In this sense, system isomorphism can be seen to be a function of the reference or designation of a conceptual system. Thus the instantial premise constitutes sufficient grounds for prediction and for establishing isomorphy among possibly dissimilar phenomena. Moreover, the extension of a formal model also provides a basis for establishing, as well as identifying, isomorphic relations among phenomena.

Systems Analysis

We can now begin to see the utility of the concept of system isomorphism, or equivalence of structure, as a basis for systemic--as opposed to systematic--analyses of systems. Isomorphy can be operationally defined as follows: Two systems are isomorphic with respect to a third--a certain conceptualization--just in case an experiment performed on each, started from the same initial conditions, or inputs, and operated on the same time scale, results in an identical outcome.

A specified process, or statement of relation, should be replicable. If one represents two systems as "experiments," beginning them at the same time from the same initial conditions, and follows the same sequence of steps, identical system configurations should follow at the "end" of the experiment. Conversely, given the initial conditions and end conditions, it should also be possible to generate an equivalent set of sequences that could obtain between the two points of observation. This set of equivalent "systems" corresponds to Ashby's product space of possibilities, to the total set of compositions that could be generated by an observer for a given set of system components.

The Product Space of Possibilities. How do we go about generating this set of equivalent systems? According to Cole, a substantial cause explanation can be transformed logically into a set of equivalent expressions, each of which can be instantiated. As each transformation of a logical tautology results in a logically equivalent statement, each "system" designated by any of the equivalent variants is isomorphic to every other system so designated.

Now the sort of conditional relations which constitute the general laws of the hypothetico-deductive model are of the form "If P then Q," which is stated in what he calls the "active voice," meaning that the relation is considered to imply all those circumstances in which if P is true than Q will also be true. Logical analysis of theoretical expressions, however, is "voice-neutral" rather than active, in which the statement is interpreted as a static "present-tense" relation, which is taken to mean that it is not possible that P shall be true and Q false, which neither implies change nor agency. A quantificational reinterpretation of the statement ($P \supset Q$) is the statement "$-(P \cdot -Q)$" (Cole, 1977). There are, therefore, two other possibilities beyond the case in which P is true and Q is also true, which equally fulfill the statement of relation, all of which can be instantiated. The statement ($P \supset Q$)

can be fulfilled by the case in which P and Q are both true, the case in which P is false but Q is true, and the case in which both P and Q are false; the only statement which does not satisfy the relation is that in which P is true while Q is false. Thus strict implication asserts an impossibility "-(P • -Q), but does not assert the co-possibilities, and is therefore "modally ambiguous."

The method by which Cole derives the set of possibilities is by translating the conventional truth tables of logic into possibility matrices with a model interpretation, by abstracting those rows from the truth table in which the composition holds. The matrix thus obtained is interpreted as representing all the co-possibilities indicated by that expression; and, by excluding those rows in which the compound expression is falsified, the matrix is considered to exhaust those co-possibilities (Cole, 1977). We thus obtain an entailment relation which is stronger than strict implication, and which avoids its ambiguity.

By translating a "causal factor analysis" into a voice-neutral logical explanation, it is possible to characterize the components involved in the explanation in terms of dispositional properties, which must be fulfilled in order to satisfy the statement of relation. Thus a voice-neutral expression characterizes possible instantiable cases in terms of dispositional properties which are entailed in the explanation, and which are, moreover, considered to be actual and not merely theoretical concepts (Cole, 1977). This method thereby allows us to avoid the problems encountered in attempting to connect separate theoretical and observational languages.

Dispositional properties are those which can be sometimes predicated of an object and sometimes not. If, as we have seen, both P and Q can be considered to be contingent, either can be present both as a negative and as a positive condition; thus P and -P would represent two alternative states of some object. Dispositional characteristics, then, would be the alternate states which a system characteristic could exhibit. On the other hand, nondispositional properties are predicated of the object "permanently"; it is not possible to predicate such characteristics of an object negatively. Such characteristics correspond to the components of the system. The example which Cole gives is the representation of some object, z, as a flashlight consisting of a switch, y, and a bulb, x, which he symbolizes as follows:

$$(\nabla Fzyx \ \& \ - \ \nabla \overline{F}zyx) \ \& \ (\nabla Sy \ \& \ - \ \nabla \overline{S}y) \ \& \ (\nabla Bx \ \& \ - \ \nabla \overline{B}x)$$

The switch and the bulb are permanent predicates; they cannot be negatively predicated, or else the object is not considered to be a flashlight. However, both the switch and the bulb can be considered to be either off or on, which conditions represent dispositional predicates, symbolized as follows:

$$(\nabla Oy \ \& \ \nabla \overline{O}y) \ \& \ (\nabla Lx \ \& \ \nabla \overline{L}x)$$

All of these conditions may be systematically expressed in a possibility matrix as follows:

$$\begin{bmatrix} Fzyx & Sy & Bx & Oy & Lx \\ Fzyx & Sy & Bx & \overline{O}y & Lx \\ Fzyx & Sy & Bx & Oy & \overline{L}x \\ Fzyx & Sy & Bx & \overline{O}y & \overline{L}x \end{bmatrix}$$

The matrix enables us to represent large quantities of information in a form which permits translation into algorithms, which are precise rules or procedures for representing or transforming systems. The matrix strongly entails the relations among its components, such that they satisfy the requirements of strict implication. In addition, we can construct from this possibility matrix a characterization of the object which provides a rule for its instantiation in a substantial cause explanation, as follows:

$$F'zyx = \text{def } (F'zyx\ Sy\ Bx)\ \&\ \begin{bmatrix} Oy & Lx \\ \bar{O}y & Lx \\ \bar{O}y & \bar{L}x \end{bmatrix}$$

S and B represent the components, or permanent predicates, of the system; the matrix represents the disposition of "switch illuminibility" (Cole, 1977).

Cole's representation of logical expressions as matrices is quite compatible with Ashby's derivation of the set of possibilities. The operations of set theory can be made commensurable with those of matrix algebra by presenting a matrix as a tabular representation; thus the following representations are all equivalent translations:

$$\begin{array}{l} \begin{bmatrix} Px & Qy \\ 0 & 0 \\ 1 & 1 \end{bmatrix} \begin{array}{c} Px\ Qy \\ 1 \\ 1 \end{array} \\ \begin{array}{ccc} 0 & 1 & 0 \\ 1 & 0 & 0 \end{array} \end{array} = \begin{bmatrix} \bar{P}x & \bar{Q}y \\ Px & Qy \end{bmatrix} = \begin{bmatrix} Px & Qy \\ 0 & 0 \\ 1 & 1 \end{bmatrix} = [Px \equiv Qy] = [f]$$

(where, according to Russell, "f" is any formula ≡ to Px ≡ Qy) (Cole, 1977).

This last translation resembles Ashby's representation of a mapping as any correspondence or rule which, given any element in a set, E, designates one and only one element in a given set, F. "If the mapping μ, operating on e of E, gives f in F, we write $\mu(e) = f$" (Ashby, 1964). The representation thereby names, for each element in the domain E of the mapping, its transform f in the range, F. Such transformations may be further variously represented, with the object of simplifying a difficult representation of some complex system by translating it into a more obvious, homomorphic representation of the system.

The concept of mapping can be extended to provide a method for composing or characterizing a possible complex system from the characterization of its constituent parts, by a process of matrix multiplication, or by a composition of two sets to form a product set. In Cole's method, if two matrices are commensurable--which they are if one or both can be extended so they may be represented in the same domain--then the product of the two is a matrix, of which the rows are common to both, as follows:

$$[p] \cdot [p \quad q] = [p] \cdot \begin{bmatrix} pq \\ \bar{p}q \\ \overline{pq} \end{bmatrix} = \begin{bmatrix} pq \\ p\bar{q} \end{bmatrix} \cdot \begin{bmatrix} pq \\ \bar{p}q \\ \overline{pq} \end{bmatrix} = [pq]$$

In Ashby's set-theoretical method, the product set, E • F, is composed of the sets, E and F, by taking all possible pairs of elements, the first from E and the second from F, each pair of which becomes an element <e,f> of the set, E • F. A relation then is defined as a subset of a product set. If "x," in the relation, R {x,y} is fixed at a single value or element, then the relation becomes a property of y. Thus quantifying--in Cole's terminology, instantiating--the relation either universally or existentially classifies the possible values of y according to whether they make the compound expression true or false (Ashby, 1964). We can thus see the compatibility of the two methods for deriving the product space of possibilities. A mapping can finally be defined as a binary relation, or function, between two sets which is single-valued and everywhere well-defined (Ashby, 1964).

What can we do with this sort of composition? In the first place, as pointed out by Cole, an explanation can be both closed and deepened by reformulating its "active" causal premises as dispositional conditions which indicate the instantiation of particular cases, and which thus meaningfully connect the conceptual system with the objects of observation. "Deepening of an explanation which depends upon dispositional characteristics is required when curiosity is aroused as to the ground of some component's disposition" (Cole, 1977). Given the methods outlined above, an explanation can be deepened by a series of increasingly more fundamental characterizations by a progressive matrix multiplication. What this means is that descriptions can be deepened or expanded by producing successive matrices which account for the possibilities represented by each of the components in a relation.

This process provides a method for representing the levels of abstraction--of organization--which can be identified for a given phenomenon, which can be characterized on the surface by a relation or set of relations, each of which can further be resolved into its constituent elements and dispositional relations, and so forth. As we have seen, a great deal is to be gained in clarity by this approach. Statements of relation among conditions are restricted to the "level" of analysis, to the particular dispositional conditions to be instantiated, thus avoiding the category mistakes so common in general theories. It is also possible to demonstrate how it is that two representations may be isomorphic at one level, and not at another; the isomorphy between the two is bounded by the characterization of the specific elements to be so related in terms of their dispositional properties. And yet a clear procedure is indicated for "deepening" or extending the application of the model in either direction; a relation then can be specified between levels of abstraction or organization as well as between two separate representations. And, finally, the relation can be tested--or instantiated --at any level of specificity, which is extremely helpful when dealing with complex phenomena.

However, we are no closer to specifying a particular organization or mechanism for a system, and it is still necessary to explain how, given the logical operations which can be legitimately performed on a system, it is possible to infer a particular mechanism from the product space of possibilities as identified.

Recalling that the current conceptualization of organization is one of contingency or conditionality, then the concept of structure or organization in a system refers to the restriction of possibilities of composition of a system from a given set of elements. The structure of relations which characterizes a given system indicates some constraint on the possible combinations of characteristics--states--of two or more (sub)-systems with respect to the larger system of which they are constituents. To assert a process or principle, to say that a system phenomenon is organized in time, is to define a repetitive or conditional relation among events in time.

Thus, conditionality for Ashby means that there is first some product space, consisting of a set of possibilities and representing the uncertainty of the observer; within that set, some subset indicates the actualities, the "real world" of what is. The constraint involved reflects a relation between the observer and the "thing," or the object being observed (Ashby, 1962). Thus in order to infer a mechanism it will be necessary not only to define the product space by specifying a set of equivalent compositions, but it will also be necessary to specify some selective principle for identifying one set of relations from among the possibilities.

The Cybernetic Conception of Purpose. In order to do that, it is necessary to first close the system conceptually. The process of decomposition is closed by taking as its "end" the characterization of the system at some moment. An aspect which is thus necessary to the process of composition and synthesis is to stipulate an end, to inform the selection and organization of system elements. It has been suggested that the ends of a system--as well as the relative position of the observer--can be specified according to a cybernetic, or mechanical, conception of purpose. The cybernetic definition of purpose is essentially an explication of the notion of alternative means to some specified end. If any occurrence, a, b, c, or d is sufficient to produce an event, E, then each occurrence can be seen to be a potential means to that event--conceived as an end. Such potential means then constitute an equivalence class with respect to E. Now, "The actions of (a) machine may be assessed with respect to their being means toward some end. Insofar as these actions are thus describable, they may be said to be purposive." (Moore and Lewis, in Buckley, 1968, p. 250) This method thus conceives of the system-phenomenon, the event E at some moment in time, as a black box, analysable in terms of its inputs and outputs, which indicate the state characteristics of the system at two separate points in time. According to Haralick, the engineering definition of a system is thus construed as a "structured device which produces an output related to its past and present inputs according to some... rule. The rule is called the structure of the system." (Haralick, 1977, p. 8)

Assuming the elements of a system have certain well-defined functional characteristics, they can be looked upon as black boxes--automata--which exhibit the following attributes: The system possesses a finite number of states, such that the characteristic operation of the automaton consists of a description of how it changes its state in interaction with the outside world. (von Neumann, in Buckley, 1968, p. 105)

According to Ashby, a "machine", E, can be described as a system whose internal state and the state of its surroundings define uniquely the next state it will go to. The system is closed with respect to its initial conditions by specifying the environment as a set of inputs, I. The object, or system, is a set, S, and the various elements of the set are the states, which constitute any well-defined property or characteristic of the system. Thus, the "machine with input", the finite automaton, can be defined by a mapping, f, of the product set of inputs and initial internal states (I x S) into the set of subsequent internal states, S. (Ashby, in Buckley, 1967, p. 261)

Every machine can exhibit many identifiable states. In a determinant machine, setting the conditions of the system and the state it is in at some moment will determine the next state it takes on. The state transitions correspond to a transformation on a set of operands, which are identified with the states, as elements of the system as a set. Each state that the machine moves to represents a transformation on the previous state as an operand. (Ashby, 1956, p. 27) Thus, "Given a sufficiently defined set M of states m, the set shows state-determined behavior if (and only if) its succeeding state m' is a isngle-valued and everywhere defined function of its present state", such that m' = (m). The bheavior of the system is operationalized as a mapping, $\mathfrak{g}$, of the system into itself; mapping a set into itself exhibits a sequence of values which occur when any element, e, is operated on repeatedly by $\mathfrak{g}$. This set of values generates a trajectory of behavior for that system. (Ashby, 1964, p. 86)

Thus, the organization of a system conceived as an abstract machine, is represented by a mapping function, $\mathfrak{g}$, of the product set (I x S) into S. If the mapping changes, then the organization changes. In other words, the possible organizations of the system correspond with the set of possible mappings. Thus, "organization" and "mapping" are two ways of looking at the same thing--the organization being noticed by the observer of the actual system, and the mapping being recorded by the person who represents the behavior in mathematical of other symbols. (Ashby, 1962, p. 262-3)

We can conclude thus far that <u>if</u> we can identify such a function, $\mathfrak{g}$, it will then be possible to predict the state of a system at some future time, and thus to infer its outcomes at that time. The ability to precisely specify our observations for purposes of prediction is greatly enhanced in this analytic view; however, we encounter recurring difficulties in specifying that mapping or mechanism by which such calculations can be made. We may begin to suspect that the mapping, $\mathfrak{g}$, corresponds to the decomposition families available to an observer, and nothing more. Ashby considers the form of the mapping function, $\mathfrak{T}$, to correspond to "natural forces" operating in real time on the system; $\mathfrak{T}$ then represents the "laws of nature so far as they are showing in the set M". (Ashby, 1964, p. 93) Unfortunately, Ashby is begging the scientific question.

Since the mapping, $\mathfrak{T}$, represents nothing less than the laws of nature, we may ask by what method that mapping can be identified. According to Ashby, the mapping derives from an empirical study of a system from which is derived a record of what happens at what times. This record is considered to specify a mapping from a domain of time-values into the set of possible states. This empirical record generates a protocol, which is a formal, sequential investigation of a system conducted by performing an experiment on its inputs, which experiment consists in a sequence of times and in the states of the parts of the system identified as inputs and outputs. (Ashby, 1956, p. 88-90)

According to Ashby, "It will now be appreciated that the concept of a "machine", as developed from the inspection of a protocol, comes from recognizing that the sequence in the protocol shows a particular form of constraints." The observer can "take advantage of this fact" by re-coding the whole protocol into a form containing only the statement of the transformation, and the statement of the actual input. (Ashby, in Buckley, 1968, p. 132) We thus recognize the redundancy in an observational sequence or protocol as a constraint; the mapping function, $\mathfrak{T}$, expresses that constraint as a representation of the mechanism or organization presumed to exist in the "real" system.

If it is possible to so describe a system precisely, we can then investigate larger systems which could be built up from these elements. (von Neumann, in Buckley, 1968, p. 98) This corresponds to Ashby's composition of two (or more) machines by adding a mapping function to the descriptions of the constituent machines, by which to specify one's input values as a function of the other's state. The concept of feedback corresponds to the reciprocal joining of two systems; conversely, any machine so composed can be analysed into parts. (Ashby, 1964, p. 94)

One must conclude that this method of identifying the mapping, upon which depends the whole identification of structure within a system, and specification of a particular composition among systems, is hopelessly inductive. It does not, in fact, specify clearly or eliminably the perspective of any observer, and thus does not, for that reason, actually select out of the product space any specific system, as a configuration of states as processes in time. Related our observations in time does not define an invariant relation or law governing the system; it identifies a highly specific relation of a particular observer with a particular set of possible temporal sequences, and provides no way of adjudicating among the various protocols that could be identified by a number of observers, even those operating on the same time schedule.

The point may perhaps be made clearer by an examination of Simon's treatment of the concept of purpose as defined by three terms: the purpose or goal, the character of the artefact (the system under investigation) and the environment in which the artefact "performs". (Simon, 1969, p. 6) According to this position, the complexity of any behaving system--a man, an ant, a hospital--may be attributed to the complexity of the environment to which the system adapts. The relation between the "inner environment" of the artefact, or goal-oriented system, and the "outer environment" is defined by the goals; thus we "...can often predict behavior from knowledge of the system's goals and its outer environment, with only minimal assumptions about the inner environment." (Simon, 1969, p. 8) Simon identifies this relation as a correlation between a desired state of affairs and an existing state of affairs. If we know the desired state of affairs--the goal or purpose of the system--and the existing state of affairs--the initial conditions--a means-ends analysis will reflect a process description of the path leading to the desired goal. (Simon, 1969, p. 112) Unfortunately, there are weaknesses in this position:

1) One must first presume the goals of the "system". If we are really interested in the "desired state of affairs" of some individual, we are not discussing a purely abstract, cybernetic, concept of purpose, but an explicitly intentional and personal concept of purpose. Moreover, since the method calls for the observer to close the system by specifying the purpose as an output, the purpose identified is not that of the "system" nor of its actors but of the observer--a traditional problem in the social sciences.

2) In this method it is necessary to provide an identification of the existing state of affaris in the initial conditions for the system. Unless we mean by "initial conditions" some origenic theory of the system, the existing state of affairs is an arbitrary consequence of the time at which the observer begins looking at the system and the categories

of observation which he employes in recording his protocol. This is highly selective, and therefore relative, specification of the "environment" (a task which is ultimately impossible in any case). More to the point, these protocols, as expressions of the "rule" or organizational structure of the system, will most likely not be isomorphic unless a restriction is placed upon the conditions of observation beyond that specified by Ashby and Simon.

3) Finally, neither a means-ends analysis nor a protocol generated by empirical observation of a system will generate the path leading to a desired goal, nor even an objective outcome simply specified as the system at some later time. Rather, such analyses demonstrably generate several paths leading to the outcome, and neither method provides a clue to a discrimination among them--which will render the "laws of nature" somewhat ambiguous.

Analysis by Design

We can conclude from the foregoing discussion that, consistent with the arguments of the logical empiricists, it is in principle impossible to verify our models, or representations of "reality". By extension, the mechanism by which we account for the organization in the system has itself the logical status of conjecture, and is contingent upon the perceptions of the observer--his identification of goals and initial system states as well as the categories of the D-set which he chooses to represent that sequence of states. There are several practical implications which follow from this Systems Analytic position, which position amounts to a kind of "working epistemology":

Since the mechanism, $\mathcal{J}$, represents an inference or invention on the part of observers to the supposed system, we find an emphasis on testing and experimentation consistent with Hempel's hypothetico-deductive model of scientific investigation and with Popper's argument that we can falsify with validity, and therefore eliminate systems from consideration on the basis of empirical investigations (Popper, in Krimerman, 1969, p. 48-49). However, we cannot assert the existence or form of presumed systems independent of our observation, and we can never demonstrably verify as correct any given system (structure) as expressed in a model once-for-all. Given these two well-expressed models, we can investigate the goodness of fit between those two expressions, but not between either model and the "real world". Consequently, the ultimate test for discriminating between two alternative models is pragmatic and lies in their relative utility as judged by some observer.

Systems Engineering:

Although it may not be possible to "prove" one model is correct, this is not to say that it is not possible to impose an organization upon a system identified in time and place. Indeed, the resolution of the epistemological controversies in logical analysis entailed in the systems approach has been accomplished pragmatically by accepting a conception of "system" as an explicitly utilitarian construct. Simon's thesis that behavior is adapted to goals and thus reveals only those characteristics of the system which limit the adaptation (Simon, 1969, p. 52) points to the very real possibility of materially embodying the canonical representation--or particular transformation--for any given system, defined as a process. According to von Neumann, a completely general description of a given automaton can be given simply by a description of its states; when specific functions are provided--no matter where they originate--the general automaton will function like the object described. A Turing machine is a universal automaton, which can duplicate any operation of any other specified machine, by being furnished with a description of the "model" automaton, and a set of instructions for its operation--the functions, or specific transformation. (von Neumann, in Buckley, 1968, p. 98) When a real machine and a transformation are related in a one-to-one correspondence, the transformation is the canonical representation of the machine, which embodies the transformation. (Ashby, 1956, p. 29)

Essentially, an explanation of this phenomenon has already been given in another context. Given a theory--a fully-defined conceptual system, complete with a determinant mechanism, $\mathcal{T}$, it is certainly possible, by translating the sequence exhibited in the mechanism into specific algorithms or procedures, to materially instantiate that model for any phenomenon which can possibly fulfill those specified conditions.

Acceptance of such a phenomenological orientation to the study of organizational phenomena implies that a methodological, rather than a theoretical, approach may be more fruitful in deriving testable theories of organization. Kaplan identifies methodology as the explanation of a set of methods and not the application of the methods themselves; the study *of* methods refers to the processes by which concepts and hypotheses, measurements and theories are defined. (Kaplan, 1964, p. 18-23) A methodological approach to the study of organization, then, would make it possible to identify the process of material and social organization with the organization of information in specific definitions--concepts and theories of order or form--which can be identified (and therefore imposed) by some observer.

A framework for just such a methodological approach to the study and design of organizations can be found in the work of Wayne Wymore, whose engineering methodology is exemplary for its characterization of organization in systems definition according to a multiplicity of alternative definitions which could be made for the "same" system. Conceived in this fashion, a "system" is a social construct, and the important question for both observers and actors is: How to account for the multiplicity of systems that can be specified by a number of observers, or by an observer on a number of different dimensions, or at different points in time; meaning, how do a group of observers know that they are referring to the "same" system in these various expressions?

Wymore's Theory of System Design attempts to answer this question by recommending a methodology--as a language format--for specifying and testing hypothetically possible systems. By using this format we are essentially controlling for the conditions of observation by making the members of that set of possible systems equivalent with respect to outcomes (conceived as solutions to problems) and to a given time scale. This equivalence is confirmed by demonstrating that the specifications (or definitions) of each of a set of systems are homomorphic or isomorphic representations of each other.

The Tricotyledon Theory of System Design

Wymore's system design methodology consists of three components: The Input/Output Cotyledon, which is a specification of a matching between possible inputs and outputs to a system; the Technology Cotyledon, which is a specification ofthe available technology with which the input/output specification can be materially realized; and a Feasibility Cotyledon, which is a specification of the relative feasibility of the several systems which both satisfy the input/output specification are buildable in the given technology. Each component generates a number of isomorphic systems which are ranked or evaluated according to an explicit merit ordering over the set of systems, each of which satisfies the specification at each point in the process, supporting the selection of one out of a set of equivalent systems according to a cost-effectiveness tradeoff agreed upon by the clients and designers of the system.

For Wymore a "system" is an "...intellectual construct that we will use to model reality." (Wymore, 1976, p. 53) The definition of system consists of a specification of inputs, states and outputs, together with a specification of how the system changes state in terms of its input and present state. (Wymore, 1976, p. 17) This conception is consistent with Ashby's and von Neumann's definition of the finite automaton, or machine with input.

Wymore's design methodology prescribes a detailed procedure for specifying such systems, which procedure constitutes a common language for communicating systems concepts in analysis and design. The system definition process consists in the following steps, or format, which correspond to the components of a complete definition for a system:

1. Naming the system
2. Specifying a time scale--months, seconds, etc.--for the system
3. A definition or enumeration of all possible states of the system
4. A definition of all possible inputs to the system; together with the input "ports", which specify the particular coded form--or input function--in which acceptable inputs are presented to the system; together with a record of input trajectories which identify the sequence of past inputs to the system
5. A definition of the "state transition functions", which demonstrates how some future state of the system is determined by the present state and the inputs to the system from the present to that future time
6. A definition of the output(s) that correspond to each possible state of the system, and that can be observed when the system is in that state. (Wymore, 1976, p. 39)

The state of a system is its internal configuration at some point in time, which is specified according to two functions, the state transition function and the input function. The state transition function identifies the transformation from one state of a system to another as a function of its input. The input function (f) defines a history or trajectory which specifies an input value for each time unit, thus specifying a range of possibilities for the presentation of inputs to the system, describing, essentially, "...what the operational environment will be". (Wymore, 1976, p. 116-118)

The output of a system is defined by a transfer function (g) which designates a certain outcome as "...any function of the state of the system". (Wymore, 1976, p. 40-43) The output function defines an output value for each state of the system; output trajectories provide values for that output over time. Insofar as the transfer function represents a specification of a matching of a (set of) outputs with a given system state, the function implies a selection of one out of a set of specific output trajectories, such that "...every system that satisfies an input/output specification generates or describes a transfer function". (Wymore, 1976, p. 149-153)

Wymore's methodology thus not only recognizes the possibility for a number of alternatively satisfactory means-ends maps, it explicitly identifies the element of choice with the selection of input and output functions, and with the preference ordering of the set of possible systems thus recognized.

The major justification for employing such a systems engineering methodology, according to Wymore, is in aiding a client to fully define the problem for which a new system will be designed as a solution, and in testing the feasibility of a proposed design prior to its actual construction. (Wymore, 1976, p. 15) The system definition problem, then, entails finding a set of system specifications which can be demonstrated to result in some projected system state and outcome, considered as a solution to the problem as defined.

System Definition:

In defining--or engineering--such a new system, an input/output specification is first generated which constitutes a set of matchings between the history of the inputs to the system and all the possible output histories or trajectories; this description can potentially generate an infinite number of output trajectories for each input trajectory. (Wymore, 1976, p. 19) The input/output specification is basically a formal description of what the behavior of an ultimate system might be, subject to test. In order to show that a system satisfies a given input/output specification, it is necessary to define the system, an initial state, an output generation, and a time scale within which the specification is valid. Defined thus, "...an input/output specification will generate a class of systems each of which satisfies the input/output specification." (Wymore, 1976, p. 21)

If the purpose of this methodology is to state a problem or objective precisely enough that feasible solutions can be derived, then the system definition methodology entailed in the specification of systems according to such language formats can be seen to produce a formal characterization of a system, Z, as the resultant of a composition of a 5-tuple,

Where: $Z = (S, P, F, T, \sigma)$

and:
S = a set of states
P = a set of inputs
F = a record of input trajectories
T = the time scale bounding the system, and
σ = the state transition function

(Wymore, 1976, p. 397)

In addition to presenting a method of characterization of systems which preserves their complexity while at the same time facilitating a precision of definition encessary to prediction, Wymores methodology makes use of the concept of isomorphy to test those definitions at each step in the process. The identification of the components of complex systems, the questions of whether one system is identical with another, and whether one system is a simplification of another are technical questions in this methodology. Such isomorphy is not formally presumed, as in a general systems approach, but is demonstrated indirectly by a process of system experimentation, drawing on the concept of isomorphy as demonstrating a correspondence of structure among a set of system definitions, or assertions.

The concept of isomorphy can thus be used as a methodological tool to establish equivalence among alternatively possible systems; all such systems that satisfy a given input/output specification as defined above can be demonstrated to be formally isomorphic with respect to that specification. Moreover, the concept of system isomorphism is used in confirming that a set of systems actually satisfies a given specification; this concept thus has particular value for testing specification for which actual experimentation is impossible or inconvenient. A proposed system can be demonstrated to satisfy an input/output specification by performing an experiment on the system, started in some initial state with any input function provided, which will show that the output trajectories that will be produced according to the output function are among those matched with the input trajectory. (Wymore, 1976, p. 143-144) Finally, the concept of isomorphy is used to demonstrate the components of complex systems.

Complex systems are composed out of simpler component systems by means fo a coupling procedure. The specification of the Technology Cotyledon depends on the ability of the systems engineer to build a new system by coupling together available technological components, defining the resulting system by means of this "coupling recipe".

> "A System Z can be built in a Technology T provided the system Z is the resultant...of a coupling recipe...all the components of which are in the technology." (Wymore, 1976, p. 183)

Wymore's set-theoretic representation combines complex variables to determine resultant systems by a process of product set multiplication, similar to Cole's and Ashby's, which assigns inputs and outputs for the systems to be coupled; thus two systems coupled together result in a third system which includes them both as subsystems. The state configuration of the resultant system is a combination of the state from one subsystem and that of another, the set of states of the resultant system corresponding to a set of "two-item lists" where the first item is the state of one (sub)system and the second item is the state of the other. The inputs and state transition functions of the resultant system are ascertained in the same manner. (Wymore, 1976, p. 53-56)

Wymore identifies three forms of relation produced by different coupling recipes. Conjunctive couples are simply the addition of two independent systems represented as sets of states and inputs together with the transformations of states in terms of input. In cascade coupling, one system is independent, but the other takes part of its input from the output of the first. Feedback coupling takes place on a single system, in which part of the outputs of the system are returned to it as inputs, and thus the set of total behavior available to the resultant system is a subset of the total behavior of the same system without the feedback loop because part of the input to the system is circumscribed by the state of the system in terms of its output. (Wymore, 1976, p. 54-60).

Any number of systems can be so coupled and the resultant complex system state and behavior--the transformations of state and output with inputs--can be determined by defining all the component systems in the n-system couple, and defining their output functions which define the total resultant system in terms of outputs. (Wymore, 1976, pp. 56-63) Thus, according to this methodology, a System Z''', the resultant of the composition or coupling of two or more systems, is defined strictly in terms of the systems Z' and Z'' which are coupled together to produce it, as follows:

1. A listing of the state of each system component of a coupling recipe is given, from which is derived each state of the resultant system;
2. An identification of the input ports of the resultant system as those which are unoccupied in the coupling recipe--i.e., those not receiving their inputs as outputs from the other system(s) so coupled--is given;
3. The states of the resultant system in time are then computed from this set of states and the coupling recipe. (Wymore, 1976, p. 68)

The concept of isomorphy can then be used to demonstrate that one system is a component of another if, given two defined systems, and a defined coupling recipe in which the supposed system component is among the systems listed, a thrd system can be defined as a result of the coupling recipe which can be shown to be identical to the original. (Wymore, 1976, p. 80)

This emphasis on system specification and test is consistent with the representation of systems as sets of design choices. The third element in Wymore's methodology--the Feasibility Cotyledon--represents the formal solution to such a design problem as the intersection of the input/output cotyledon and the technology cotyledon. This intersection produces a set of systems "...each of which satisfies the given input/output specification and is implementable in the given technology." (Wymore, 1976, p. 221) The intersection is not produced by formally composing two sets, but by producing a set of "artefacts"--again, a formal system specification, this time adding to the input/output specification a demonstration that the components of the coupling recipe exist in the technology available to implement them, and an identification of the set of feasible systems produced by the conjunction of these two specifications in terms of a trade-off of costs and benefits.

Finally, once we can clearly identify alternative system configurations as trade-offs of various specifications, the notion of optimality can be represented in the specification of a merit ordering, or preference ranking, defined over each of these three design elements, such that the merit ordering over the feasibility cotyledon represents the resultant systems in terms of a tradeoff between the features or characteristics represented in the input/output and technology merit orderings. (Wymore, 1976, p. 32)

Wymore's theory of system design, finally, specifies a design format or procedure which consists of the generation of a set of system-theoretica constructs and their supporting artefacts: An Input/Output specification X, a Technology T, merit orderings defined for the input/output, technology and feasibility cotyledons, respectively; and a system test plan. (Wymore, 1976, p. 353) With these documents, a multidisciplinary design team is able to communicate precisely without limiting the complexity in their consideration of a number of alternatively possible systems for achieving some design objective.

Conclusion

This chapter has differentiated three separate positions still current in Modern Empiricism: Inductive Empiricism, General Systems Theory and Systems Analysis. We have suggested that a particularly well-articulated movement in modern philosophy, begun by the logical empiricists, has been translated into a new research paradigm from which to view the phenomena of organization as the object of methodological inquiry. The work of Wayne Wymore illustrates a clear instance of this Systems Analytic Paradigm, a prescriptive approach formalized as an engineering methodology, which essentially details a procedure for the context of discovery as a process of definition or specification of meaning. The systems analytic paradigm exhibits several distinct advantages over the other methodological approaches to the study of organizational phenomena, and at the same time several distinctive limitations of its own (which will be discussed in Chapter 2).

In this chapter we have used that systems analytic conception of organization as showing in our definitions or expressions to identify two separate definitions for the concept of Isomorphy, which concept has then been used to trace differences in the meaning of System, as the basis for distinguishing among these methodological paradigms. From this analysis three issues have been identified which distinguish the characterizing assumptions in systems analysis from alternative paradigms:

1. The concept of "System" is clearly identified as a construct--a model. Wymore is particularly clear on this point; an input/output specification is not only not itself a system; it determines a whole

set of systems. (Wymore, 1976, p. 142) This set of systems, moreover, does not refer to a group of concrete entities, or even types, but to a set of assertions--definitions per se. (Wymore, 1976, p. 164) Similarly for Ashby and Simon and other apologists for systems analysis, the concept of system refers to models, to sets of ideas as communicated in a language.

The Systems Analytic position contrasts with that of the General Systems Theorists and the Inductive Empiricists, both of which posit an order in the universe that is external to our observations and thus, we must argue, ultimately unsupportable by any of our observations. The burden of proof thus falls upon the proponents of these positions to demonstrate that their concepts are even meaningful. The General System Theorists continue a long tradition of identifying the (presumed) order in the universe with general forms and functions, but we have argued that those general forms and functions must, therefore, refer to terms in a language since being general they cannot refer to observations at all, which are discrete. Thus, even any heuristic value in using the language of Systems Theory is lost because these general terms cannot be used to refer unambiguously to specific observations.

For the Inductive Empiricists, the presumption of order in the universe is posited to support an approach which correlates the supposed properties of these "natural" or "concrete" systems in such a way that the idea of organization as a patterned relationship among the elements of the (again) presumed systems from which these properties are abstracted is altogether undermined in a reconstruction of the form of their observations on the basis of these secondary characteristics.

The Systems Analytic definition of System as a construct--a mental representation--frees the observer from these unnecessarily restrictive ontological assumptions concerning the existence of order in the universe, or the properties of such universal forms, or the approximation of our theories to the "true" form of natural systems, presumed "real" because external to perception. We will argue throughout this analysis that such ontological presumptions ultimately obstruct any clear definition of order--or systematicity--among our observations in terms which adequately communicate the meanings of organizational phenomena from one definitional environment to another. Not only need the systems analytic position make no such unsupportable assumptions, but the particular strength of Wymore's engineering methodology over other analytic models such as Ashby's and Simon's is that the social element of definition in the concept of System-as-Construct is made explicit and is therefore clearly identified in the choice of input and output functions in defining systems for analysis and design, and in the evaluation and selection from among alternative systems according to explicit preference criteria. Not only is the system a construct; it is constructed. Therefore, much of the order that we recognize is both attributed and contributed by people, and this process is observable in a way that "order-in-the-universe" is not.

Moreover, such unsupportable ontological presumptions of order support similarly unsupportable epistemological presumptions concerning' methods appropriate to the investigation of organization among natural phenomena, which suggests the second issue addressed by systems analysis.

2. In the Systems Analytic Paradigm, order or organization is conceptualized as a constraint on the total set of definable possibilities. In contrast, in the other two paradigms, order is conceived as a characteristic property predicated either of universal forms--as in General Systems Theory--or of concrete entities--as in Inductive Empiricism, in which case organizational properties are added to the other properties and individual elements under investigation. We have seen, however, that when the individual elements are subtracted from such a definition of System, no residue of form remains; in fact, it has been demonstrated that the identification of form changes as the scope of observation changes, which suggests that order, as well as System, is recognized in our observations, and as such can only be identified with constraints which limit the myriad of possibilities to some recurrent set.

The difference in the conceptualization of Organization in these competing paradigms is exhibited in their differences in the treatment of the concept of Isomorphy. For the General Systems Theorists the postulation of a natural order in the universe is reflected in a representation of all systems as formally isomorphic, thus justifying the use of a single set of equations to represent that general order in environments as diverse as ant populations, monetary systems and political parties. Similarly, the methods of the Inductive Empiricists imply the same definition of isomorphy as standing for <u>the</u> order in nature. This allows them to use a simple logic which implies a single if-then conditional relation between problems and solutions, goals and means, decomposition and composition of some analytic problem. Furthermore, their belief in a natural, external order leads them to assert an isomorphy between their theoretical constructs and some "real" or "concrete" system--conceived as an organization, an entity external to observation. Even Ashby and Simon, although important proponents of the Systems Analytic paradigm, and though accepting a view of Organization as constraint, echo the Inductive Empiricists when they fall back upon presumptions of order in the universe to support their protocols as evidence of an "external" order showing in a sequence of observations.

We have argued that the presumption of formal isomorphy, like the presumption of the ontological existence of systems, is ultimately unsupportable. It is unnecessary, and, moreover, unnecessarily limiting, to posit that a sequence of observation represents any such "natural" order. Whether organization is presumed to be exhibited in differential equations, or in empirical protocols, the portrayal of any specific organizational form is obscured by such presumptive--and general--research orientations. It is much more useful to investigate the variety of forms showing in a range of observation from a number of different perspectives, and to compare those forms by controlling for the conditions of observation and examining the isomorphies exhibited in the record. Wymore's system design format provides a procedure for characterizing a system unambiguously without the apologia of emergence and equifinality to mask an incomplete or inconsistent specification, or to protect some model from its inability to predict outcomes (a particular failing of general systems theoretic models).

The advantage of Systems Analysis over other competing paradigms, and the strength of Wymore's theory of system design over both Ashby's and Simon's explanations, is that Wymore's methodology does not arbitrarily truncate the set of possible system structures nor deny the variety of possible forms which organized systems can exhibit by attributing any natural order to a sequence of observation, or by asserting that factoring or decomposing a problem for analysis leads to just one solution--the mechanism, or organization in the system. We have demonstrated that this "mechanism" cannot be simply or directly inferred as a means-end map derivable from a statement of a goal or problem, and that to make such an assertion amounts to a claim that one's model is isomorphic to the order in the "real world", which is ultimately unsupportable.

In Wymore's systems analytic methodology, the concept of system isomorphy, rather than being presumptively identified with the natural order of the universe, is used to confirm the equivalence of alternative system constructs and to test the specifications of any given construct to fulfill its designed objectives. The latter use has perhaps the greatest practical value in providing a means for testing the adequacy of a set of specifications for a system which is too complex and/or inaccessible to permit of direct experimentation, thus providing a confirmation of its measures of effectiveness in advance of actual operation.

The recognition of a constraint as a pattern, or redundancy, in our observations in some environment of interest can be used to assert a pattern of order in the universe, as do Ashby and Simon and the

Inductive Empiricists, or it can be used as Wymore does to exhibit an isomorphy between our several representations of that universe, from which we may infer that the pattern of our observations matches that of our theories and thus is the basis for theoretical confirmation, or that the pattern exhibited in one characterization of a system matches that represented by another and thus gives a confirmation of the commensurability of the language(s) we are using to communicate with each other. In this manner, the systems analytic approach constitutes a more powerful research paradigm than its competitors.

By creating these successive system definitions--the Input/Output, Technology and Feasibility Cotyledons--the designer constrains, and therefore reduces, the number of alternatively possible relations that can obtain through the coupling recipes and the input and output functions, and thus limits the number of system configurations possible.

For Wymore, systems analysis thus appropriately rests on an identification of the input/output relations among simpler system components, from which a resulting system can be determined by means of the coupling procedure. If one knows the state of the resultant system at some time, as well as the input to the unoccupied ports (meaning those not internally coupled) then it is possible to determine the inputs provided to the occupied input ports within the resultant system from the state of each of the components and the definition of the output functions. From the state transition functions of the individual component systems we can deduce the next state of each individual system, giving thus a list for all the components which is the next state of the resultant system. (Wymore, 1976, p. 68) The benefit of this methodological approach is that it enables a degree of determinacy of characterization and predictiveness of inference by employing more precise and, therefore, more powerful definitional and computational techniques than do other conventional research methodologies.

The very specificity which is the strength of this method, however, imposes limitations on its valid application to "real-life" systems problems. Wymore argues that the process of analysis does not logically reverse to enable us to identify separate components within a resultant system, once given, or built. Although subsystems can be identified as any subset of the states and outputs of a larger system of which they are a part, system components are more difficult to specify once a resultant system has been composed. A system component is truly identifiable only in the coupling of one system to another because once the resultant system has been produced by the conjunction of two systems, the component states are not merely a subset of the states of the resultant system, but are integrated into it in the coupling recipe. Thus,

> "...it is a difficult theoretical matter to discover such relationships in any given system whose components are not known". (Wymore, 1976, p. 80)

It is clear that the analyst must assume as given either the input/output specification (including separate identification of system components) or the coupling recipe itself (which can be seen to correspond to Ashby's rule, $\mathfrak{F}$, for composing the system) in order to characterize the structure or organization in a complex system. We must now assert that either it is inevitable that we beg the question of organization as an identification of some rule or mechanism that can be identified with a system (which our mathematical equations supposedly represent , and on which our predictions are grounded), or else we can recognize that the opportunity to "see" and thus to confirm a structural form in a system in its composition out of smaller, simpler components may be restricted to the process of composition--or design--itself as the only period in time during which components can still be separately identified and in which the choices of input and output functions are (perhaps) overtly expressed.

It is this difficulty which underlies the emphasis on system design in a methodological approach to organization analysis. In the analysis of existing systems an observer creates a model which may or may not reproduce a presumed coupling recipe, which is not unambiguously evident in a listing of components; in systems design a model is created from which a new system is to built, and thus explicitly rather than implicitly identifies a specific coupling recipe as part of its application.

3. The ability of the systems analytic conception of organization as constraint to represent the element of choice from among a variety of alternative possibilities suggests the third major issue which distinguishes this paradigm from its competitors--the irreducibly social element in systems analysis and design, which necessarily embeds Systems Analysis in a larger context of value and action. It is a particular strength of Wymore's methodology over other systems analytic methods that this, essentially social, element of choice is explicitly included in the investigation and attribution of order in systems, which can be

identified with the selection from among equivalent possibilities by a deliberate negotiation undertaken by the participants to the system design process. "The client must, essentially, tell us how to decide." (Wymore, 1976, p. 233)

Since a methodological approach is grounded on a pragmatic demonstration of the utility of a proposed system, the test of that system is to be found in the ability of the conceptual model to predict and control outcomes in a construct engineered in the "real world" on the basis of its specifications. Thus it follows that a design approach will emphasize problem-solving and issues of error and reliability in applying engineering methods to the investigation of organized systems. In such an approach order is demonstrably not external to our observations; it is bound up not just with out perceptions of pattern and redundancy, but also with regard to choices that people make according to evaluative criteria that can either be made explicit and thus taken into account, as in Wymore's method, or left unexamined as a presumption of some underlying "natural" order.

Wymore's explication of the system definition process identifies a set of alternative systems that can be identified with a set of observers, or actors in a multi-disciplinary team, and thus directs attention to the context of discovery in what is essentially a social process. However, he does not account for the locus of choice of the nature of the decision-making process that selects one from a set of alternatively available systems, other than to refer that process of selection to a criterion set of values by which to discriminate among alternatives. Wymore notes that early attempts to use the tricotyledon theory demonstrated that the tricotyledon theory can be very effective in approaching problems of design in large-scale, complex, human/machine systems; "...the tricotyledon theory is no absolute guarantee against personality clashes within interdisciplinary teams." (Wymore, 1976, p. 382) What is needed then is a description of the social dynamic--or feedback--by which such specification and selection is undertaken in action.

A methodological approach can be confirmed and justified in use; however, an explanation of the use of a method, language, technology, or any other cognitive framework for action (including investigation itself) requires providing a description of more than the technical specification of systems alternatively definable, given a client and a problem. Because a methodological approach is--by definition--empty of content, the advantages gained in the precision of specification and inference by formalization of analytic methods and languages of inquiry are, in themselves, insufficient to resolve practical problems of implementation and use. As Wymore notes, the primary responsibility of the system engineer is to help the client define the problem for which the new system would be a solution. In this light it is crucial to the application of systems analytic methods that we be able to identify clearly in advance who is the client or decision-maker to whom (or by whom) the alternatives are addressed, and what is the problem or objective use to which the methods are to be directed? It is possible to discern at this point a particular circularity which is involved in a methodological--or design--approach to systems definition and analysis: If this approach does not generate a theory of organization, it must presume one.

In Chapter II we will investigate the nature of Uncertainty in this process, and argue that at least part of the error involved in predicting systems outcomes can be attributed to uncertainties deriving from the limits of the analytic model to specify the context from which it is derived and in which it is to be applied, and to the process of implementing the definition in action. These limitations are characteristic of the pragmatic approach to systems engineering. Wymore's framework is formally adequate to account for the variability involved in abstract systems, but this methodology must be extended to account for the implementation of that system--which ultimately is the test of the validity and value of any formal model.

We will be interested in developing a modelling procedure to supplement Wymore's system design procedure, from which a resultant system might be deduced not just from a chosen Input/Output specification, but from a combined specification of: 1) the model(s) <u>for</u> the system expressed in the design per se, i.e., the resultant system Z; 2) the model(s) <u>of</u> the system expressed by organization members and observers, which understandings define the context into which new (resultant) systems are to be introduced; and 3) a characterization of alternative modes of implementation expressed in decision strategies and exhibited in histories or trajectories of action beginning with the introduction of new systems of organization. Perhaps the most direct way of extending the model and predicting how these methodologies might be used in in the development of automated office systems, would be to investigate those areas in which systems engineering methods have been instantiated to date, most prominently in the influence of the operations research movement begun during World War II, on traditional management science and recent artificial intelligence approaches to computerization in offices.

Chapter II

EXIGENCY

In his discussion of the history of the socio-technical systems movement, P. G. Herbst contends that the current computer "revolution" represents not a new form of organization, but a working out of the conclusions of the first industrial revolution, the end of which was (and still is) logically and technically defined in terms of process automation. (Herbst, 1974, p. 17) The concept of automation means the combination of some (perhaps implicit) model of social organization as represented in an organized body of information, together with the development and configuration of hardware in the design of self-regulating continuous-process systems. This chapter considers the relationship between technological development and social organization (at the macrolevel) in different historical periods as a basis for investigating the relationship between the technology of office automation and the human-machine interface (at the microlevel) where problems in use are experienced.

A major source of uncertainty in the design as well as the implementation of computer-based technologies may be attributed to controversies over the nature and extent of social-structural change implied in the (re)-definition of work and working relationships associated with applications of office automation technologies in ongoing environments. In attempting to account for ergonomic problems in the use of automated office technologies, and (at the extreme) for implementation failure of designed systems, a broad review of the literature indicates that processes of structural change in ongoing organizational environments are not well-understood, and that there are few empirical studies of technologically-induced change the results of which can be extended with validity to other environments. However, there are numerous historical examples of systems engineering as a process, many of them associated with the prosecution of war, and notably so in the course of American history, and in the development of computer technology in the course of allied efforts during World War II.

Computer science is a prototypic development field--standing midway between the development of formal languages and engineering disciplines involved in machine development and design, and (in the emergence of that development within ongoing organizations) the theory and practice of organization design and management control. The power and flexibility of computer technology, exhibited first in World War II and extended into the dramatic postwar successes of the aerospace and electronics industries suggests that computers could well be the vehicle for the occurrence of a second industrial revolution, given the magnitude and scope of change which the introduction of computer technology implies for modern organizations.

More than the development of computers as hardware, however, we will argue in this chapter that it has been the application of systems analytic methodologies in organiztions--among which methodologies the emergence of computers and computer languages are notable instances--which has generated the greatest transformation in organization theory, structure and practice. Viewed in this light, both the development of computing devices and the transformations of organization structure which we now expect to follow from the implementation of computer technology have a history which can be traced beyond the 1940's to the beginnings of the Industrial Revolution, and beyond that to the emergence of the ideas of automation much before the dawn of the "modern age".

This history of systems engineering methodology--and lately of computerization as reflecting and deriving from this methodology--depends in large part on the exigencies created by war, and the applications of science in industry and government which have been developed in response to wartime emergencies in the U.S. and Europe for several centuries. The systematic application of scientific knowledge to corporate problems is considered a hallmark of the modern age, and a basic factor in the Industrial Revolution. More than in any European country, the cooperative relationship between military, government and industry in systems engineering under the pressure of war has been characteristic of the American context of development for a broad range of technological innovation from the outset. This relationship can be identified with a particular mode of development which has favored the rapid and commercially successful emergence of new technologies; however, this "designer-based" mode of development has also been associated with certain inherent limitations in the implementation of those technologies and in their effects on human beings who use them.

Computers were not developed so much for their own intrinsic value as they were built as a necessary tool for carrying out the type of systems engineering which was undertaken during World War II. The implementation of computers-in-use in ongoing organizations, therefore, presumes a "systems approach" to problem-definition and solution, which approach has implications for the structure of decision-making and communication in those organized contexts. The emergence of computers and computing thus assumes a central place in the applications of science-in-industry in two different senses:

1) The development of computers as hardware, as well as that of the formal languages associated with their use, were an outgrowth of the application of systems engineering methods to military problems during World War II, an effort which was accompanied by the creation of new organizational forms under extraordinary--and temporary--circumstances.

2) On the other hand, the development of computer applications and the successful implementation of computers-in-use in the course of planning, system design, testing and installation depends upon the extension of these same systems analytic methodologies within established organizational structures in civilian organizations.

It is the application of systems analysis upon which the use of computers depends, and it is systems engineering methodology--and not computers per se--which transforms organizations. The extent of that transformation, however, is variable, and in spite of the dramatic successes of systems engineering methods in the design and development of computer hardware, many observers have noted pervasive limitations and uncertainties which accompany the implementation of computer-based systems in ongoing environments. These uncertainties appear not to be related so much to the capacities of the equipment, but rather to the capabilities of that equipment-in-use as defined in a given environmental context, and to an organizational capability for successfully managing the implementation of technologically-induced change.

Viewed in this way, differences in the context of development can be seen reflected in different technological outcomes or "styles", and differences in the context of implementation can be seen in different problems associated with the use of these new technologies. In this broader view, the emergence of computer technology and the application of this technology in different environments is a special case of the field of systems engineering, a case so special that it illuminates what Wymore recognized as the irreducible social--and value-relevant--element in processes of technical design, as well as the particular advantages and limitations associated with systems engineering methodologies in war- and peacetime. These advantages and limitations provide a clue to the underlying reasons for problems in computerization and suggest alternative directions for design and implementation strategies in the future.

In support of this argument, we refer to available historical accounts of systems engineering projects--including the development of computers--in a variety of environments. This record indicates the following:

1) Systems engineering is not a recent phenomenon; the type of directed operational research which is the substance of systems engineering, and the prerequisite for computerization, has a long tradition. The computer itself, as an automaton or process control device, is likewise a conspicuously recurring idea since ancient times.

2) In the development of computers, and in systems engineering in general, the context of implementation and use has exerted significant influence on the development of new technologies.

3) The introduction of new technology has commonly been associated with transformations in the social organization of enterprise. Because new technology represents the addition of alternatives to the set of components making up a description of the organized process, the introduction of technological innovations both facilitates and necessitates transformations in the organization of the enterprise. This transformation imposes a new set of constraints on predicting and controlling outcomes which, by extension, may imply changes in the process of management.

The development of computer technology has been based extensively on systems engineering methodologies in the design of equipment and in the development of programming languages and applications models. As a process control device, both the computer itself and the system of which it is a part can be described as state machines--or automata--here meaning, in principle, that each state that the system is in, combined with some input from the environment at each moment in time could translate directly to a determinate (or statistically approximate) outcome. However, both the design of computer hardware and the use of computers as machinery which processes information are activities which involve logic, language, communication, and social organisation, all variable factors which extend considerably beyond any technical systems definition. As demonstrated in Chapter I, a methodology of design-by-analysis involves more than merely the "application" of formal methods of analysis, design and control to immediate objectives. Because computers are multi-purpose machines, and because the contexts of their application are so highly variable, definitions of use cannot be derived from the stipulation of systems design objectives or specifications without at the same time specifying that context of implementation in terms which are commensurable with design criteria. Analytic methodologies-in-use thus presume some implicit theory--or concept--of organization to which these methods are referred during processes of design and implementation.

Based on an examination of previous systems engineering efforts, we will suggest that the relationship between a formal system design and a system-in-use in some environment is defined during processes of implementation. The process of implementation, therefore, can formally be described as having the logical status of an instantiation which establishes a mutual relationship between technical design and social action, in which representation orders and, therefore, constrains organizational "realities". This process can be identified with the concept of adaptation, and perception of constraints--or exigencies--as "problems".

L. J. Henderson has noted that adaptation--or system self-regulation, such as that involved in the implementation of new technologies--refers not only to transformations taking place in the organism (or process) of interest, but also to an environmental context, which is itself simultaneously undergoing change. (Henderson, 1958, quoted in Miller, 1978, p. 68) Insofar as systems engineering represents a "problem-solving" activity, the process of defining specific problems for application of new technologies establishes the context--or environment--within which the development of systems and procedures takes place. Thus the context of development is decided in part by the manner in which the system is conceived in use, which is variable. Because this context of application varies, assumptions concerning the nature of the system and its context, the development of structure and procedures, and the locus and scope of decision-making may not be consistent in the definition of the system produced by designers and that produced by the actions of users--with potentially adverse consequences for the reliability of the total system-as-implemented.

The development of computer technology exhibits this "contingency"--or contextual dependency--in a sequence of innovations taking place in different environments, and against a background in which significant changes have occurred not only in societies and cultures in which systems engineering projects are undertaken, but also in theories of social organization and management, partly as a result of previous changes in that social-cultural environment, including changes represented by the development of formal methods of systems analysis as well as the invention and use of computer-based technologies per se.

Wymore's system design system presents us with a clear description of the systems analytic paradigm in a design model which is unique for the formal rigor of its methods of analysis and for its broadening of the concept of system definition to include the process of system design itself. This method thus facilitates consideration of alternatively possible systems and outcomes. It is possible to discern distinct (and emergent) developmental trends in the wide stream of research which has contributed to the development of computers and their applications. Although it may be difficult to separate these trends in the ongoing flux of change, it seems feasible that this method of analysis could usefully be extended to characterize alternatively possible contextual environments, in terms of underlying assumptions concerning the nature of the system, its objectives, and its processes of communication and decision-making built in during the course of system development. On this basis it should be possible to identify differences in the mode of development as predictive of differences in the outcomes of implementation.

One reason for the differences in styles of development of formal methods such as systems engineering and computing is that although something we could call "informal engineering" has long been practiced, systems engineering as a formal discipline has only recently been developed over the course of the last century, and then largely as a consequence of "practice" rather than the formal application of theoretical principles and methods. Throughout the 18th and 19th centuries, many large-scale collective endeavors were undertaken, which could be explained after-the-fact as reflecting systems engineering concepts. These endeavors included military conflicts in which problems of logistics, communication, and decision-making (Command and Control) reflect a continuity with ancient campaigns; the taking of census data in various times by different, and generally "new" governments, from the Norman conquest to the American and French revolutions, as well as the great civil and industrial engineering projects which were an especially notable feature of 19th century industrial development (and associated with the Industrial Revolution per se).

The modern concept of systems analysis, however, is of recent origin, extending only as far back as the 1930's, a period in which philosophical empiricism was especially strong, particularly in Europe--and notably in Germany and Great Britain. The development of computers and computer science, from which word processing is only a latest derivation, is generally considered of even more recent origin, as a product of the 1940's and World War II, in particular. However, the development of computers and computer languages is dependent upon prior achievements in the areas of formal languages, the innovation of automated machinery, and in the use of systems engineering methods for planning and control of human effort in complex organizations, developments which occurred separately over several centuries and only converged in the 1930's and 1940's.

Although certainly the intellectual antecedents of computing can be traced beyond the enlightment, in following the developmental history of formal languages and methods of inquiry in physics, mathematics and philosophy during the period roughly from Leibniz' time to the post-World War II era, it appears that formal ideas were circulated only among a very small educated elite and that there was not otherwise widespread contact with these ideas among the general population. Formal methods and concepts of management were not part of the 19th century literature of business (what little of it there was, which was heavily weighted to personal biography and testimony), nor were they characteristic of practical engineering projects. Moreover, among those who were responsible for developing formal languages and systems of logic, and for considering the question of the understanding of the world in terms of scientific explanation, there has been a fairly long tradition, in western cultures at least, which has tended to separate formal thinking from mechanical endeavor by a wide gulf of social distance, which has made the application and use of new technologies highly problematic.

<u>The Invention of Computing Machines</u>:

In ancient Greece, mathematician-philosophers entertained themselves by building automata--intricate "toy" replicas of living creatures and figures in a tradition of craftsmanship which flourished in Europe during the Renaissance, and throughout the 19th century in clockwork mechanisms which could recreate the orbits of the planets in the solar system and engage in games of chance and skill. In the 1st century A.D., Hero of Alexandria created precision appliances of many types, including spheres which poured hot and cold water, mechanical birds that moved and whistled, a turbine driven by a steam jet, and an automatic theatre complete with little figures which acted out scenes to the accompaniment of thunder and lightening, singing and dancing, and ships crashing on the rocks. (Leithauser, 1959, p. 23-24) In the 18th century the French mechanic, Jacques de Vaucanson, constructed among other wonders an artificial duck which waddled and ate, and two mannequins--a mandolin player and a spinet player--which were copies of human organisms down to precise renderings of their internal organs. Perhaps his most controversial automaton was an artificial donkey which worked a loom, in derision of Vaucanson's enemies, the silk workers. (Leithauser, 1959, p. 266-267)

The construction of automata has held a fascination for intellectuals for centuries, and in this diversion a common element of representation-for-its-own-sake emerges as a characteristic feature of the development of such clockwork systems. According to Leithauser, the ancient Greeks respected abstract thought, as exemplified by mathematics and philosophy, but denigrated manual labor and mechanical talents to such an extent that they would not stoop to undertake practical endeavors, except in the most extraordinary of circumstances. Thus Plutarch could write about Archimedes, whose work, ironically, has such importance for the most mundane aspects of modern everyday life, that

> "He held practical mechanics, and every art practiced from necessity to be low and worthy only of the artisan. His ambition was confined to such sciences as possess an inherent value of goodness and beauty, without serving necessity."
> (Leithauser, 1959, p. 22)

Mechanical systems created by the great thinkers of the distant past were valued for their uselessness, because the social mores of ancient Greek society were such that it was repugnant for men of reason to apply their talents to mundane labor, which was the province of slaves. Instead, the talents of the intellectuals were devoted to problems in language and philosophy, which found their translation into issues in ethics and politics. It is in the political arena of ancient societies--rather than in their material technologies--that the systems engineering tradition is most clearly exhibited, albeit not formalized into a body of knowledge of abstract principles. As the wealth and power of Greek society declined, however, the social distance which maintained philosophers and mathematicians at a remove from the practical, economic endeavors of their neighbors began to crumble, and their talents were increasingly called upon to contribute to more strictly technical pursuits, ultimately reflected in the public works projects carried out by the Roman "heirs" to the Greek tradition of inquiry.

The influence of systems engineering methodologies in the invention of computing machines reflects characteristics of both aspects of this mode of "technological development": a concentration on building a computer as a state machine (in the tradition of Zuse, but not necessarily of Turing) as well as an implied conception of the application of computer technology to organizational problems associated with the coordination of facilities, labor and materials involved in the engineering of complex, large-scale socio-technical systems. Each form exhibits the instantiation of some abstract machine, with certain differences. In automata, the human component is withdrawn when the machine is constructed, and thus the "designer" is external to the system, which is, then, self-regulating. In socio-technical systems, although describable as state machines with respect to plans, laws and other formal arrangements, humans are included among the system components, and thus to a large extent the role of the "designer" is internal to the system, which is therefore also self-regulating, but on a different basis.

As with the concept of automaton, the idea of building a calculating machine can also be traced to antiquity. The abacus is one such machine. The slide rule, invented in the 17th century, is also a calculating machine, made possible by the introduction of logarithms and logarithmic tables into the collection of formal methods, attributed to the Scottish mathematician, John Napier. In modern parlance, the abacus is a digital "computer" while the slide rule is a mechanical analog computer on which arithmetic distances are represented on a scale analagous to geometric measurement. (Schmidt and Meyers, 1970, p. 18)

According to Schmidt and Meyers, the first mechanical calculating device "was invented to aid business data processing". In 1642 the French mathematician, philosopher and religious thinker, Blaise Pascal, impressed with the drudgery of financial calculation which was, in part, the nature of his father's work, undertook the invention of an adding machine, which operated by mechanizing repeated additions and subtractions. During the same historical period, the German philosopher and mathematician, Gottfried Leibniz, invented in 1670 a machine which could perform additions, subtraction, multiplication and division, as well as the extraction of square roots. (Schmidt and Meyers, 1970, p. 18)

Leithauser also argues that the invention of the idea of a calculating, "thinking" machine was not necessarily the effective impetus to producing the calculating machines invented during the 19th century, the practical successes of which were the seeds of the giant office machine industries of the 20th century. According to Leithauser, the key to transcending the ancient passtime of mechanically talented mathematicians in building "toy" automata, was to be found in 1) practical applications which would justify the manufacture and cost of building such machines, and 2) the availability of an instrument-making industry which would be capable of manufacturing precision tools with high-quality materials. (Leithauser, 1959, p. 272)

These prerequisites have been most fully exemplified to date in the work of the British mathematician, Charles Babbage, who combined an attention to practical objectives with remarkable skill in mechanical engineering to produce his "difference engine", introduced in 1812. Babbage is remarkable for combining in one person all the prerequisites necessary to build a working computer: 1) the invention of not one but two computing machines, the "difference engine" and the "analytical engine"; 2) the craftsmanship required to advance the state of art in mechanical engineering to the point necessary to produce precision components for his machines; and 3) the formal mathematical education necessary to develop a mechanical notation designed to carry out the machines' instructions. According to J. J. Dubbey, by 1833 Babbage had anticipated all of the characteristics of the modern computer, which did not (again) become available until 1937. (Dubbey, 1978, p. 178-179)

At the time that Babbage began his work, available calculating machines were crude adders and multipliers which operated by means of a hand crank, and which usually did not work. Babbage (as Leibniz before him) was impressed with the enormity of a certain practical task of which he was aware, namely the calculations of new tables of logarithms ordered by the French government, which project involved nearly 8 million figures in producing one table alone. The calculations were performed according to the mathematical method of differences, but the aspect which interested Babbage was the division of labor among the 96 persons who performed those calculations. This group was divided into three sections:

1) The first consisted of 5-6 highly skilled mathematicians who performed the analysis and determined the formulae to be used in computations.

2) The second was made up of 7-8 skilled calculators who received the formulae from the first section and, after transforming them into numbers,

supplied the third section with the appropriate differences at stated intervals, receiving in turn from that section the results to be independently verified.

3) The third section, comprising 60-80 persons, skilled only in the rudiments of arithmetic, completed the tables by means of simple additions and subtractions performed in a prescribed order. Dubbey cites the Rev. Dr. Robinson of the British Analytical Society as having recalled that Babbage commented to him, half in reverie,

> "I am thinking that all these tables (logarithms) might be calculated by machinery."
> (Dubbey, 1978, p. 174)

Babbage noted that this process could realize a considerable savings in routine human labor and labor costs largely through the elimination of those persons making up the third section, whose labors could be performed by a large enough difference engine. The first successful calculating machine he produced was the small "difference" engine, which performed automatic calculations and printed out the results in a fraction of the time required for a person to produce the calculations manually--at a rate originally of about 44/minute. The machine operated according to the method of differences, requiring addition as its only mathematical operation. The operation of the machine in performing calculations for trigonometric and logarithmic tables used a method based upon the assumption that an equation of a degree "n" will have a constant "nth" difference, such that once initial values are provided for X and Y, the remaining values could be calculated. In the difference engine the sequence of successive values of the quadratic functions are calculated from a table of differences in which the values are ranged in columns. From these columns the products can be calculated using only simple addition by successively adding and recording the columns. (Dubbey, 1978, p. 197)

Babbage's first machine was a small desk calculator which tabulated a second-degree difference equation, $\Delta^2 u_n = C$. This first difference engine was successfully completed in 1822, and he set about to acquire funding to undertake a larger difference engine which would be capable of performing calculations to six orders of difference, represented as $\Delta^7 u_z =$). This engine would have been sufficient, according to Dubbey, to have completed the calculations of the French tabulators without fully extending its capacities. (Dubbey, 1978, p. 184-5)

In building his engines, Babbage adopted the punched-card principle of the Jacquard loom, which was invented in 1801 by Joseph Marie Jacquard. This loom, the automatic weaving machine which first successfully automated the 19th century textile looms, operated by following instructions coded into punched cards. (Tomeski, 1979, p. 59) Babbage applied this principle in his calculating machines, which consisted of a store, a mill and a control system, based upon this punched-card mechanism. The store contained data, programmed instructions and calculations in progress; the mill contained arithmetical operations which are carried out on the data introduced from the store, two at a time; and the control system operated by means of the punched cards to control the sequence of operations in the mill, and to input data to the engine by means of operation and variable cards. For output, Babbage had developed applications in which punched cards would be used to print out the results automatically, and to imprint other cards. According to Dubbey, "...these five parts are the major logical components of any modern computer." (Dubbey, 1978, p. 199)

In the course of building his difference engine, Babbage noticed that his machine would compute sequences resulting in successive values of the square of the difference as a function of itme, and inferred from this that the machine would be able to calculate series if it were suitably arranged mechanically.

According to Birkhoff (1980) Babbage conceived the extension of the punched-card principle to the basic conceptualization of the analytic engine in 1836.

> "This day I had for the first time a general but very indistinct conception of the possibility of making an engine work out algebraic developments. I mean without any reference to the value of the letters. My notion is that as the cards...of the calculating engine direct a series of operations and then recommence with the first so it might perhaps be possible to cause the same cards to punch others equivalent to any given number of repetitions."
> (Knuth and Pardo, in Metropolis, 1980, p. 201)

Although Dubbey cites this as evidence of Babbage's "weakness" for getting sidetracked, he also notes that the emergence of the idea of the analytical engine--in which are combined the features of modern computers--was suggested by the powers exhibited in the original difference engine, capabilities which exceeded those initially conceived and designed for the device. That excess capacity suggested to Babbage that by reconfiguring the connecting wheels making up his device, it would be possible to influence any part of the machine in a number of ways, performing any operation in any order as many times as required. (H. P. Babbage, quoted in Dubbey, 1978, pp. 196-197)

Finally, Babbage developed a mechanical notation to carry out the instructions. This notation amounted to a logical program which would perform any type of analytical calculation by analysing it into a series of basic operations, or instructions. These instructions could be repeated as many times as necessary, and it was also possible to make decisions--i.e., to establish conditional operations--and then to continue with ongoing operations. Babbage planned a large stored "vocabulary" for his devices. Six orders of difference, each having 20 places, were planned for the difference engine, and in the analytical engine the store was to contain 1000 numbers, each significant to 50 figures. (Dubbey, 1978, p. 198)

Babbage's mechanical achievements in constructing his difference engines have been widely recognized. (Davis, 1965, pp. 3-4; Schmidt and Meyers, 1970, p. 19; Dubbey, 1978, p. 194) The British Royal Society certified the mathematical soundness and mechanical genius involved in the construction of the difference engine as having inspired progress in mechanical engineering, in developing tools and in training mechanists, and in revolutionizing the founder's art by devising a method of casting which was superior to cutting. (Dubbey, 1978, p. 194)

His mathematical work, and especially the development of his special notation--essentially the first example of programming for a computer--are considered sound today, and the method by which his engines were designed to execute a sequence of operations arbitrarily and to store data internally bears a marked similarity to the stored program concept introduced to computing by John von Neumann over a century later. (Davis, 1965, pp. 3-4; Schmidt and Meyers, 1970, p. 19; Dubbey, 1978, p. 216) His contemporaries in the British Royal Society, and in other scientific and governmental groups were impressed by the soundness of Babbage's calculations and by his special notation in particular, and argued that his undertaking was altogether feasible and laudable and worthy of governmental support. (Dubbey, 1978, pp. 184-185)

However, in spite of his remarkable accomplishments, the context in which Babbage's developmental work was undertaken ultimately proved not to be conducive to furthering work on computing machines. Babbage was ultimately forced to support his inventions largely through his own efforts and finances, devoting his entire life to developing a technology which was never to see completion--although heartily endorsed by all the "experts" of the day. With the exception of his son, H. P. Babbage, and his colleage and fellow mathematician, Lady Lovelace, Babbage worked almost entirely alone and without the support of a group of like-minded researchers with a rich body of shared knowledge and facilities for research at their disposal. Although the British government initially supported the undertaking, as the interests of political decision-makers varied over the course of succeeding administrations, that support eventually evaporated and work on the second difference engine and the analytical engine languished through years of sporadic and insufficient funding. (Dubbey, 1978, p. 174)

The British government invested altogether some L17,000 and Babbage himself invested L20,000 on the construction of the larger difference engine, which Babbage had presented to the government, until in 1833 he could no longer pay his workmen out of his own pocket. At this point the government engineer assigned to supervise theproject abandoned the work, dismissing the employees and taking with him the tools and equipment which had been specially constructed for the machine--thus effectively ending work on the difference engine at that moment. (Dubbey, 1978, pp. 185, 188, 193) Although Babbage apparently began working on the details of the analytical engine almost immediately thereafter, he received no final decision on the fate of his second difference engine until 1842, after 8 years' total inactivity on the project. In withdrawing support, Chancellor of the Exchequer, Henry Goulburn, argued for the government,

> "We both regret the necessity of abandoning the completion of a machine on which so much scientific ingenuity and labour have been bestowed. But on the other hand, the expense which would be necessary in order to render it either satisfactory to yourself, or generally useful, appears on the lowest calculation so far to exceed what we should be justified in incurring, that we consider ourselves as having no alternative. We trust that by withdrawing all claim on the part of the Government to the Machine as at present constructed, and by placing it at your entire disposal, we may, to a degree, assist your future exertions in the cause of science." (Dubbey, 1978, pp. 189-190)

According to A. deMorgan (quoted in Dubbey, 1978, p. 189) Sir Robert Peel joked in Parliament, "...that the machine should be used to calculate the time at which it would be of use."

In 1852 Benjamin Disraeli, then Chancellor of the Exchequer, again turned down government support for the project, arguing that

> "Mr. Babbage's projects appear to be so indefinitely expensive, the ultimate success so problematical, and the expenditure certainly so large and so utterly incapable of being calculated, that the Government would not be justified in taking upon itself any further liability." (Dubbey, 1978, p. 194)

The final blow came seven years after Babbage's death when the British Association, inquiring into the possibility of completing the work which had been begun, found that the machine was a marvel of mechanical ingenuity, resource and utility, and mathematical soundness. However, because it was considered impossible to determine a reasonable estimate of the cost of the machine, and because they were further of the opinion that completion of the machine was "not more than a theoretical possibility", they concluded that they could not advise the Association to procure the construction of the analytical engine or its printing tables. (Dubbey, 1978, p. 215)

Ironically, after disposing of the "useless" parts of the second difference engine by donating the working portion in 1843 to the Kings College Museum, the British Government ultimately purchased a difference engine built in Sweden by a printer named Scheutz and his son on the basis of an article describing Babbage's machine. This "new" difference engine, which was considerably simplified and which worked according to different mechanical principles, was awarded the gold medal at the 1855 Paris Exposition, and was acquired by the British government to compute a set of life tables according to "...mathematical principles worked out by Babbage nearly forty years earlier." (Dubbey, 1978, p. 192)

The work of Charles Babbage in transforming abstract formal methods and ideas into practical, functioning machines, exemplifies the methodology of modern systems engineering, all in one person. In his work, On the Economy of Machines and Manufactures, published in 1832, Babbage echoed the ideas of Adam Smith in advocating a "more perfect system" for the division of labor, which (like Smith) he considered to be the most influential factor in the economy of manufactures. Babbage argued that specialization of the worker to a single task brought with it the following benefits:

1) The time required for learning could be reduced to a minimum, as determined by the difficulty of execution of a single task or operation, with benefit both to the apprentice and to the master.

2) As each person limits his attention to one part of the total process, the waste in materials associated with the learning phase, as well as the waste in time incurred in changing from one occupation to another is eliminated. In its place, the apprentice and master both enjoy the benefits of greater focus of attention, and capacity for enduring fatigue which are associated with the establishment of habit in one occupation.

3) Economies are realized when one machine is kept continually employed at one type of work, in preference to changing tools for each job. Moreover, by focusing the worker's attention on a single process and tool, improvements are more likely to be suggested to his mind, and further, these improvements are the basis for incorporating these new routines into machine processes.

> "When each process, by which any article is produced, is the sole occupation of one individual, his whole attention being devoted to a very limited and simple operation, improvements in the form of his tools, or in the mode of using them, are much more likely to occur to his mind, than if it were distracted by a greater variety of circumstances. Such an improvement in the tool is generally the first step towards a machine."
> (Babbage, 1832, p. 173)

His investigations into the pin-making industry and the penny-post system anticipated modern techniques of systems analysis, and his conception of the complex processes and mechanical requirements necessary to produce a working computing machine, as well as his justification of the effort in terms of its potential savings in labor and expense are characteristic of the type of analytical investigation now considered the hallmark of modern operations research. (Dubbey, 1978, p. 179) However, Babbage's accomplishments were not received with a corresponding "practicality" on the part of decision-makers with the means to support an endeavor of this importance and magnitude. According to Dubbey,

> "When the industrial revolution was nearing its peak the Government showed itself to not be particularly interested in the advance of science or technology.... If only Babbage could have worked for IBM!"
> (Dubbey, 1978, p. 216)

It is illustrative to compare the developmental context for the invention of computing and calculating machines in Great Britain and the United States during the same period of history. During an era in which scientific work in Great Britain was largely an amateur pursuit, the fledgling U.S. government provided early support to systems engineering projects from the outset--bearing considerable cost overruns in the process--and ultimately ended up underwriting the cost of developing the first working computers nearly 100 years after Babbage's work had lapsed into neglect, and his personal fortunes into ruin.

In the United States, which Boorstin points out was still quite backward in many respects relative to 18th and 19th century European societies, "practicality" was the impetus to technological innovation from the beginning. The cash register was invented for the eminently practical objective of preventing pilfering by employees, and calculating machines which were made in America were practical devices built to serve merchants. "They were the work not of astronomers or mathematicians, but of mechanics." (Boorstin, 1973, p. 204)

The Hollerith machine, a punched-card device which was later important in the American development of computers, was invented by an engineer, Dr. Herman Hollerith, in order to classify and tabulate the data from the 1890 U.S. census, which would not otherwise have been compiled (according to Tomeski) until perhaps 1902-3. In this device, the census data was stored directly as coded perforations on the cards, which could then be rapidly manipulated by sorting or counting (tabulating). In striking contrast to the events associated with Babbage's computing machine, Dr. Hollerith later formed his own company to manufacture and sell his equipment, which firm eventually merged to form the original International Business Machines Corporation. His colleague at the Census Bureau, James Powers, also formed his own company to produce and sell this equipment, which company was eventually absorbed by Remington Rand Corporation and later by Sperry-Rand. (Tomeski, 1979, pp. 59-60)

Interestingly, the development of computers in the United States brings us full-circle in relating the building of automata to the administering of complex socio-technical systems projects, for in 20th century America, the early development of computers represented a joint endeavor between the academic community (where the inventors were scientists and mathematicians), the business community, and (with the entry of the U.S. into World War II) the military establishment. At Cambridge (MIT), Prof. Vannevar Bush applied his "Yankee ingenuity" to the construction of a differential analyzer, essentially an analog computer, still considered in the early 1940's to present equal potential with digital computers, which were exemplified by the MARK I.

The MARK I was developed by Harvard Professors Howard Aiken (USNR) and Theodore Brown (among others) under research grants from IBM and the U.S. Department of the Navy. (Tomeski, 1979, p. 61) This project culminated in the introduction of the MARK I--Automatic Sequence Controlled Computer--which was used to prepare mathematical tables for purposes of military planning and problem-solving during World War II. (Schmidt and Meyers, 1970, pp. 21-23; Birkhoff, 1981, pp. 21-24; Tomeski, 1979, p. 61)

The MARK I computer operated by means of mechanical components. This sequence controlled calculator was built on the basis of standard Hollerith counters, with a tape sequence control superimposed to direct the machine operations. In contrast, Babbage in his analytical engine had conceived a way in which instructions could be called and recalled conditionally and repeatedly on the basis of stored programs. (Dubbey, 1978, p. 216) A working model of an electronic computer which eliminated the necessity for physically-moving mechanical components was actually conceived in the 1930's and built by 1042 by Iowa State College mathematics professior, John Atanasoff. This work was extended by J. Prosper Eckert and John W. Mauchly at the Moore School of Engineering (University of Pennsylvania), who introduced the electronic computer, ENIAC, for military use in 1945, and the EDVAC for civilian use in 1952. (Tomeski, 1979, p. 62)

In 1946 Princeton mathematician John von Neumann developed the concept of a stored program logic, while working under the auspices of U.S. army ordnance. (Schmidt and Meyers, 1970, p. 23) von Neumann, who also suggested the use of binary in preference to decimal arithmetic, represented a link between the development of computing in the U.S. and Great Britain, and it is ironic but fitting that his stored program concept should first be incorporated into the EDSAC computer, developed at the University of Cambridge, England, in 1949. (Tomeski, 1979, p. 62)

The development of computers and computing in the 20th century has only recently begun to approach the sophistication of Babbage's remarkable achievements, however. According to Dubbey,

> "There is, unfortunately, no evidence that any of the early inventors of modern computers made use of Babbage's work or were even aware of its existence."
> (Dubbey, 1978, p. 216)

Howard Aiken's MARK I, begun in 1937 and completed in 1944, took twice as long to develop as the first difference engine and was "a very modest affair compared with the one that Babbage envisaged", consisting of a store of 72 counters, each holding 23 figures, in comparison with Babbage's 1000 variables of 50 figures each.

In comparing different developmental styles in instances of systems engineering methodologies-in-use, we have seen that one reason for differences in the context or path of development has to do with the relative importance attributed to practical objectives in a given socio-cultural environment, and the degree to which those objectives can be clearly articulated and agreed upon, thus providing the problem- or goal-orientation for processes of innovation and implementation.

A second factor underlying regularities in contextual patterns, or patterns of development, has to do with various constraints and opportunities which characterize different environments in which problems are identified and solved. At this point, the otherwise incidental influence of historical accident (again) becomes a significant factor upon which the development of systems engineering methodologies and the invention of the computer converge. Just as decline and conflict within ancient Greek civilization affected the manner in which formal reasoning and inquiry were brought to bear on practical affairs, and as the shifting interests of the British government officials who withheld support retarded the development of the first "analytical engine", so also the outcomes of World War II represented a major influence in directing the course of development and use of computer-based systems, in two aspects:

1) The invention and initial use of the computer emerged as an intermediate objective in the work of operations research groups, established within branches of the military both in England and in the U.S. to conduct research on military problems during the course of World War II. Operations research was a common ground upon which systems analytic methodologies came to be conventionally recognized, tested and accepted, and the context in which computers were finally developed as a viable technology. This movement brought together groups of many well-educated and well-funded scientists and mathematicians at separate locations--in the United States, Great Britain and Germany--who worked as teams to apply the methods and principles of their respective disciplines (primarily physiology, physics, and mathematics) to military problems in ballistics, logistics and human factors engineering the in the effort to win the war. The exigency of war

certainly satisfies Leithauser's argument that building a computer which would be more than a "toy" automaton would require an application which justified the cost of its construction. Birkhoff implies that without the influence of war, computers might not have developed as they did, if at all.

> "...(I)t was dedication to the struggle against Hitlerism, and later to other problems of national defense, that provided the main driving force behind the development of the computer in the 1940s. It's absolutely impossible to understand it except in that context."
> (Birkhoff, 1980, p. 23)

In the United States, those researchers already working on the development of computing machines continued their collaboration under the military auspices of the Office of Scientific Research and Development (OSRD) and the Joint New Weapons and Equipment Board (JNWEB), both headed by Prof. Vannevar Bush of MIT. (York and Greb, 1977, p. 14) In Great Britain, operations researchers worked to defend England from attacks of German aircraft, which required improving methods of analysis previously used to direct fire against military targets. This effort, together with that of a group of researchers working on code-cracking problems (including British computer "pioneers" Turing, Blatchley and Pask) provided the context of development for the use as well as the design and construction of computer technology.

Operations research groups thus provided a conducive context not only for the development of the hardware of computing, but also for the development of programming languages and applications which would make computers useful, and for the development as well of the associated skills which helped to spread, not just formal design principles and methods of analysis, but a common orientation as well to a logic of use, and to a form of social organization for inquiry which emerged in the course of their collaboration.

2) The context of war, however, also provided for the "historical accident" of the destruction of much of the material and economic capability of both Great Britain and Germany, a situation which operated to the advantage of the U.S. at the end of the 19th century as well. Birkhoff reminds us that only the United States and Canada emerged unscathed and triumphant "...in a world in which most advanced countries were prostrate and in ashes...." (Birkhoff, 1980, p. 29)

In Germany, scientists educated in the finest European traditions of mathematics and philosophy, and skilled in the use of precision technology, were struggling with a lost cause. In the development of computing which accompanied the operations research effort in Germany, Konrad Zuse (an engineer first working alone and later with the aid of the mathematician, Schreyer) was in the process of developing what would be perhaps the earliest and most powerful programming language at that time, the Plankalkul, which was in effect a mechanical instantiation of Hilbert's propositional and predicate calculus, in a formal system which can be considered the equivalent of modern logics. The exigencies of war destroyed Zuse's research site and sent him into comparative isolation in Switzerland--isolation certainly from the commercial development of computer technology, which effectively removed the support and exposure for Zuse's work until 1972. (Knuth and Pardo, in Metropolis, 1980, p. 202)

Great Britain also did not fare well at the end of World War II (as at the end of World War I). With much of its resources spent, its physical plant in various states of destruction, and its population in a relative state of deprivation, Britain did not have the economic luxury of commercially developing the knowledge gained in electronics and computing during the operations research movement into strong industries.

In the United States, in contrast, the cooperation of industry and government, characteristic of war efforts throughout American history, and the good fortune not to have suffered physical damage as a result of war, together with the advantage which derived from being the beneficiary of the "brain drain" which flowed from Europe, and especially from Germany, to universities and research sites in the United States, all were factors which combined to ensure the early commercial dominance of American firms producing computers--primarily IBM, which had collaborated in some initial research efforts from the beginning.

Consequently, it became customary to think of the U.S. as scientifically pre-eminent, and Birkhoff argues that most Americans came to think of computers as a national monopoly. (Birkhoff, 1980, p. 29) From the vantage point of our position in the future, it is easy to see how one might confuse the dominance of an idea supported by luck and commercial success with "universal scientific principles" in a developmental race in which the leader is in a position to define the terms of the game for his competitors by virtue of the advantage of setting precedents and capturing markets.

One consequence of that dominance and precedence which has been important for the course of development is that a characteristically American approach to the development of man-machine systems may have come to be embodied in the development of computer technology--an approach which may be culturally idiosyncratic, and contingent upon certain conditions which are characteristic of aspects of American history and of the American environment, but which may not be shared in other environments, nor perhaps in contemporary American society. An unexamined belief in the universality of one developmental approach to the introduction of computing in science and industry may obscure differences in alterantive modes of development, overlooking specific advantages and limitations which are influential in further development. The Achilles heel of the traditional American mode of industrial development is that it has made a virtue out of a real deficiency, which was the chronic lack of skilled labor during the early years--a deficiency which is no longer characteristic of American society. (Boorstin, 1973, p. 194)

The labor problem was "solved" so successfully in the "American System" of manufacture that the model of the productive system based upon the unskilled worker has become the paradigm (template, to use Drucker's term) for defining systems engineering as automation. This approach may be extremely unfortunate in the case of computing, as it imposes constraints on the range of conceivable problem solutions involving information management which may constitute a limiting factor in the resulting acceptability and reliability of computer-based systems in use. Thus it may be worthwhile to investigate in earlier (American) contexts the origins of some of these assumptions which constrain the development of computer applications today.

The American System of Manufacture:

Systems engineering was not new in what was to become the United States even in 1769. We can recognize in Wymore's description of the system design process just the sort of directed operations research which has characterized scientific investigations in industry and government for generations. In contrast with other countries (such as Japan) the manner in which the Industrial Revolution unfolded in the U.S. exhibited a marked tendency to use machinery in place of labor. (Cochran, 1977, pp. 45-46) Since the colonial era, that tendency has become institutionalized as a model of industrial development, which model exhibits a reciprocal tendency to preserve those conditions which initially were obstacles.

The Uniformity System (U-form) of manufacture, known outside the United States as the American System, is commonly attributed first to Eli Whitney (the inventor of the cotton gin, who called his model the Interchangeable System). This model of production relations describes a system which represents "organization" on two levels in the design of the process: in the technical-logical organization represented in the design for a product, and in the social organization represented in the process of manufacturing that product. As one of the more prominent of a group of industrial pioneers in colonial America, Whitney's special contribution was the early development of a method of social-technical organization which made possible the production of complex items of material culture without the corresponding craft skills which had been essential to their production in Europe.

The immediate problem which confronted the U.S. during the post-Revolutionary War era primarily concerned efforts to become self-sufficient in the manufacture of a number of critical items--especially firearms--which had previously been handcrafted in Europe and obtained by colonists who did not themselves have the skills to make them. The problem of insufficient skills was exaccerbated by restrictive emigration policies in England, which policies effectively prohibited "technology transfer" by preventing skilled technicians and craftsmen from migrating to the colonies. (Ashton, 1975, p. 86) During the Napoleonic Wars, France threatened war with the United States, which was essentially unarmed, since no firearms had been manufactured in any quantity in the U.S. up to that time, most having been brought from Europe, and especially from France.

A second special condition characteristic of the early American context of industrial development with reference to skills was the extreme social and geographic mobility of its population. It has been a frequent occurrence in the history of industrialization in the U.S. that (free) men continued to work in industry only until they had achieved a sufficient stake to become self-employed--the perennial "American Dream". Laborers continued to regard their status as temporary (particularly in the North), and in consequence loyalty for one's employer based on leadership or custom was not something that could be presumed as it generally was in more traditional societies; rather, "...respect was situational and temporary rather than institutionalized and lasting...." (Cochran, 1977, pp. 5-6) In responding to this condition, employers hired successive waves of unskilled immigrants, women and children in order to replace the continuing outflow of adult male laborers. Certain characteristics of the labor force specific to the American context followed from this adaptation: First, the American laboring population has reflected continuing flux from the earliest days to the present; moreover, this population has included women almost from the beginning. One corollary of the continual in-and-outflow of laborers has thus been that for generations there was very little continuity among the work force upon which a union movement could build, a factor which retarded any effective opposition to laissez-faire practices for over a century.

A third special characteristic of the American environment which added impetus to the development of new systems of manufacture combined this pressing need for skilled labor with a relative absence of significant social institutions which would tend either to constrain or to support alternative modes of production for satisfying basic "everyday" needs. Just as the United States lacked a skilled labor force, they also lacked guilds and unions and established systems of education--including institutions of apprenticeship, prisons and workhouses--which would have tended to supply those needed skills. In addition, the environment was similarly devoid of established institutions such as transportation, banking and credit and finance, civil service and the like--institutional arrangements which had constituted a "civilized" infrastructure upon which industrialization in Europe was built, and by which it was constrained. Cochran notes that "...nowhere was the social structure so favorable to rewarding business activity...." (Cochran, 1977, p. 5) America was, in a sense, a context-free environment for developing technologies of production, with no privileged elite or established interests to limit the range of development, and with (at that time) shallow "institutions" of government and law that were considered less "authorities" than "utilities". (Cochran, 1977, pp. 5-6) Over the course of time, unfortunately, the special advantages and disadvantages of this relative backwardness (characteristic of colonial governments in general) have come to be enshrined in American culture as the virtues of a laissez-faire economy and mode of production (which, of course, was very notably a fiction once those institutions and others were established.)

It was therefore under these special circumstances that in May, 1798, Eli Whitney offered his machinery, water power, and workmen to the government for the manufacture of firearms, which according to Boorstin, was "probably the first contract for mass production in the American manner." (Boorstin, 1973, p. 31)

Whitney's solution to the problem of manufacturing firearms was to conceive of a design for the technical organization of the production of weapons which 1) would replace the skills involved in the craft of the gunsmith, while still producing a reliable working product, by building those skills into the design specifications; and 2) would develop a system of production to carry out that process, given available labor and resources, by a microdivision of tasks based upon that design.

In Whitney's American System of Manufacture, the representation entailed in the description or design of the product orders the process for producing that product (firearms), as follows:

1) The Product: A complicated machine, such as a gun or a clock, could demonstrably be produced by breaking down its manufacture into the separate manufacture of each of its component parts, which could then be reproduced in large numbers, so that they were interchangeable in the construction of the finished complex machine, with the result that "if one piece broke, another of its type could be substituted without shaping or fitting." (Boorstin, 1973, p. 30)

A related aspect of this mode of product "design" or synthesis was the reconceptualization of the concept of "quality" of the output, which value was (with Whitney) identified with functionality and economy. In reproducing a design, Whitney eliminated all decorative and distinctive aspects from the product, in the interest of producing a reliable machine which would "work", and of producing it at the least cost in terms of available resources. The advantages of simplification were evident in the savings enjoyed by reserving the expense and difficulty of precision for the process of machining those parts which required sensitive tolerances, while leaving special finishing processes out of the design altogether, or building them into special machine tools and processes.

In a later era, Herbert Simon would elevate this accommodation to the normative concept of "satisficing". We may also note in passing that this adaptation provided the impetus for solving the second of Leithauser's prerequisites for developing a computing (or indeed, any) industry--namely the necessity of establishing a precision machine-tool industry, the early accomplishment of which enabled the United States to compete successfully with much older industries in Great Britain and Germany, and by 1900 to be well-established as a major economic power.

2) The Production System: The system of social organization involved in organizing the manufacture of complex "products" as the construction of interchangeable parts, correspondingly made possible the employment of interchangeable--and similarly simplified--human components in the manufacturing process. Whereas to build a complete complex machine required special skill and knowledge on the part of the craftsman, the repeated manufacture of merely one piece of that machine could be accomplished by a considerably less skilled human operator. Thus the attraction of what Whitney called the Interchangeable System lay in the fact that it was now possible to "...substitute correct and effective operations of machinery for that skill of the artist in such short supply...." (Boorstin, 1973, p. 30)

Whitney's system is representative of the form of social organization which emerged in the early stages of the Industrial Revolution, especially in Great Britain. As described by Faunce, the relation of man to machine (or, alternatively, the role of the human component) differs at each stage of technological development in the production process. In handicraft-type production, the worker is a skilled craftsman, working with specialized tools and/or his own imagination. In mechanized production, the person becomes an operator of a special-purpose machine, which provides power and standardized increments of production within a bounded range of variation. In automated production (still largely abstract) the function of the human operator is only to monitor the operation of the system; the machine actually does the work, while the person spots problems. (Faunce, 1968, p.)

Differences in these forms--and particularly the transformation from handicraft to mechanized production--were grounds for serious conflicts with workers during the initial stages of the Industrial Revolution in Europe, where craftsmen in England and France attempted to destroy new technology as it was introduced. In Manchester, England, the "luddites" directed their attack on the technology to the machines themselves; in Paris, the developer of the first commercial sewing machine (M. Thimmonier) was "run out of town" by irate Parisian tailors. ( ) Since that time, it has become traditional to think of technological change as accompanied by conflict and resistance, as the term "luddite" has come to be generalized to persons who are critical--and presumably hostile to--any new technology.

By contrast, the United States was relatively immune in this first stage of conflict because of its "fortuitous" backwardness and the absence of any skilled workers with interests to protect. It was much easier to implement both the specialization of (unskilled) labor and the simplification of the product entailed in the American System in the absence of resistance from skilled craftsmen and guilds which would have objected both to the downgrading of their skills and to the "cheapening" of the object of their craft. It may be that the relative absence of any effective resistance to this model of development in the early stages underlies the stability and continuity of this (designer-based, American) orientation as represented in recent schools of management science, human factors engineering and artificial intelligence, all of which can be identified with a model of the organization as a servo-mechanism, or finite state machine.

Whitney's accomplishment, therefore, not because of the novelty of his ideas, for as we have seen already, the microdivision and specialization of labor was a common theme already translated into practice in the early days of the Industrial Revolution, and the concept had been clearly articulated by Adam Smith (among others) during the latter part of the 18th century. Babbage himself quotes Smith as attributing increases in industrial productivity to this system of dividing labor.

> "The great increase in the quantity of work, which, in consequence of the division of labour, the same number of people are capable of performing, is owing to three different circumstances: first to the increase of dexterity in every particular workman; secondly, to the saving of time, which is commonly lost in passing from one species of work to another; and lastly, to the invention of a great number of machines which facilitate and abridge labour, and enable one man to do the work of many."
>
> (Smith, quoted by Babbage, 1832, p. 175)

What is significant is that, under contract to (what would be) the state, the American System of Manufacture instantiated the conventional industrial practices of the day in a model which was reproduced in the work of Moody and Taylor, and in Henry Ford's revolutionary assembly line methods of production, later adopted in Europe, and especially in Great Britain, during World War I. Moreover, this model of development in the practice of systems engineering is representative of a long-standing relationship in the United States between government, private industry, and the military, which has had significant influence on the development of new industries--based upon technological innovation and development--since colonial times.

Whitney's system may be termed a "designer-based" system, meaning that the orientation in the process of design and development is to the capacity and reliability of the machine system (hardware). The design of the equipment represents the primary objective orientation, to which specific objectives-for-use ("usage") are subsequently applied through a secondary process focusing on the development of applications. Implementation then involves "tailoring" those applications, and perhaps re-configuring the equipment and/or programming, for a specific environment of use. In a designer-oriented "system design system" (to use Wymore's term) the focus of development is directed to designing a technical system to which people are later (somehow) adapted--which is the function of management.

An implicit (or often explicit) objective in designer-based systems is that of eliminating the human component--with the logical "end" being a fully automated system. The specification of the machine-based process thus represents a specification of the minimum contribution required of the human operator, which person is "fitted" to the system by selection, by training and supervision, and by "process" controls, including pacing. These concepts are by now quite traditional in British and American manufacturing methods, and this style can be seen in the development of computer technology-in-use since the 1940's--a century after Babbage and Smith were writing.

We have seen that the development of technology--and computer technology, specifically--takes place within some historical context, and cannot be separated from considerations of use and practicality. Moreover, although the concepts for understanding the use of machines and the micro-division of human labor in industrial production are essentially unchanged in Britain and America, from the 18th century to the present, the historical context presented by American institutions and represented in American history reflects certain characteristics not shared in European cultures. Thus, although a long intellectual tradition in abstract reasoning and in formal languages and methods of inquiry may have favored Europe in the invention of computing machines, the commercial advantages enjoyed in the United States, together with its relative absence of significant contextual constraints, have tended to favor the development of computing in a designer-based orientation to system development, which conceives of computerization _as_ automation, consistent with Whitney's model of the Interchangeable System.

This orientation may have greatly benefitted the design and manufacture of computers, but has been a relative handicap in the development of computing--or the _use_ of computers--rendering problematic the implementation and use of computer-based technologies in ongoing environments. In a designer-based orientation the development of applications--or uses--for the technology is problematic because such a general-purpose machine as a computer is itself too complex and flexible for one to argue meaningfully that the definition of use is implied in its design, as one could, for example, with the cotton gin. This may be a major reason for the narrow transactions-processing path of computer development taken to date, a path which exhibits a tendency to borrow--and thus to perpetuate--conventionally available methods of analysis and design, and traditional concepts of organization and management in the development of new technologies.

What we have been referring to as "developmental paths" can be expressed within the terminology of systems analysis in the concept of "technological style", which W. A. Sedelow defines as the manner in which particular aspects of material culture come to be instantiated in specific ways of thinking and symbolizing. (Sedelow, "Algorithm", p. 7) In comparing different developmental styles in specific historical instances of systems engineering and the development of computing machines, one reason for the differences in the context of development, we have argued, has to do with the relative importance attributed to practical objectives in a given socio-cultural environment, and the degree to which those objectives can be articulated and agreed upon, thus providing the problem- or goal-orientation for processes of innovation and implementation. A second factor underlying contextual patterns, or patterns of development, concerns various constraints and opportunities (the perception of alternative possibilities) which characterize different environments in which problems are identified and solved. We may identify as a third factor determining the nature of the context of development that knowledge which is available to people in those environments for understanding and acting upon the "external world" in the interests of fulfilling their objectives. This requisite knowledge--which we might characterize in terms of "style"--is an aspect of the context of development which is especially critical in the development of computer applications--i.e., in translating formal models into use.

We will argue at this point (with Birkhoff) that computers would not have been developed--and still may not be fully developed-in-use--in the absence of a favorable context in which those factors come together to satisfy the prerequisites for technological development:

1) The ability to fully work out the technical--mechanical and electronic--specifications required to build a working model. The translation between the concept of a finished product and the internal components and their configuration must be completely defined--i.e., represented in an "effective procedure"--in order for the machine to work reliably.

2) A sponsor with sufficient resources and organizational flexibility to absorb development costs without immediate and visible return on investment, and to adapt organizational procedures and methods of communication to the exigencies of the development process.

These two prerequisites were satisfied in exemplary fashion in the context created by the Operations Research movement during World War II.

As a context for the development of computers in military research during the war, operations research groups provided for the widespread development and dissemination of the systems approach and methods of quantitative analysis which, as in the early stages of industrialization, also supplied on-the-job education of designers and technicians familiar with new technologies as they were being generated. Further, the use of systems analytic methods of inquiry led to the formalization of systems concepts which was necessary to the development of computer hardware, languages and methods of use. Finally, in the context of the operations research movement, the use of analytic methods of problem-solving became associated with a form of social organization for investigation and collaboration which was distinct from traditional organizational hierarchies--different, most notably from the type of production system developed by Eli Whitney and those American designer-industrialists who followed him.

Operations Research:

The Operations Research movement represented a significant watershed in organizational research and development. Work undertaken on a set of concrete military problems by teams of scientists drawn from universities and coordinated by a new set of governmental authorities resulted in a formalization and standardization of an approach to scientific inquiry we have identified as Systems Analytic Methodology. As we argued in Chapter 1, this methodology implies a qualitatively distinct concept of the logic of scientific investigation from the conventional constellation of inductive empiricist methods and general systems theories, and it also implies a structurally different type of social organization for inquiry and production than conventional organization theories. Thus the transformation is both technological and social, and the contributions of Operations Research in World War II are as follows:

A. Formalization of the Concept of the Abstract System

Operations research groups embodied the methodology of systems analysis in the manner in which inquiry was organized according to theoretical concepts chosen for their relevance to specific practical problems, with prediction carried out according to mathematical and logical methods of classification, quantification, and analysis. This movement was instrumental in formalizing the concept of the "abstract system" and the mathematical theory of feedback, as expressed in Norbert Wiener's Cybernetics. There is a mutual relation between the use of these formal methods in complex problem-solving and the development of computers, and we will argue that those areas in which methods of quantification and analysis in operations research remain(ed) within the inductive empiricist paradigm are the areas which are presently inhibiting the further development of higher level computer languages and applications.

B. Institutionalization of Operations Research as an Abstract Technology

The operations research movement also embodied the methodology of systems analysis in the sense Wymore identifies as a system design system through networks of interaction, both in interdisciplinary teams and in networks of association among individuals holding offices in different cooperating institutions. In this instantiation of the systems analytic methodology in systems development and management, the concepts and methods of analysis of abstract systems are extended to the process of analysis itself, with direct implications for decision-making and social organization for inquiry and production. The social organization of operations research reflects the same type of multi-disciplinary project-team approach to problem-solving advocated by Wymore for modelling and testing of complex engineered systems. The "team" concept has been widely invoked in computer system development and implementation, and is a central concept in contingency theories of organization, as well as very early management science in the U.S.

There are significant differences in contingency theories in Great Britain and in the U.S., however, and we will argue that those areas of organization management where traditional normative concepts of corporate hierarchy are preserved are influential in inhibiting the further development of more analytically powerful computer applications-in-use, and in creating problems in the implementation of computer technology in ongoing systems.

C. Transformation of Human Factors Engineering into Ergonomics

The methodology of operations research illuminated the irreversible complexity of modern science in warfare--and, indeed, industry in general--a complexity brought about partly because of the interconnections involved in "real-world" research, and partly because the scope and complexity of the problems operations researchers were facing necessitated developing and using formal methods and instruments of analysis, which in turn added to the complexity of the situation for analysis and control. This complexity--and the corresponding uncertainty of finding effective solutions which it entails--were most clearly exhibited in a transformation of traditional concepts of the "human factor" in industry, associated with systems engineering which focuses on equipment design with performance elicited by training and leadership, to a focus of research attention on adaptive systems design with a model of human capabilities and limitations at its center.

Controversies over the "role" of the "human factor" implied in task definition and organization of work in computer-based systems are now extending the scope of conventional human factors engineering research beyond its traditional man-machine focus to questions of social organization and organization design. We will argue that to the extent that current research in human factors in computers is oriented to conventional presumptively hierarchical theories and to conventional methods of quantification and analysis in inductive empiricist research programmes, the orientation of human factors research will be limited to a scope too narrow to account for social and cognitive problems associated with computer use--and particularly for stress and its related physiological effects. Human factors research within a management science tradition, moreover, is certainly too narrow to account for phenomena of structural change in organizations, and under conditions of conflict and change, this type of human factors research may actually contribute to social unrest and conflict within organizations undergoing computerization.

A. Operations Research as "Science in War":

Whereas for Whitney the problem for research was to reproduce an item of material technology that was known, the problem for the operations researchers during World War II was to produce a range of items of material technology as well as new techniques and methods of use which would successfully defend against other items of weapons technology and other strategies which were, to them, unknown. Operations researchers in the various services encountered problems for investigation that were more complex than available methods for solving them, and in the course of developing solutions, they necessarily developed new methods for carrying out research in complex and uncertain environments.

Both the airplane and the submarine had been developed toward the end of World War I, and thus were still virtually experimental and untested in combat at the onset of World War II. Procedures and tactics had not yet been worked out as World War II began, and in addition, equipment and ordnance left over from World War I was largely obsolete two decades later. Depth charges used in World War I were ineffective against modern submarines, for example, and it was known that the Germans were developing aircraft and missiles which were capable of speeds which exceeded the ability of all available means of detection and interception. Problems in air defense, in submarine warfare, and in coordinating air, ground and undersea operations required information concerning the range and effectiveness of available technologies. However, according to Baxter, in 1939 there was no adequate scientific data on record in any Air Force on the effects of bombing, and modern antisubmarine equipment was relatively unfamiliar to naval officers who would be using it. (Baxter, 1946, pp. 34, 40; Morison, 1972, p. 199)

In addition to the lack of information there were problems in communicating the information that was available and in coordinating the diverse and complex contributions made by the various services in different environments. There were problems in air-sea and air-ground coordination; there were problems in handling the volume and complexity of information transmitted to ships by radar, sonar, radio and telephone; and there were difficulties in coordinating newly established radar installations along the British coast with antiaircraft batteries and fighter aircraft. (Baxter, 1946, pp. 85, 40)

Complex technical problems such as these and others under the extreme pressure of time led to the introduction of civilian scientists into military service as scientists, and to the extension of the methods of scientific inquiry into the realm of military operational research. Exemplifying the type of complex problem encountered in this research was the development of radar defense against air attack, especially German V1 and V2 rockets, which were long-range missiles weighing 14 tons and carrying a 1-ton payload. They were relatively accurate, with an average error of 4 miles out of 200, were immune to jamming, and worst of all, travelled in excess of 3,400 mph, which was too fast to be intercepted by airplanes or antiaircraft fire. Intelligence received in 1943 indicated that the Germans were preparing to launch these missiles against London and ports in the south of England. Great Britain had already been subject to air attacks since 1940, and research on rocket-bombs had proceeded from the study of pieced-together fragments of the missiles and information gathered from prisoners of war, carried out by a subcommittee of the British Minister of Aircraft Production. In cooperation with the National Defense Research Council in the U.S., mock-up models of the rockets were subsequently constructed and tested by researchers from the Naval Research Laboratory and the Airborne Instruments Laboratory working at facilities at the University of New Mexico. (Baxter, 1946, pp. 34, 234)

The problem of defense against these missiles could not be conceived from within the scope of current methods. It was initially thought that shooting down such missiles would require the skill of the gunner's eye. However, accurately intercepting them was not possible given current methods because the speed of the aircraft exceeded classical methods for calculating the line of fire, and because the velocity of the missile was a value added to the velocity of the missile used to shoot it down. This meant it was no longer sufficient merely to aim and fire, but it was now necessary to calculate in advance the position at which the target and the missile would meet, which required predicting the future position of the aircraft. The range tables which had been used to compute firings during previous wars were not adequate to the task of computing the values for shooting down rapidly moving targets in this manner, and extrapolation from the present course was insufficient because aircraft flying under fire do not fly a straight course--which was one of the central assumptions of earlier methods of calculation. (Wiener, 1948, pp. 6-7; Birkhoff, 1980, p. 23)

According to Baxter, it was the complexity of problems of air defense which brought civilian scientists into strategic planning at the highest levels, and the solutions they devised were qualitatively different from customary practice.

> "...(B)efore the study was completed the solution reached was to rely as little as possible on the gunner's skill of hand and eye, and instead to trust the accuracy of the new devices which had been developed for antiaircraft fire... the SCR-584 radar, the M-9 electric predictor, and the radio proximity fuze...used in combination." (Baxter, 1946, pp. 36, 234)

Out of the efforts of these scientists a radar warning system was developed which enabled British air forces to detect and defend against incoming attacks before they were destroyed piecemeal on the ground, indeed, enabling them to locate aircraft and missiles as they were launched in France and Belgium. (Baxter, 1946, p. 5) Cooperative work, undertaken in a "fever heat" succeeded in delivering the first proximity fuzes to England three months before the rockets were launched. During the V-1 attacks these devices and the methods for using them were remarkably effective in intercepting the missiles; in the last weeks of the attacks in 1944 the success rate improved from 24% of the targets destroyed in the first week, to 46%, 67%, and 79%, respectively in succeeding weeks. Of 104 V-1's detected by early warning radar on the last significant day of the attacks, only 4 reached London. (Baxter, 1946, pp. 234-5)

In the course of their investigations, researchers assigned to the problems of air defense noticed that a pilot does not have a completely free chance to maneuver at his will; the motion of the plane was subject to certain constraints which could be determined. Thus the problem was reconceived to involve--not straight-line prediction--but curvilinear prediction based on operations on its own past values, given those observed constraints. This concept is central to the idea of feedback and control--which is the basis on which the new radar detection equipment operated and on which the fuzes were triggered. The formalization of the mathematical theory of feedback is one of the factors which makes modern systems engineering different from "informal systems engineering" as practiced by Whitney and other early industrialists. As Wiener notes, this idea itself was one of the significant products of the operations research effort. (Wiener, 1948, pp. 6-7)

Feedback

As the key concept emerging from the complex endeavor represented in the operations research movement, feedback stands for the essential principle underlying all servomechanisms. (Weinberg and Weinberg, 1979, p. 193) According to Mayr, the theoretical study of feedback control came late in the development of science and technology, the term "feedback" itself having its origin in radio research at the beginning of the 20th century. The concept of feedback control emerged when biologists and economists in the 1930's noticed parallels between the phenomena they were studying and the control devices in engineering disciplines, and realized that the concept itself, independent from considerations of any specific type of hardware, was potentially a "versatile and powerful tool for investigating many forms of dynamic behavior". The formal study of feedback is even more recent, attributed to Norbert Wiener in the 1940's (Wiener himself locating the origin of the concept in the radar research of the operations research groups during World War II.)

The purpose of a feedback control system is self-regulation--or carrying out some command automatically. A feedback control system, then, is defined as a system which maintains a desired relationship between one system variable--the controlled variable, or output signal--and another system variable--the command variable, or input--by comparing the values of each and using the difference between the two as an indicator for processes of control built into the device. Thus the controlled variable may be maintained at some level "...in spite of interference by any unpredictable disturbance." (Mayr, 1970, p. 45)

Feedback devices are to be found as early as the third century, with the invention of a water clock which maintained a constant rate of flow of water into a tank on which an indicator measured the passage of time by the level of the water in the tank. Other types of early feedback devices included the thermostat and mechanisms for controlling windmills. A version of the water clock was constructed by Hero of Alexandria who, true to form, used it as the mechanism of a wine dispensing vessel. In the 9th century the float mechanism principle was refined somewhat and presented in a book entitled (in Arabic) On Ingenious Mechanisms. The float valve was a widespread innovation in Islamic technology up to the 13th century, at which point all references to float valves, or devices for water-level (or indeed any feedback) regulation disappear in Europe, and do not reappear until the 18th century, and then in England. (Mayr, 1970, pp. 47-49)

The first use of the feedback principle for temperature regulation came in the 17th century, and is attributed to the Dutch engineer, Cornelis Drebbel, who lived and worked in England. The float-valve principle was first adopted (as reflected in patent applications) as a water-level regulator in steam boilers by the British canal and bridge builder, James Brindley in 1758. Finally, feedback mechanisms were invented in the 18th century for the automatic control of windmills in England and Scotland, the first such device the fantail, developed as a rudimentary servomechanism designed to keep windmills facing into the wind, an innovation attributed to Edmund Lee in 1745. Another device used by millwrights to prevent millstones from moving apart with increasing speed of rotation was the lift-tenter, which was developed into a speed control mechanism making use of a centrifugal pendulum by British millwright Thomas Mead in 1787. This idea became important in the "invention" of James Watt's revolutionary steam engine; and it is James Watt's governor, based upon this concept of a centrifugal speed regulator, which has entered the textbooks as the paradigm example of feedback for later generations. (Mayr, 1970, pp. 50-52)

The concept of an "abstract machine theory" by which to model the behavior of cybernetic systems is less than 45 years old, generally following from the work of Turing and von Neumann, and dating to the World War II era. (Sedelow, "Algorithm", p. 7) A servomechanism can be defined as an abstract machine in which, as Ashby insists, the issue of "materiality" is irrelevant. What is important is that the system behave in a regulsr and law-like manner (which is what is exhibited in these ancient devices). The reason why a machine behaves in a law-like manner is due to the action of feedback mechanisms. Wiener defines the feedback mechanism formally as a chain of information transmission and return which ensures "effective action on the outer world" by measuring the performance of those components of the system which act upon the world and then returning information concerning that performance to the system as additional input used in regulating performance to "produce a properly proportioned output." (Wiener, 1948, pp. 5-7)

In the formal theory of feedback, an abstract machine or system is defined as a mathematical entity--or model--and the process of developing and analysing that model, and of drawing inferences and conclusions on the basis of assumptions input to it can be identified as an abstract technology--a set of methods for performing analytic investigations. The methods most widely used yet, and those used by the operations researchers, were based on mathematical modelling of linear systems by a set of differential equations, in which the mathematical model itself is an abstract system.

The concept of an "abstract machine" defined in terms of its behavior, together with the concept of feedback which defines the output of the system as a linear function of its input, was tied to the methods of mathematical modelling by the observation of operations researchers and early system theorists that the behavior of otherwise different systems appeared to be subject to the same "law" over time, and that this "law" could be expressed in terms of ordinary differential equations which they recognized in elementary laws of circuitry, mechanics, engineering and biology. In all of these disciplines, linear difference and differential equations are a common mathematical form (the calculus as the conventional paradigm) which is used to relate continuous behavior or the configuration of a set of variables over time, a form which lends itself well to modelling of dynamic operations. According to Thom, the solution of such differential equations constitutes the conventional paradigm for "scientific determinism". (Thom, 1975, p. 4)

The use of these equations assumes that the rate of change in one variable is a linear function of the rate of change in a second variable over time, which leads to concentrations that can be measured. A variable is a "meter that is capable of registering a reading"--i.e., a measure. Input and output variables are conventionally defined as "causes" and "effects", respectively, by Zeigler (1976, p. 29) and as "independent" and "dependent" variables, respectively by Starr (1971, p. 68), thus demonstrating the relationship of these concepts to conventional definitions and methods of scientific inquiry.

The linearity in this model means that the growth in the concentrations is proportional, that is, that the rate of change depends exclusively on the size of the other variable. Modelling a system using this technique focuses on representing that system as a population defined in terms of one variable by which to characterize a large number of otherwise undifferentiated individuals. The state of the population--or aggregate--is then described by a value on that single variable, which value represents the number of individuals in the aggregate at that moment in time. This makes it possible to transform the system equation, $S_{t+1} = f(S_t, I_t)$ into the familiar form, which becomes $N_{t+1} = f(N_t, I_t)$.

Thus the number of individuals in the aggregate at any given time depends on the number present at the previous time, and our interest is directed to finding out what influences the number of new and surviving individuals in the aggregate over time. If we can identify the number of inputs and outputs to the system during a given time interval, then we can predict the number of individuals in the aggregate in the next time interval by solving the equation, $N_{t+1} = N_t + I_t - O_t$ which, when expressed as the equation $N_{t+1} - N_t = I_t - O_t$ means that the left side of the equation represents the difference in the state of the aggregate during one time period, and this equation can further be transformed to describe, not the state (or the number in the aggregate at some moment in time) but the rate at which the system is changing (either growing or declining) in each time period expressed as a standard differential equation, $\frac{dN}{dt} = I = 0$.

These equations represent exponential growth, or concentration, on the variables of interest, which when represented graphically, display the feedback relations as a series of exponential curves, representing the state of the system in terms of the size or value on a given variable. The growth--or the process--is represented as a change of state over time, expressed as a rate. In a first order exponential equation, the rate of increase is proportional to the size; as the size gets bigger, the rate of increase also increases, accounting for the slope of the curve. In accellerated, or second-order exponential growth, some fraction of the existing system is added in each interval, and that fraction is itself increasing; or perhaps, the size of each unit is increasing simultaneously with increasing numbers, as for example, in increasing demand. Finally, if we add two second-order equations representing exponential growth processes we will get a third-order system which grows with great acceleration at the beginning and which deteriorates very rapidly at the end.

In cybernetic modelling, using linear equations, the following expression represents a solution to the difference equation

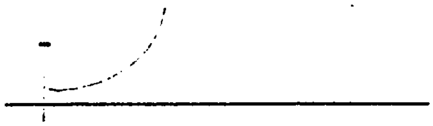
$$z_{n+1} = mkz_n + kx_n$$

which when given the initial state of a system, and an initial input will determine output at time n by remembering the output from the previous time,

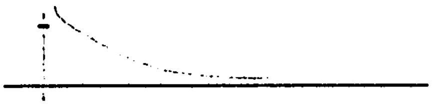
$$z_n = (mk)^n z_0 + k[\sum_{i=0}^{n-1} x_i \, (mk)^{n-i-1}]$$

These equations can be represented graphically to display the behavior of the aggregate over time as the "solution" of the differential equation. (Weinberg and Weinberg, 1979, pp. 17-22) Each of the curves displayed below represents one characteristic form of dynamic behavior of some system over time--growth, decline and stasis.

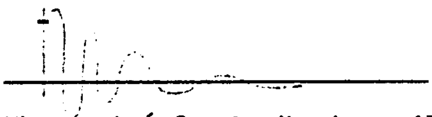
1. Where $mk > 1$, feedback increases exponentially to infinity.



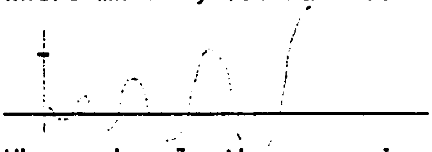
2. Where $mk < 1$, $mk > 0$ (ex., $mk = \frac{1}{2}$) feedback decreases exponentially, but does not reach 0.



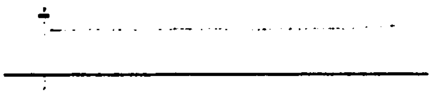
3. Where $-1 < mk < 0$ (ex., $mk = -\frac{1}{2}$), feedback oscillates exponentially to 0.



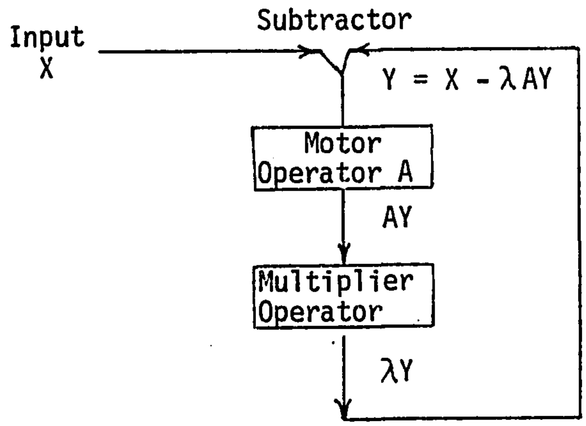
4. Where $mk < -1$, feedback oscillates exponentially to infinity.



5. Where $mk = 1$, the curve is a constant function.



In controlling some process according to the principle of feedback, as exemplified by James Watt's steam engine governor, the procedure is to measure the difference between the desired pattern and the motion which is actually performed, which difference is input to the system in regulating the process. Given a pattern which we desire some system to follow, feedback involves noticing the difference between that desired pattern and the actually performed motion, which difference is then measured and the measurement returned to the system as additional information used to regulate the motion in such a manner as to more closely approximate the desired pattern, according to the following diagram:



(Wiener, 1948, p. 100-102)

Based on this diagram, the operation of feedback can be expressed as a relationship between the input and a controller such that

$$Y = X - \lambda AY$$

which is equivalent to $Y = \frac{X}{1+\lambda A}$.

The motor output is $AY = \frac{A}{1+\lambda A}$

Thus, the "operator produced by the whole feedback mechanism" as defined by Wiener is $= \frac{A}{1+\lambda A}$, which will be infinite only when $A = -1/\lambda$.

Not all feedbacks lead to an increase in stability. A system will generate negative or positive feedbacks depending on the cumulative direction of the relationships expressed between the components, calculated for the system as a whole. If the influences expressed in these relationships tend to be in the same direction, where an increase in one component is accompanied by an increase in the value of the other, we can speak of positive feedback occurring. When an increase in one value is met by a decrease in the value of the second variable, then negative feedback is operating, which we commonly associate with control or regulation. (Weinberg and Weinberg, 1979, p. 199) Thus the operation of feedback can be destabilizing as well as stabilizing for the system as a whole. According to Wiener, if the operator produced by the feedback mechanism is $(A/1 + \lambda A)$, then any feedback with a multiplier of λ operating on any interior point of a system (defined as a mathematical entity) will produce a "catastrophe", defined as "unrestrained and increasing oscillation (to infinity), Figure #4. Where the point $-1/\lambda$ is represented as exterior to the system, feedback will remain stable, in spite of such oscillation. (Wiener, 1948, p. 103)

Positive feedback increases the difference or deviation of the outputs from some expression of a "steady state" by altering the variables defining that state and thus transforming it. Positive feedback initiates system change which, according to Miller, can ultimately be sufficient to destroy the system. Negative feedback, conversely, functions to maintain steady states by opposing or cancelling error detected in the output. (Miller, 1978, p. 36)

In practice, the concept of feedback is interpreted as optimizing some output value defined for the system, or maintaining the stability of the system itself. Feedback relations rely on two channels to carry information to the system--the process itself, and a feedback channel which functions as a monitor on the process, identifying the results of previous actions and returning that information as part of a new sequence of action, in this way reducing the strain which results from error experienced in the process.

Among feedback relations, there are those, such as Maxwell's steam engine governor, in which no human element intervenes, and in those systems negative feedbacks provide stability by opposing what the system is already doing, measured in terms of velocity, position or motion. Maxwell's dynamical theory of governors meant that the machine could regulate its own velocity and maintain that velocity within its own threshhold of capacity regardless of variations in power or load. (Wiener, 1948, p. 9)

According to Miller, "...a living system is self-regulating because in it input not only affects output, but output often adjusts input." (Miller, 1978, p. 36) A living system is an adaptive system. If what counts in defining stability is not the physical appearance of systems, but the endurance of the relationships, then according to the Weinbergs the process of regulation ensures the stability of the components in the system, and conversely, it is this stability that makes regulation possible. (Weinberg and Weinberg, 1979, p. 157) A feedback device which measures the amount of output and monitors it back to regulate the system is thus a controller or governor, by which we can describe output as a linear expression of input, which is the basis for Cybernetics, as the study of control and communication in systems--in other words, the study of just those methods of feedback involving the transmission of energy and information, and defined as processes of control. (Wiener, 1948, p. 11)

This formal theory of feedback made it possible to represent complex practical problems as abstract systems operating in time. At the same time, however, the complexity of this type of analysis made it necessary to develop computing devices, and it was this computational load that was leading Vannevar Bush and others to attempt to build machines to compute the complex equations representing real-world research problems--hence the concommitant development of computers in the context of radar research as part of the operations research movement.

B. Organization for Problem-Solving:

Operations research activities were first undertaken in Great Britain in the early stages of the war, and cooperative relationships were soon established with the committees which were being newly established in the U.S. to deal with similar problems of military research of the complexity just described. Operations research units arose out of the need to develop a coordinated scientific effort in military service which would attract sufficient numbers of well-trained specialists, and provide them with ample funds and a large measure of freedom to attack problems of practical importance.

Network Organization for Science in War: Prior to World War II, departments of government and military were not organized in such a way as to make the most efficient use of scientists in developing new weapons and strategies. Existing organizational structures within the military operated exclusively on the principle of the chain of command, under conditions suited to routine conduct of military operations and incremental improvement of standard equipment. Procurement standards and schedules were based upon such routine operations and departments were, therefore, evaluated in terms of production standards. In addition, established service laboratories in Great Britain and the U.S.--such as the Naval Research Laboratory, founded in 1923--were overwhelmed by demands for routine testing which arose in wartime, thus diverting them from their responsibilities for basic research. (Baxter, 1946, pp. 7-8)

We have seen, however, that the type of problems encountered during World War II did not led themselves to this incremental approach based on an extension of customary practices. As Baxter notes, basic scientific research and development cannot be standardized because of its uncertain nature, and (as Bush had noted) it is dependent upon a degree of freedom from performance standards and production controls which inhibit the development of new principles and solutions and result in jealousy among departments competing for appropriations. Bush argued in testimony before the 78th Congress in 1945,

> "This may be particularly serious when we remember that modern weapons may either draw their components from or be, at least in part, the responsibility of several competing procurement units--each of which is in a position to retard or advance the progress of the other."
> (Bush, quoted in Baxter, 1946, p. 12)

Thus there was pressure to reorganize scientific research within the military establishment in order to mobilize civilian scientists to the war effort, and to create an organizational context which would better 1) support recruitment and assignment of scarce talent and "create the conditions under which the task could best be performed"; 2) facilitate teamwork by creating effective liaison among the armed services and between the services and academic and industrial contractors; and 3) provide necessary information with a minimum of delay to scientists and military personnel working in these different environments.

The type of organization which emerged was initiated in the U.S. with the establishment of the National Defense Research Committee, created by act of congress in May, 1940, and charged to

> "...(C)oordinate, supervise and conduct scientific research on the problems underlying the development, production, and use of mechanisms and devices of warfare, except scientific research on the problems of flight."
> (Baxter, 1946, p. 14)

Scientific research on the problems of flight was referred to the Committee on Medical Research (CMR), one of the major divisions of the later version of the NDRC, the OSRD.

Membership in the NDRC was to be appointed by the President of the United States and was to include up to 12 members, with two each from the departments of War and the Navy and from the National Academy of Sciences. The original members of this committee included its first chairman, Vannevar Bush, Prof. of Electrical Engineering and former President of MIT, inventor of the prototype differential analyzer.

As an appointed member of the National Advisory Committee for Aeronautics (NACA) since 1938 and its President in 1939, and a member of the Committee on Scientific Aids to Learning, Bush enjoyed a network of professional relations on which to rely for making up the membership of the committees of the NDRC as well as a model for the organization of such committees in the workings of the NACA.

NACA, established in 1915, was staffed by civil service employees and had its own laboratory facilities; in addition, this committee had the authority to contract with educational institutions for consulting and research projects under joint military authority. An important function of the newly established NDRC was also to "...contract with educational institutions, individuals, and industrial organizations for scientific studies and reports." (Baxter, 1946, p. 14)

It is consistent with this model that other original NDRC committee members included Karl T. Compton, who had also been President of MIT; James B. Conant, who had been President of Harvard in 1939, and was also a member of the Committee on Scientific Aids to Learning; Frank B. Jewett, President of Bell Telephone Laboratories and the National Academy of Sciences, and a fellow member of the Committee on Scientific Aids; and John Victory, Secretary of NACA. The interconnections in these committee organizations made the facilities and personnel of a number of established institutions available across traditional organizational boundaries, and provided numerous opportunities for interaction and cooperation between the military and the scientific community in the U.S.

The establishment of the NDRC also provided the framework for developing cooperative relationships with researchers in Great Britain and Canada. Two British scientific missions were sent to the U.S. to establish reciprocal interchange of scientific information and service experience, the first headed by Cambridge University Professor and recipient of the Nobel Prize in Physiology and Medicine in 1922, Archibald Vivian Hill, who was also a member of the British Parliament and Secretary of the Royal Society. Hill enjoyed a position similar to that of Prof. Bush in the U.S., enabling him to lay the groundwork for the second, "Tizard" mission, sent after the fall of France.

This second exchange was headed by Sir Henry Tizard, Rector of the Imperial College of Science and Technology and Scientific Advisor to the Ministry of Aircraft Production, and included members of the British armed forces and the Canadian defense services and the Canadian National Research Council. As an official mission between allies during time of war, this committee was authorized to exchange secret information with the U.S. concerning all aspects of ongoing research, including "...radar, fire control, underwater detection, communication, turrets, superchargers, chemical warfare, rockets and explosives...." These exchanges were highly productive; the U.S. matched British contributions of 12 months' data and thorough grounding in science with advanced techniques in engineering and large-scale production, and access to U.S. military laboratory facilities, arsenals and airfields and to research departments of electric equipment manufacturers. "The result was a great stimulus to research on new weapons on both sides of the Atlantic." (Baxter, 1946, p. 120)

The stimulation to research which too, place in this context can perhaps best be illustrated in operations research activities on radar. The development of a new technology, such as radar, reflects both a sequence of ideas which are connected by their technical contributions to the eventual design of some device or mechanism and also an interconnection among the people who are credited with the ideas represented in such a sequence of innovation--as we saw with the concept of feedback and servomechanism. This interconnection may be implicit, and historical and reflected in the education of succeeding generations about their predecessors' contributions; or it may be explicit, and represent actual personal and professional relationships existing during some period of time, relationships which in large part constitute the context of development we are referring to as the Operations Research movement.

Progress in developing radar was built upon the work of Heinrich Hertz, who "discovered" radio waves in 1886 by proving that they were reflected from solid objects. In 1922 Dr. A. Hoyt Taylor and his associate Leo C. Young suggested that this principle was responsible for producing distortions or "phase shifts" in signals which they had observed reflected off a ship on the Potomac, and they speculated that radio waves could be used to detect objects obscured by fog or darkness. According to Baxter, the idea of exploiting this principle in techniques for detecting aircraft and ships--including submarines--was arrived at by scientists in the U.S., Germany, France, and Great Britain at nearly the same time. The construction of such a device was first proposed in the U.S. in 1933 by Leo Young, who with Taylor was now employed by the Naval Research Laboratory.

At the same time, work on radar was being conducted in the U.S. at the Signal Corps Laboratory and in research laboratories in Great Britain. Advances in the development of antiaircraft detection devices came from attempts to apply techniques derived from principles of microwaves. However, using microwaves in radar equipment depended on related developments in electronics, specifically, the development of a vacuum tube which could produce microwaves of the desired intensity for use in detection equipment. Breakthrough was achieved in this problem-area by Prof. N. L. Oliphant of Birmingham, England, who is credited with developing the "magnetron", the "...first tube capable of producing power enough to make radar feasible at wave lengths of less than 50 cm." (This innovation was one of the contributions of the Tizard mission to the U.S. in 1940, and in November of that year the Radiation Laboratory was established at MIT to develop microwave radar. The first models based upon designs developed at the laboratory were produced by RCA and installed in 1940.) (Baxter, 1946, pp. 140-145)

Team Organization for Operations Research Groups: Aircraft warning radar sets were established along the British coast in 1940, under the supervision of the British Telecommunication Research Establishment. However, there were difficulties in coordinating radar equipment with antiaircraft and fighter units. This problem was the basis for establishing operations research groups in Great Britain, the first group organized to investigate the effectiveness of radar sets in actual use. This group was headed by Prof. P.S.M. Blackett, who established operational research sections in both antiaircraft and fighter commands. Similar groups were established in the British Army, Bomber Command,

and Combined Operations, and Prof. Blackett was appointed Chief Advisor for Operational Research to the head of the British navy in 1942. According to Blackett,

> "Many war operations involve considerations with which scientists are specially trained to compete, and in which serving officers are in general not trained. This is especially the case with all those aspects of operations into which probability considerations and the theory of error enter...the scientist can encourage numerical thinking on operational matters, and so can help avoid running the war by gusts of emotion...."
> (Blackett, quoted in Baxter, 1946, p. 404)

In the United States, similar operations research groups were being established at roughly the same time under the authority of the NDRC. The type of problem which first stimulated operations research activities in the U.S. involved upgrading obsolete ordnance and developing better methods of search and detection for use in submarine warfare.

> "The United States Navy entered the war well prepared except for antiaircraft and antisubmarine work, which happened to be among its most pressing needs."
> (Morison, 1972, p. 367)

Studies performed in 1941 indicated only a 5% probability of success in antisubmarine attacks given the type of depth charge then in use. (Baxter, 1946, p. 405) At the request of the Bureau of Aeronautics, Admiral Furer of the U.S. Navy called a conference on December 1, 1941, requesting the NDRC to begin research on developing radar countermeasures receivers and jamming equipment. Assignment of this project fell to the Radiation Laboratory, newly established at MIT, in collaboration with the Naval Research Laboratory and the Signal Corps, which established cooperative relations with the Radio Research Laboratory at Harvard University. Signal Corps personnel assigned to the project were predominantly engineers, rather than physicists, as was the chairman of the project at the Radiation Laboratory, Dr. Frederick E. Terman, Stanford Prof. of Electrical Engineering. (Baxter, 1946, pp. 159-161)

Studies carried out by the Navy and at the Radiation Laboratory were a stimulus for the formation of the Antisubmarine Warfare Operations Research Group (ASWORG) which was organized within the NDRC in 1942 and located in Washington, D. C. at the headquarters of the U.S. Fleet. This research team was headed by Dr. Philip M. Morse of MIT and by Dr. William Shockley of Bell Telephone Laboratories, and included mathematicians, actuaries, physicists, chemists, biologists, and an architect, all of whom were encouraged to study combat operations first hand. (Baxter, 1946, p. 405)

At the outset there was a general lack of understanding concerning the behavior of sound in the ocean, and new equipment which was unfamiliar to officers who would be responsible for using it. The methods used by these researchers involved applying statistical analysis to the record of past operations from which they derived 1) basic laws of visual and radar sightings, and 2) tactics and doctrine for the most efficient use of new equipment, and training of operators. (Baxter, 1946, pp. 40-41)

The methods of operations research based on statistical analysis of the record of operations became a standard format for conducting this type of investigation. One significant area of inquiry involved analysis of the "search problem", which involved the proper placement of defensive escorts and an attempt to increase the range of detection, and thus the search rate, by extending the capabilities of methods of detection by eye and radar, and thus to increase the area which a pilot could search in an hour. The problem for research was defined in terms of the probability of sighting, from which it was then possible to use standard statistical methods to compute the average range of search and the optimum effective search rate under different conditions. These measures, once developed, became the criteria for evaluating alternative search plans. A computer was used for this task to analyze operational data which had been punched on IBM cards, the results indicating a measure of efficiency for each of a number of various tactics of attack as a basis for comparison and decision. (Baxter, 1946, p. 405)

As was the case in England, operations research groups were soon established in other branches to undertake similar types of research. Subgroups were established within the Navy to specialize in operations research for submarines, aircraft, antiaircraft and amphibious operations; and operations research groups subsequently were established in the Army and Army Air Forces by 1942. (Baxter, 1946, p. 407)

Contracting Relations for Research and Production: The NDRC underwent reorganization in 1942, when it became the parent and cornerstone of the OSRD (The Office of Scientific Research and Development), which was empowered by the President, rather than the Congress, of the U.S. In the same year, the Joint New Weapons and Equipment Board (NWEB) was created, reporting to the Joint Chiefs of Staff, and the Military Policy Committee was established to supervise the Manhattan

Project--and Vannevar Bush was President of all three. (York and Greb, 1977, p. 14)

The NDRC had originally been divided into subcommittees according to classes of problem they were assigned to investigate, in the areas of armor and ordnance; bombs, fuels, gases and chemicals; communications and transportation; detection, controls and instruments; and patents and inventions. The organizastional rationale adopted by the NDRC had been to decentralize research, leaving scientists free to work in their own laboratories as much as possible by contracting out research to academic and industrial organizations and avoiding building up central laboratories. However, by 1942, this original approach had been outgrown by the increasing workload and central laboratories came to be established at the University of Illinois, Chicago University, Northwestern University, The Carnegie Institute of Technology, George Washington University, Johns Hopkins, and MIT. (Baxter, 1946, p. 125) Work was allocated and coordinated among these different research sites under the umbrella of a set of committees of the NDRC, and subsequently OSRD. Thus, according to Price, MIT was responsible for developing radar; rockets were developed at the California Institute of Technology; the first nuclear reaction was undertaken at the University of Chicago and fabricated at the University of California; and the contracts to build the facilities to produce fissionable materials were let to DuPont, General Electric, Union Carbide and others. (Price, 1965, p. 73)

The divisions of the NDRC were reorganized into 18 new divisions of the OSRD, and a number of new panesl and committees were organized, including the Applied Mathematics Panel and the Applied Psychology

Panel, organized in 1942 and 1943, respectively. An ad hoc Subcommittee on Radar Research and Development was created in August, 1942, to develop the possibilities for integrating the use of new weapons, but was not as influential in directing this research as was the Radiation Laboratory, which operated under the Microwave Committee. (Baxter, 1946, pp. 30-31)

The Microwave Committee shows clearly the nature of the relationships established among scientists, industrial contractors and the military in the makeup of its membership, which included Prof. Ernest O. Lawrence of the University of California and Prof. E. L. Bowles of MIT and representatives of Bell Telephone Laboratories, Sperry Gyroscope Co., RCA (Radio Corporation of America), General Electric, and Westinghouse Electric Co. (Baxter, 1946, p. 141fn) As it happens, this pattern of university/industry/military cooperation is not at all new in U.S. history, but it is the special contribution of the operations research movement to have made this relationship explicit, and to have expressed this type of cooperation in a programme of directed research. Bush defined the process of operational research broadly as a program involving the contribution of several stages from the inception of the research to the production of new equipment.

> "The entire program of bringing a device into operation against the enemy...consists of several stages. If any one of these is omitted, the device will be ineffective. For a newly conceived device, these stages involve 1) primary research, 2) engineering development, 3) initial production for extended field tests, 4) and engineering for quantity production. For devices that have gone through these stages, as well as for older devices which are being adapted into new forms or for new uses, there are also the stages of 5) production, 6) installation, 7) maintenance, 8) development of tactics, training, and use."
> (Bush, quoted in Baxter, 1946, p. 125)

To expedite progress through these stages, Bush instituted the role of the "few-quick" organization through a special assistant acting as a liaison between research laboratories and the branches of the services responsible for procurement. This unit became institutionalized in the Engineering and Transition Office, which coordinated production facilities, supplies and priorities. The "few-quick" unit attempted to shorten the time required to move from design to production, by transfering newly developed devices from laboratories to manufacturers who would produce several units for field testing. In this process, established manufacturers were preferred as they could be expected to rapidly undertake large-scale production of the device, once tested. (Baxter, 1946, p. 125)

One further organizational adaptation instituted by the committees of the NDRC and OSRD and employed by operations groups, especially within the Navy, was the development of "field service" whereby personnel recruited through the divisions and panels of NDRC could be sent out to assist with the installation, training, and supervision of new specialized equipment in the field. This practice also had the favorable effect of making available information obtained under actual conditions of use for special study and evaluation. The practice of field testing and assistance was formalized by the creation, in October, 1943, of the Office of Field Service (OFS) as a major subdivision of NDRC.

OFS emerged out of the changing nature of the problems that scientists were called upon to solve. Whereas previously research on new weapons had been of foremost importance, at some point the need was greater for assistance in using this new equipment effectively, which involved shifting personnel from laboratories into the field. Bush defined this relationship as most effective under the following conditions:

> "Experience has shown...that successful use of such personnel...requires a) that the officer to whom they are detailed definitely wants them; b) that they be allowed access to such information as they may need for their work; c) that they be allowed reasonable freedom as to the way in which they do their work; and d) that they be responsible to the Commanding Officer and make their reports and recommendations to him, distribution of such reports within and beyond the Command to be subject to his approval."
>
> (Bush, quoted in Baxter, 1946, p. 411)

U.S. scientists in military service (whether as top-level administrators or lower-level specialists) were more likely to be civilians than their counterparts in other nations. Sending scientists into the field to assist Army or Navy units with installation and testing and training of experimental equipment was preferable to inducting such specialists directly into the services, which had been the practice for a time. The creation of the OFS offered a means by which laboratories could retain their personnel, and yet participate in field testing. Civilian status was important to the effectiveness of these scientists because it enabled them to communicate freely to personnel at all levels from Commanding General to the "lowliest GI"; it freed them from being assigned to routine administrative tasks and made them available for assignment to other tasks and projects as necessary; and it established their primary loyalties with their home laboratories. (Baxter, 1946, p. 411)

Radiation Laboratory personnel were conspicuous among those scientists collaborating with field units through the OFs, following their equipment into the field in order to assist in its installation and use, but also to "...learn its merits and shortcomings under combat conditions." In addition, courses of study in operations analysis were developed at the Radiation Laboratory and at the Princeton University Station; staffed by OSRD personnel, the curriculum in such courses covered in 6-8 weeks the topics of probability, mathematics, mechanics and dynamics, and weapons analysis and ballistics. Branches of the military assigned their officers to these laboratories for training and collaboration in order to reduce the need for special laboratory personnel to accompany the equipment into the field, and their lead was followed by representatives of industrial corporations who had contracts with these laboratories.

In some cases staff members from the Radiation Laboratory were assigned to manufacturers, and in others engineers and production specialists from these organizations were sent to Cambridge for training and "collaboration in the production of prototypes." Philco became the first of the manufacturers to establish such a relationship in 1943, and 100 other contractors soon followed. This type of interaction became an increasingly important part of the work of the laboratory, and staff members were assigned as liaison to each of these companies. Out of these arrangements the collegial relationships of multi-disciplinary teamwork came to be extended into industrial, environments. Baxter describes training sessions conducted at the Laboratory as if they were made up of members of a large team, "one of whose key players was analyzing some of the moves of past games" in order to learn from past performance.

Field service was also extended to researchers working in Great Britain on modifying and debugging radar counter-measures equipment and assisting in its use. In this case, assistance consisted of providing in branch laboratories pools of personnel, equipment and facilities, together with a communication link to home laboratories. The first of these branches was the Field Engineering Division of the New London Laboratory, followed by the British Branch of the Radiation Laboratory, and the American British Laboratory-15, which were affiliated with Divisions 6, 14, and 15, respectively, of the NDRC. The Radiation Laboratory established its British Branch in 1943 in order to collaborate with their Telecommunications Research Establishment for the purpose of "...devising tactical plans that would squeeze from the new gear that last drop of offensive power." (Baxter, 1946, pp. 156, 408)

Withal, the NDRC excelled in its efforts, particularly in the development of rockets and radar; and American industry performed miracles of production. Even more than the powerful methodology of systems analysis and cybernetics, the organizational innovations associated with this type of mission-oriented research and its system of committees and contracting relationships stand out to contemporaries as a contribution of major significance. Morison says simply, "England was saved by her scientists...." (Morison, 1972, p. 349) Grand Admiral Doenitz of the German Navy recognized the influence of science-in-war as the tide progressively turned against his initially superior submarine force. Doenitz credited his enemies with achieving this object, "not through superior tactics or strategy, but through his superiority in the field of science". (Doenitz, quoted in Baxter, 1946, p. 46)

According to Buffa, the introduction of the new radar system increased the intercept probability by a factor of ten, but he attributes to the effort of the operations research groups a further increase in the probability by a factor of two. (Buffa, 1978, p. 14) And Baxter also attributes success to the organizational factor just as much as to the contributions of science per se:

> "If a miracle had been accomplished anywhere along the line it was in the field of organization, where conditions had been created under which success was more likely to be achieved in time. (Baxter, 1946, p. 7)

Post-War Extension of Operations Research: During the first phases of demobilization immediately following the end of World War II, the different projects of the NDRC and OSRD were redistributed to other authorities; for example, the development of nuclear technology, including the Manhattan project, was administered under the Atomic Energy Act of 1946. In the National Security Act of 1947 a new Joint Research and Development Board was organized which took over the remaining functions of the OSRD and the JNWEB as the "highest level R&D management unit in the Pentagon." This board, whose first chairman was Vannevar Bush, was established to "...coordinate all research and development activities of joint interest to the War and Navy departments...." More important, those aspects of research which were of greatest interest in this post-war era were the organization-management concepts concerend with coordination, integration and the avoidance of unnecessary duplication of effort.

This board conducted its work by parcelling out projects to specialized committees made up of civilian experts and ranking military officers on the format established during the war. However, in the post-war context, the extreme decentralization of the committee approach, the part-time status of its members, and the fact that the board no longer reported to the President but to the Secretary of Defense, meant that the board had less direct influence on decision-making than it had had during the war. According to York and Greb, the board continued to be influential because

> "...I)t served as a school where a number of the future leaders, managers and explicators of defense R&D received an education in national security affairs and kept active in the public policy arena...."
> (York and Greb, 1977, pp. 14-16)

In addition, permanently established bodies were created out of the work of earlier operations research groups, perhaps the first and most well-known being Project RAND, the idea for which arose out of the operations group which included Bowles of MIT, William Shockley, of Bell Laboratories, and Frank Collbohn and Arthur Raymond, of Douglas Aircraft Company. Project RAND emerged as an "autonomous division" of Douglas Aircraft Company, established as a permanent specialized body for the purpose of conducting studies on the application of modern technology to national defense. The objective of this group was to study intercontinental warfare broadly in order to recommend preferred techniques and equipment in all aspects of aerospace power, most notably the development of the earth-circling satellite and ICBM's. RAND was made an independent research corporation in 1948, and now conducts all manner of research, not just defense-related. (York and Greb, 1977, p. 16)

Other organizations created during the post-war period in the U.S. included the Weapons Systems Evaluation Group (WSEG). This group, originally an operations analysis group reporting to the Joint Chiefs of Staff, was subsequently transformed into the Institute for Defense Analyses (IDA) in order to provide civilian-oriented adminstration necessary to recruit civilian researchers. Originally organized as a consortium of universities, the IDA later became an independent not-for-profit corporation. The Navy organized the Center for Naval Analysis (CNA) and the Army the Research Analysis Corporation (RAC). The Air Force Science Advisory Board, likewise, was a "direct outgrowth of the wartime collaboration" of General H. H. Arnold and Prof. of Aeronautics Theodore von Karman of Cal Tech. Arnold argued for a continuation of wartime research programs for the next war:

> "I believe the security of the United States of America will continue to rest in part in developments instituted by our educational and professional scientists. I am anxious that all Air Force postware and next-war research and development programs be placed on a sound and continuing basis. (Arnold, quoted in York and Greb, 1977, p. 16)

In addition to these organizational transformations, certain written reports, such as the comprehensive Toward New Horizons, compiled for the Air Force Science Advisory Board, are important beyond the comprehensiveness of the report because its authors were influential members of Air Force and other high technology programs in the coming years.

York and Greb argue that, beginning in 1950, when the U.S. began to partially remobilize and reorganize in response to the sudden beginnings of the Korean War in June of that year,

> "...(M)any of the civilian veterans of the great World War II military R&D programs...regarded it as time to remobilize their efforts in the defense of freedom." (York and Greb, 1977, p. 17)

Quick budgetary action raised the budget for fiscal year 1951 from $13 billion to $48 billion, and new organizations were created at all levels. The newly created Science Advisory Committee again reported to the executive office of the President, and a number of projects were initiated to determine the best way to apply science and technology to long and short-term national security. In this reorganization scientists were reluctant, again, to be made part of the military--thus a new structure was created, which was conceived as equivalent to the OSRD, but which did not report directly to the President. Rather the SAC/ODM was organized as the Science Advisory Committee reporting to the Office of Defense Mobilization in the White House. Its first president was Oliver E. Buckley, formerly President of Bell Laboratories.

Following the Korean War, the explosion of the first hydrogen bomb and the election of Eisenhower to the Presidency, U.S. defense policy was reappraised and even greater emphasis was placed upon military technology. In the Defense Reorganization Act of 1958 the system of part-time committees of the Research Development Board was replaced with two full-time staffs headed by Assistant Secretaries of Defense: the Asst. Secretaries of Defense for Research and Development and for Applications Engineering--ASD/R&D and ASD/AE. In addition, the civilian resources available through the SAC/ODM were mobilized in the formation of two "study panels", the Technological Capabilities Panel and the Security Resources Panel, established in 1954 and 1957, respectively. (York and Greb, 1977, pp. 19-20)

The missile program continued to receive separate treatment, in the creation of the Strategic Missiles Evaluation Committee, with its chairman Trevor Gardner, from the rocket development program at Cal Tech. This committee, which included computer Pioneer John von Neumann as well as George Kistiakowsky, Simon Ramo and Jerome Wiesner--veterans of other such groups--created a set of mechanisms designed to provide "...general systems engineering and technical direction (GSETD) for the missile and space programs." The research of the committee was undertaken under the organizational aegis of the Guided Missiles Research Division of the Ramo-Wooldridge Corporation, founded in 1960. Under complaints from private firms of unfair competition, many of the programs were subsequently contracted to another new organization, the Aerospace Corporation, a not-for-profit corporation established by the Air Force, in a similar type of spin-off as the Rand Corporation. The establishment of the Aerospace Corporation was followed by the creation of the Mitre Corporation for development of electronics and communications programs. (York and Greb, 1977, p. 21)

This type of post-war reorganization can be distinctly contrasted with the nature of the context of development in Great Britain during the same time period. In the U.S. post-war research carried out in these "organizations" extended the wartime context of development for the further design and implementation of computer technology, which was now harnessed to aerospace and rocketry and nuclear engineering projects, and thus was assured both of continued funding and administrative support and coordination of scientific talent under government sponsorship, necessary to the continuation of complex projects over the long run. According to York and Greb,

> "The R&D budget measured as a percentage of the total defense budget rose rapidly from 1945 to 1961, then levelled off for a few years, and since 1965 has substantially declined. (York and Greb, 1977, p. 25)

The final series of organizational transformations in the post-war period was initiated by the launching of the Sputnik in 1957, which York and Greb argue was a technical shock to the American public. In this reorganization the National Advisory Committee for Aeronautics (NACA) was reorganized to become the National Aeronautics and Space Administration (NASA). In the White House, the President's Science Advisory Committee was elevated to report directly to the president, thus increasing access and contact, accompanied by the creation of the Special Assistant to the President for Science and Technology as a new full-time position reporting to the president and customarily filled by the chairman of the PSAC. Under President Kennedy's Presidential reorganization of 1962, the White House Office of Science and Technology was established, and the chairman of the PSAC also filled this post. The first special assistant for Science and Technology was James Killian, President of MIT, who was not himself a scientist but was distinguished as an especially capable administrator technological and political aspects of problem-solving. The PSAC ultimately reviewed most of the important high technology programs in the Department of Defense, AEC, and CIA. (York and Greb, 1977, p. 24)

During this era Robert McNamara, as Secretary of Defense, was involved in developing new technological systems in the Pentagon, especially computer-based administrative systems, most notably PERT. The influence of the PSAC declined somewhat, partly attributed to competition between the office of the President's Special Assistant for National Security Affairs (McGeorge Bundy) and the Special Assistant for Science and Technology (now Jerome Wiesner). Ultimately, following poor relations with Presidents Johnson and then Nixon,

> "...(V)irtually all of the related White House level organizations were abolished."
> (York and Greb, 1977, p. 24)

Within the Pentagon, the Defense Reorganization Act of 1958 abolished the Assistant Secretary for R&D and created in its stead the office of the Director of Defense Research and Engineering (DDR&E), first headed by Herbert York, the first chief scientist of ARPA and director of AEC's Lawrence Livermore Laboratory, who was closely associated with Killian and Kistiakowsky. He was followed in this position during the Kennedy Administration by Harold Brown, who had been director of Lawrence Livermore Labs and a member of the PSAC, and was associated with Jerome Wiesner. The DDR&E had the authority to modify and approve all defense department R&D programs, which its predecessor organization had not. Also within the Pentagon, the Advanced Research Projects Agency (ARPA) was created to coordinate special projects, initially in the missile and space defense areas, without necessarily going through "red tape". (York and Greb, 1977, p. 24) ARPA became the first major agency of government to pioneer in the use of computers in a communication role, which had seemed somewhat obvious in the complexity of organization and interaction which attended this type of complex problem-solving and design-based activity.

In the U.S., the post-war activities formed a favorable context of development for computer technology which extended the benefits of the operations research movement during World War II into furthering computer design and implementation in a number of areas. Computers were used in applications in space and missile programs for the overwhelming and precise computations required to direct a complex system of automatic controlled devices, greatly furthering the development of automata theory, information theory, and related mathematically-based methodologies. Computers were also used in the development of communications networks in ARPA's experimental system, and, as we noted in the introduction, among early experimental users was the CIA, also empowered as a potential customer in these post-war reorganizations.

Another important site in the early history of computers in the U.S. was the Los Alamos Scientific Laboratory, where one developmental goal was to make computers more accessible to research scientists. Research sites such as Los Alamos have been favorable environments in which to undertake such innovation, because they are relatively isolated from non-technical problems that enter in development, and because of the clear-cut and bounded research objectives in such environments, which make it possible to work on more limited, and therefore tractable, problems than would a commercial development team. (Wells, in Metropolis, 1980, p. 275)

In the American context of development, computers continued to be developed and funded for these extensions of military research emerging in the aerospace program, and in "next-war" research, and in nuclear engineering and the missile program. These projects continued to provide a market for developing the technology further, and maintained necessary contacts between government, university research and the industries, stimulating the growth of many industries--among them computer manufacturing. As a measure of the degree of interdependence between government and industry in the post-war period, the amount of research and development activity in industry tripled during the period from 1953-1960, reflecting a doubling of corporate expenditures, but a four-fold increase in government funding. (Price, 1965, p. 36)

In a sense, the operations research groups in the U.S. never altogether disappeared; they redistributed their members throughout a number of government agencies and in newly created "private" firms which lived by government contracting. And they returned to the universities at which they held teaching and administrative posts, joing after the war by displaced European scientists, engineers and mathematicians, their interaction further stimulating the development of the field of systems engineering, and particularly the formal and abstract disciplines associated with the development of computers. Industry and university both benefitted from the association of science-in-industry which accompanied the broad-scale expansion of systems engineering in American industry during and following World War II. Corporate research and development became widespread in large industries--and a major factor in the many new companies working on advanced technologies--and with research and development came the "problems" of decentralization and the management of professionals which occupied much of the organization literature of the 1950's and 1960's. In this post-war context, the methods of systems analysis--especially the requirements for quantification and teamwork--if encountered in no other way, were imposed upon businesses by the introduction of computers in both process control and office applications.

The whole orientation of engineering--and, by extension, management--changed in the post-war period, incorporating within it the formal approaches of the basic sciences and systematic procedures of operations research, in imitations of the organization for inquiry in the operations research movement, and in clear contrast to the mechanistic model which can be identified in the work of Eli Whitney and other early industrialists.

The Operations Research movement which emerged in the context of worldwide war changed that context in two fundamental ways: It introduced the mathematical theory of feedback, and the systematic procedures of operations research, which formalized the process of scientific inquiry in the complex and uncertain realm of practical "problem-solving". In so doing, the complexity of problem-solving led to the organization of working relationships in multi-disciplinary teams, coordinated by a network of committees and contracting relationships.

This system of committees and contracting relationships reflects a form of organization which is different from traditional and bureaucratic hierarchies (defined in Weber's terms). To the extent that an organization "exists", its structure is fluid and flexible, contingent upon requirements in some problem-environment for personnel and information, requirements that must be fulfilled by processes of coordination. Continuity is largely reflected, not so much through official relations, but through personal and professional relationships established in a number of different institutional environments over a period of years. Ultimately, the nature and duration of any particular institutional configuration--indeed, even the existence of the entire system of committees--is itself contingent upon events taking place in other systems. This form of organization is structurally different in two respects: 1) In the relation of individuals to the institutional environments in which they participate--i.e., in the role(s) they play in different contexts; and 2) in the relations which connect different institutions in a broader environmental context, via a network of personal and contractual ties.

York and Greb argue that organizational changes associated with continuing technological developments "in the American half of the arms race" were responsible for improving the capabilities for carrying out increasingly sophisticated development programs. Organizational innovations such as we have described, were institutionalized either as permanent mechanisms in existing arrangements or in processes of reorganization which resulted in more effective organizations, which themselves became permanent. These authors also point out that "nearly every one of the principals...knew most of the others on a personal basis for a very long time...", and were related to each other directly (through positions they held on various committees) and/or indirectly (through teacher-student relations which linked an even broader population of researchers through these fluid committee and project ties.) (York and Greb, 1977, p. 13)

Consequences in the realm of social organization were a form of centralization/decentralization combining a fluid and collegial form of project teamwork with a network of interorganizational connections which establish a pool of resources in personnel and facilities to staff those project teams. As Price notes, this system fuses economic and political power in committees of the government responsible for sponsoring technological development, and at the same time diffuses central authority in a system of committees and agencies. Price argues that this type of arrangement destroys the notion

> "...that the future growth in the functions and expenditures of government, which seems to be made inevitable by the increase in the technological complexity of our civilization, would necessarily take the form of a vast bureaucracy, organized on Max Weber's hierarchical principles...."(Price, 1965, p. 75)

This arrangement is almost self-evident when viewed in historical perspective, but it seems to come as a surprise to social scientists in each generation. Journalists have recently discovered joint research as an organizational phenomenon affecting universities:

> "Centers, programs and institutes for joint research in microelectronics, computer-based automation, telecommunications and biotechnology are proliferating on campuses across the land." (Nobel, 1983, p. 129)

Industries are seen as "moving into" universities and "seeking to acquire privileged access to and control over the form and flow of scientific research." This view of directed research not only confirms the contiɒuity of this American pattern of interlocking networks of government/industry/university ties; but it is also indicative of a general impression that this pattern is not only extraordinary but of recent origin, and that although it may be considered effective, it is not necessarily considered respectable.

This hybrid form of centralization and decentralization is significantly different from the organizational hierarchies in both the German and Japanese military. Decision-makers in the German hierarchy followed the position that it was not necessary to mobilize scientists to the war effort in any special way. Thus German military research was carried out within the armed forces and war industries laboratories, while German scientists by and large remained unconnected to the effort. Either they continued their civilian research under individual grants-in-aid, or they were drafted into the military, not as scientists, but as soldiers. Moreover, those agencies which were created for coordinating scientific research in Germany--most notably, the Reichsforschungsrat, created in 1943--were "bogged down in a welter of overlapping jurisdictions." (Baxter, 1946, pp. 8-9) At least partly in consequence of this restrictive organizational context, research proceeded in a reactive mode--what Baxter calls "forced draft" research--a very limited form of inquiry. When the U.S. introduced radar detection equipment and proximity fuzes, German researchers failed to shift to microwave principles being investigated both in Britain and the U.S., and instead "strove feverishly to render their existing sets jamproff." (Baxter, 1946, p. 94)

Japan, similarly, left the majority of its scientists unmobilized during the war, isolated in individual research. To an even greater extent than was the case in Germany, enmity existed between the different branches of the armed forces, and military officers distrusted civilian scientists to such an extent that their ability to conduct research was seriously inhibited by security regulations and restriction of information to the various research branches. In contrast,

> "Industry in America had long since learned that to subordinate the research staff to the production department is the shortest road to failure."
> (Baxter, 1946, pp. 8-9)

Price claims that where scientists have shaped organizational development, they have done so by shaping political and administrative patterns so that they reflect the way scientists actually behave, and that science progresses in a distinctly non-hierarchical manner. Baxter notes that the complexity of the organization for inquiry which characterized the operations research movement reflected the complexity of the problems they were called upon to solve, together with the necessity for maximum speed. Under these circumstances, the organizational mechanisms for decision-making and coordination became correspondingsly complex, through a proliferation of relations with a number of independent elements.

The operations research movement itself also exhibited considerable "structural" change; it was not the case that a new form was created, after which it became institutionalized and permanent, nor that this form was everywhere unique and consistent. The goals of the operations researchers changed in the course of the war from weapons design to field assistance and implementation, and after the war to a number of different objectives. Likewise the organizational bodies and ties among them were transformed by several reorganizations, which accelerated, as we saw, in the post-war period. And, finally, organizational bodies and relationships established in contracting relations displayed a contingency and limited existnece which challenged traditional concepts of organization even beyond reorganization in form, to a recognition of the essentially conditional and adaptive nature of organization as a process, subject to constraints--an idea which has had significant impact on Organization Theory since World War II.

It was thus on the basis of this experience that applications of science in industry were expected to involve a decentralization of authority, in which, for example, professionals would be more loosely tied to any particular organizational context, much like the civilian scientist, tied to his peers by project- and professional-relations; and where those "organizations" would spend much of their time in cooperative interaction, which would display more stability than the existence of any organizational units in the network. A great outpouring of organizational literature resulted from these expectations, which were translated into speculations for the impact of computers on organizations.

However, by 1979, reporting on the "International Comparative Study of Research Units", Gerald Cole notes that organizational context had become more important in determining the behavior of scientists than scientific--theoretical--priorities, and that this type of science-based decentralization was a reality only in academic institutions. (Cole, 1979, p. 382) This is not that surprising, when we consider that this form of centralization/decentralization has a long and consistent history in the United States--one which significantly polarizes the structure of organizational relationships between professionals (designers) on the one hand, and operators on the other hand. Moreover, in that history we have seen before the limitations of a designer-based methodology of systems development.

American Contract Research Antecedents:

The particular combination of underdevelopment and colonialism characteristic of the American context has tended to place scientists in decision-making positions, as we have seen, from the earliest days. In contrast with the British system, in which the administration of government is coordinated by a single administrative corps or civil service, in which there are few scientists and in which decision-makers tend to be educated in the humanities, early American distrust of established civil service, as well as any type of "elite corps"--including lawyers, clergy and governmental bureaucrats, undermined the development of any institutional body at the same time that it put a special premium on the practical skills and knowledge of scientists. The result was that after the early Jacksonian revolution eliminated the beginnings of career civil service in the U.S., when government service was instituted in later years, scientists found an important role, and were moreover, quickly elevated to positions of influence. Thus the higher ranks in the American civil service were well represented by the sciences, and increasingly so following World War II, after which "supergrades" were added to the existing civil service to provide scarce talent in science and engineering. According to Price, these beginnings gave to American scientists an influence in policy-making which was not shared by their counterparts in any other country, including Great Britain, where "...the notion persisted rather strongly that scientists were instruments for predetermined ends." In the U.S. on the other hand, the role of scientists in decision-making made it possible to demonstrate that

> "...(W)hat scientists discovered by unrestricted research might be of greater military importance than the things the military officers thought they wanted...in short, that the means might determine the ends." (Price, 1965, p. 59)

Interestingly, one characteristic which Price notes is shared by scientists both in the U.S. and in Great Britain is a lack of interest in management, but a vital interest in the creation of policy. However, policy is approached in a deductive manner in Great Britain, where scientists work under the authority of a tightly organized corps of administrators. In contrast, policy in the U.S. is approached inductively, with scientists taking an active role in initiating and advocating policies. (Price, 1965, pp. 59-65)

Thus, in the American context of development executive departments and agencies as well as corporate organizations all have their own professional identities, and must be treated as at least semi-autonomous, rather than as automatic "transmission belts of political decision." The magnitude of the research effort in World War II, the unpredictability of scientific development and the pressure of rapid change were accommodated in a form of organization within government much more characteristic of "...the same sort of free competition that is supposed to prevail within the private economy." (Price, 1965, p. 48)

This collegial, decentralized form of organization also closely resembles that of the foundations and research institutes which were the models for the cooperative organization of the NDRC and OSRD. The National Academy of Sciences, established in 1863 (during the American Civil War period) operated by a system of committees and boards, and was the model for the National Research Council, established in 1916 by Woodrow Wilson (again, in the context of preparation for war.) Baxter, 1946, p. 13)

This form of organization is a familiar pattern in American history, particularly in wartime. In each war in which the U.S. has engaged since Whitney was contracted to produce firearms, a similar form of organization has emerged to link together industry, education and government for the purposes of coordinating industrial production and manpower in the war effort. During World War II the War Industries Board coordinated purchasing for the U.S. government and its allies, providing funding through the administration of a finance corporation whch issued securities and was responsible for supervising security issues and loans to industries contracted to produce war materiel. Labor relations were regulated by a labor administration, which arbitrated disputes, banned strikes, and in some cases fixed hours and wages.

In World War II the War Production Board straightened out the confusion of high speed planning and production which followed Pearl Harbor. Once coordination was provided "...the curve of production rose sharply...", American industry again producing not only for itself but for its allies as well. This type of arrangement was highly effective, and characterized by a centralization in decision-making associated with purchasing for a highly decentralized complex of research and development and production sites. The evidence that this is a conventional pattern in American society, in spite of the fact that it contradicts in many places conventional concepts of bureaucratic administration, is that there is little opposition to these organizational reconstructions. (Morison, 1972, pp. 207, 368)

> "...(L)ike the evidence of Sherlock Holmes watchdog that did not bark: no Congressman chose during the 1950's to make political capital out of an investigation of the interlocking structure of corporate and government interests in the field of research and development."
> (Price, 1965, p. 51)

Chamberlain argues that the civil war had the same effect of transforming the nature of American industry,

> "...bringing forward new men and new methods of organization...(and) changing the ground rules under which business operated, inevitably strengthening the role of the federal government."
> (Chamberlain, 1961, p. 129)

During the Civil War, the U.S. federal government found that it was economically feasible to prosecute the war and still conduct "business as usual"; in fact, the mobilization of industry represented an "economic victory" commensurate with those on the field of battle.

And in the Civil War the first signficant success came in the production of clothing for the troops in spite of the loss of cotton from the south. The clothing industry switched from cotton to wool, and moved the production of clothing into the factory by harnessing Howe's and Singer's sewing machines, and by 1864 the ready-to-wear industry was producing clothing for a million soldiers and increasingly for civilians as well. (Chamberlain, 1961, pp. 123-124)

Wartime research has frequently provided the stimulus for analytic methods of research and engineering which emphasize a pragmatic approach to problem-solving and a heavy reliance on data collection, measurement and quantification as a basis for prediction and planning--and control. Wartime provides unique opportunities for data collection, as a prerequisite for quantitative analysis, and measurements taken for military purposes have proved of great benefit to industrial development. The need for uniforms for Union Army recruits provided the occasion for standardized measurements to be taken on a broad range of the (male) population at that time, which information was invaluable in the postwar development of the ready-to-wear industry. The existence of a base of empirical data made it possible to develop national markets, which made it further possible to sell clothing at high volume for low prices, the secret of Henry Ford's success. The development of this market also stimulated the market for the manufacture of sewing machines, initially developed in Europe but put to use -n the U.S., where they were increasingly used in the notorious 19th century "sweatshops" in which volume production could be carried out most "efficiently".

The Context of War:

Whether or not a technological innovation creates change in a society or firm, and whether or not it creates "revolutionary" change in the structure of organizations, is largely a matter of the context into which that innovation is introduced, and the manner in which change is implemented. We have argued that it is not the technology per se, but the use(s) of technology which determines the outcomes--or "impacts"--on organizations and individuals. The uses of technology, as we have seen, cannot simply be determined from a description of the features or capacities of the equipment alone, which can only fully be defined with reference to applications. Rather, definitions of use are a function of the context in which the technology is developed and in which it is applied, and thus the context of development for new technologies exerts a mutually-determinant influence on the context of application. One contextual factor which appears conspicuously in these accounts is the influence of war, which renders the efforts and achievements of scientific and technical specialists meaningful in practical terms, assures them of material support, and provides the basis for a network of communication and collaboration among researchers who otherwise might be working separately.

The invention of automata over a long period of centuries had little influence on the nature of the societies in which these inventions took place. Rather, in ancient Hellenistic society special machines were invented which were only the isolated reflections of their creator, and built for aesthetic enjoyment. A major difference between ancient and modern technologies lies in the "conspicuous absence of any consistent attempt to replace human by non-human energy" before the middle ages. (Edelstein, 1952, pp. 579-80)

We have argued that in order for a technological innovation to be influential in producing change in society, it must demonstrate some practical purpose by which it can be rendered meaningful to an external "audience", a purpose which will justify the effort and expense of moving from invention to production of a new cultural artefact. Indeed, we have demonstrated cases in which, in the absence of practical objectives, creative knowledge languished, serving only as entertainment (or torment) for its creator, without exerting any significsnt influence on the nature of the world. In arguing for the influence of social context on the development of science and technology, Edelstein notes that the accomplishments of the ancients must be understood "within its own setting", and that their social context was characterized by a complete indifference to the value of scientists' work. In the case of the Greeks, according to Edelstein, mechanization did not develop further than it did because there was little need for mass production in traditional society. Where technical progress was more rapid, it came about through production for war. (Edelstein, 1952, pp. 598-599, 582)

Among practical objectives, we have seen that none can compare with the exigency of war, which throws up a host of "problems", a common objective purpose behind which people who otherwise may have no reason to cooperate will combine their efforts, and the authority--money and media--to raise the ephemeral "existence" of a body of ideas to a concrete reality, embodied in published reports, in artefacts of machinery, in constructed environments, and in new social relations which effectively reconstitute the fabric of the world of social life. The exigency of wartime is sufficient to transform the individual, contemplative pursuit of knowledge "for its own sake" into the design of practical technologies, even in those cultural contexts in which practicality is generally denigrated. As an exception to "normal" social conditions, the influence of war serves to create new forms of collaborative organization, which transform the social context of inquiry and development, and thereby contribute to a process of social change as well as to that of technological innovation.

Werner Sombart argued that the warlike society is transformed and strengthened by economic changes brought about by industrial society. He agreed with Spencer that the progress of civilization transformed warlike societies over time into industrial societies; however, he disagreed with Spencer's inference that the progress of civilization would carry with it the disappearance of war. Rather, the influence of economic changes carried out in the industrial sector serve to effect a fundamental transformation in the conception of war, which becomes ever more involved with economic interests, and the stakes for which war is waged become increasingly--exclusively--economic. (Sombart, 1937, p. 25)

In U.S. history, the influence of war on the development of the industrial economy is striking. We have argued that systems engineering was practiced in America from the earliest days, largely under the press of war. By the 1850's America was already leading Great Britain in mechanization, standardization and mass production in a variety of fields, an accomplishment which Bagwell and Mingay attribute, at least in part, to the scarcity of labour in the U.S., which encouraged capital-intensive methods of production through the use of machines (a mode of development which we have identified as "designer-based").

> "Because of the scarcity of labour, the introduction of new machines and techniques did not threaten the jobs of workers; without displacing anyone they increased labour productivity, and possibly brought some increases in wages."
> (Bagwell and Mingay, 1970, p. 163)

Overall, U.S. industrial productivity burgeoned after the Civil War, displaying a six-fold increase in manufacturing production between 1860 and 1900, and a further doubling between 1900 and 1913. In this period the U.S. because the world's leading industrial nation, by 1910 accounting for approximately 36% of the world manufacturing output. U.S. industrialization took its lead in textile manufacture--and especially in cotton--which employed 115,000 people in nearly 1,100 establishments by 1860. (Bagwell and Mingay, 1970, pp. 163-4)

In the sense in which Eli Whitney is credited with the first recognized instantiation of the "American System" of manufacture, and just as the American Civil War is credited with organizational innovations that were instrumental in the relative success of the U.S. industrial system in comparison with established European industries in spite of--or perhaps because of--its relative backwardness, similar efforts of scientists in the course of World War II transformed the organization of industry, fostering technological innovation and resulting in greater industrial productivity.

The contributions of scientists and industrial and military contractors to the war effort in World War II--a phenomenon we have been calling broadly the "operations research movement"--resulted in several major accomplishments in the application of scientific knowledge in industry:

1) Specialized work on problems such as radar and communications was accompanied by the development of the formal concept of feedback, which was identified with the concept of "system" as an abstract mathematical entity. Defined in this way, complex phenomena could be precisely modelled in all their complexity, and hence such formalized models proved more powerful than conventional inductive-empiricist research methods for understanding and solving complex problems, such as sighting and firing at rapidly moving objects. The principle of radar embodied the idea of feedback which, by giving it a concrete representation a) made it possible to abstract that principle, formalizing the idea of feedback itself as the mechanism of self-regulating systems. b) Once abstracted and formalized, the principle of self-regulation could then be translated into other instantiations. Once the problem of miniaturizing components was solved (initially by the British) then it was possible to build in feedback--control--devices into other machines (such as missiles). As a component of a larger machine, this type of device functioned to transmit signals and to register or measure their responses, thus acting as a controller or governor for the larger device. This was the basis for building self-guiding--self-regulating--equipment, which is the essential characteristic of automata.

2) Other problems, such as those involving the logistics of maintaining and transporting critical materiel, or those involved in recruiting and training personnel to man the equipment created by these design groups, called for massive data-gathering and computational resources to analyze that data. Solving these problems involved the development of procedures for data-gathering and analysis, which solutions drew heavily from statistical models, and bookkeeping solutions borrowed from "inventory-type" applications in commercial enterprises, and are still indicative of the involvement of budgetary issues in planning and provisioning of complex projects.

3) Procedural innovations in data collection and data processing were associated with innovations contributed through the operations research movement to the third major problem in the war effort--the necessity and difficulty of planning, organizing, and communicating among a diverse andhighly complex set of problem requirements and environments--and coordinating among this host of decentralized activities under the extreme pressure of time and mortal conflict. The solution to these problems came, at least in part, in the form of social-organizational innovations which resulted in systems of communication and coordination through flexible networks tying together multi-disciplinary project-oriented teams, which could be quickly and efficiently mobilized for specific--and changing--objectives. This type of organizational arrangement had the advantage of central administration and the presumptive identity of personal and organizational goals in

wartime emergencies. In addition, this system of decentralized and flexibly constituted organizational components permitted transformations--or adaptations--of organizational structure and style to accommodate the specific exigencies of given problem situations, notably the progression from basic, problem-solving research to problems involved with implementing these designed solutions in the field.

4) And last, of course, in working out the data-handling and computational requirements of both these formal and procedural problem solutions, all relying heavily on quantification of complex problem-situations, the evolution of the computer-in-use stands as a significant accomplishment which can be largely attributed to the work of applied mathematicians and others who contributed to wartime research. Their work involved practical objectives of such compelling necessity and complexity that the impetus to the expenditure of effort, resources, and coordination among separate contributors finally led to the commercially-viable emergence of computer technology, and not incidentally to a nucleus of quasi-private industrial firms already positioned to carry out the commecial development of this new "product".

In confirmation of Sombart's claim that war becomes increasingly involved with economic--and, by extension, organizational--interests (both factors important in the development of computers) Janowitz argues that "the impact of military technology during the past half-century can be described in a series of propositions about social change", each of which have tended to blur the distinction between civilian and military organization. Increasingly, a larger proportion of the gnp is given over to "the preparation, execution, and repair of the consequences of war". The technology has developed in such a way that it has increased the destructiveness of warfare and broadened the scope of automated weaponry; thus military objectives increasingly focus on deterring rather than applying violence, while at the same time demobilization is abandoned for a more continuous and permanent adjustment of the military to a continued role in what Baxter has referred to as "next-war" research. This further breaks down the boundaries between military and non-military systems, under the requirements for research development and technical maintenance, and broadens the role of military professionals--who now have a continuing need for specialist training--due to the permanent nature of preparedness. (Janowitz, 1974, p. 54)

Innovations in the applications of science-in-war and industry did not begin with World War II, however, but have a much longer history, extending in the U.S. to the Revolutionary War era, and developing progressively on a continuing path through each succeeding war, a "path" characterized by a "designer-based" mode of development which exhibits an emphasis on machine design and flexible networks of project teams held together by contractual and administrative relations. In the U.S. in each war presumptive hierarchical forms of organization have been transformed by flexible, directed organizational arrangments, linking heretofore independent and separate institutions. Throughout the period following the Industrial Revolution in America, wars have served as an arena of social change particularly amenable to technological innovation and development--in a sense by relaxing or eliminating conventional restrictions and arrangements by which production and trade are normally organized, and reconstituting these arrangements in a highly focused manner which 1) is free of the demands of profitability and the constraints of established business hierarchies, and 2) which constitutes a new "community" of co-workers.

For Sombart, war is the truest expression of community, which he defines as the love of the individual for the state, from which he derives his liberty. Liberty derives from the "law", which represents the "conscious ordering of the organic unity of the state", as defined by Hegel, who argued that all of man's special interests can only be satisfied by identifying the particular purpose of the individual with the general purpose--or political opinion--upon which consciousness man accepts the state as a necessary abstraction. That abstraction consists in the realization or consciousness--expressed in that political opinion--that one's "real and special interest" is entailed and preserved in that interest and purpose of another represented in the state. In this consciousness the individual is "free", his rights preserved as duties, the distribution of which falls, not on individuals, but on the state, which alone affords the possibility of linking together the diverse contrasts into a "world-plan" (which amounts to nationalism). (Sombart, 1937, pp. 214-215)

Sombart identifies community with the presence of love and the direction of God, which received its truest expression in the Middle

Ages. "Community is an idea which is realized through the conscious participation of individual persons." It may exist through three ideal "structures": religion, family, and political association, but for Sombart neither the first two in itself can constitute a community. Sombart reserves this designation for intentional political associations based upon conscious attitudes of persons toward the state. Under these conditions, the state becomes "imbued with love", and the solution to the "state-person" dilemma can then be found in Goethe's ideal of "voluntary dependence", which is only possible through love.

"Community", defined in this way, is a condition, and a temporary condition at that, which tends to occur only within small groups or circles of persons, and/or in extraordinary or revolutionary times. Thus Sombart argues that this enthusiasm or love for the state, this condition of voluntary dependence, is only experienced by large parts of the population during times of stress--namely, war, the "love-creating" powers of which are "overlooked only by cranky pacifists". During peace-time, the "fruitful 'remembrance of war'" will remain and unite and integrate individuals. (Sombart, 1937, pp. 220-221)

The significance of war, and of science-in-war, is that through the act (and process) of organizing, of planning and creating solutions to problems mutually defined, people are brought together and their individual contributions and creations are rendered mutually comprehensible--meaningful--and useful. Sombart is right in that sense--community is generated in the creation of a social order by a group of people with common objectives--and war enforces that commonality of interest, elevating collective over individual interests, ultimately to the point of "do or die". And sometimes they do.

However, it is the act and not the outcome which is important in this regard. For Sombart the individual derives his political existence--his liberty and consciousness--from the state. For Jefferson and Tom Paine, among other early Americans sharing in the tradition of British enlightenment thought, it is from the individual--in company with his neighbor--that the state derives its political existence, and as a tool for the protection and betterment of man, it therefore can always be changed or recreated, which is another way of "creating community". The application of scientific knowledge to the effort to gain political independence and to establish a new social order was, in Revolutionary America, a great and noble experiment, and as Price puts it, Americans "thought of science as the basis for world-wide political revolution and for continuous political progress thereafter". (Price, 1965, p. 88) The scientific worldview was liberating--of individuals and institutions--from the ignorance and injustice of traditional (religious and family-bound) institutions. By extension, science was a legitimate ally of policy as the method of improving institutions and making more perfect societies. Science was also the ally of the "rights of man", by which even the commonest citizen could be improved indefinitely by education, a point of view which developed into the quite respectable and distinctively American philosophical school of Pragmatism, which takes the "practical" as the seat of moral virtue and the highest form of scientific inquiry.

In the United States, the occasion of war happens to have been a major historical factor in the development of the American economy, and that largely through the role of government sponsorship of technological development and change. Given America's historical origins, and its cultural biases deriving from its colonial and revolutionary beginnings, war has represented a (frequent) opportunity to remake institutions, and this fact may underly what Drucker notes as a value for an endless procession of corporate reorganizations, which has accelerated since World War II. In this process, the common view of technology as something created by man and thus an available vehicle for improving institutions, and through the acquisition of skill, improving the position of individuals, constitutes an ideology--a set of ideas not about specific technologies per se, but about the value of technology, and an acceptance of organizational change (seen from top to bottom in American corporate organizations) as preferable to the constraints of the institutional present. In this context, the traditional--American--ideals of the personally-liberating value of science reemerge in expectations of mobility associated with the introduction of new technologies--and also frequently associated with the occurrence of war.

However, Sombart himself warns against taking the liberating powers of technique too much for granted. He argues against both the Kantian approach to the meaning of technique as the ground for the "freedom of the spirit" in a positivist/creative sense, as well as the Nietzschean view which sees in technique the "will to power", in favor of a definition of technique which is "culturally neutral" and "morally indifferent". Each case must be individually evaluated according to differences resulting from three "intervening" factors:

1) Different "spheres of interest"--whereby he refers to the interests of the "totality" and not those of individuals;

2) Different personal interests, which does take into account the effects of technique on the individual producer, consumer and "third party"--that person whom technique affects, who submits to it and who admires it; and

3) Differences in "the modalities of the application"--referring to those by whom a technique is used, to the place and time of application and to the extent to which technique is used. (Sombart, 1937, p. 242)

This viewpoint is consistent with our argument that it is the application--the uses--of new technologies, rather than the characteristic features of the technologies per se which is responsible for the outcomes for organizations and for individuals in them. For Sombart, "Technique in general signifies procedure." Sombart defines technique as referring to any system--or complex whole--consisting of means for the achievement of a definite purpose; and thus technique can be characterized according to the kinds of means or to the purposes to be accomplished. Means can be distinguished between the powers and abilities which the human uses to shape procedure, and technique in this sense refers to vital, formal or organic technique applied to some desired end. This is the technique used by designers of the system.

Means can also refer to instrumental technique, however, in which the means to achieve given purposes consist of definite objects or "instruments" which serve the purpose being employed, of which Sombart argues that "there are as many different kinds of technique as there are purposes." On this point we may note agreement with Toda and Shuford's observation that "structure" reflects all the types of purposes and means which can be identified, and thus that there are as many different possible structures as there are possible techniques. Sombart notes that Europeans generally identify technique with instrumental technique and in this regard technique is identified as often in the use of finished products for specified purposes as it is with the creation of those real objects themselves. (Sombart, 1937, p. 226) In this regard, the use of computers represents the embodiment of instrumental technique in both of these senses: in the use of a physical object--the computer itself--in fulfilling given purposes, and in the creation of the computer as an object or purpose, which reflects this other--"vital"--technique, which we have identified in the broad range of activities engaged in by researchers during World War II. This distinction is important for inquiring into the transfer of what we have been referring to as a social organization for inquiry characteristic of the operations research movement into postwar, corporate, contexts.

Sombart argues that the traditional discussions of technique have been one-sided andoverly concerned with mechanization, focusing on the machine as the major characteristic of modern technique. The machine principle, however, is "as old as technique itself", and although mechanization has attained a high degree of development, it is still only one element in the explanation of modern technique. Modern technique can only be understood by including all the inventions and procedures leading up to a given point, identifying them with "one denominator, illuminating the common spirit and general principles on which modern technique originates and exists." This common denominator is what Sombart means by "style", and it is style in the form of a common "spirit" and set of principles which must be accounted for in addition to machine principles in order to explain modern technique. (Sombart, 1937, p. 229)

The general principles upon which modern technique is based are represented in the knowledge of the natural sciences, together with a characteristic attitude that theory and practice cannot be separated, but rather are mutually dependent elements flowing naturally into each other, and following from the "practical will to conquer". In the style of modern technique the universe no longer reflects the handiwork of God, but represents a "system of relations whose separate parts, as the whole, are soulless and are held together through inherent "nature conformable to law". Derivatively, the process of production is believed to unfold according to this natural law, independent of human will, and thus "technique artificially creates a world which unfolds according to a formula set up for the universe by natural science". The essential principle in creating this new world lies in emancipating control from the limitations and barriers of living nature, by the use of material, by the application of power, and by the selection of procedures and methods. (Sombart, 1937, pp. 229-231)

In the formation of procedure, human control is removed through the elimination of human cooperation, a process which takes place largely through machine use. According to Sombart, while instruments can be seen to support human labor, the essential mechanical principle is to be found in the replacement of human labor, and it is this principle which characterizes the modern age as the age of technique.

> "(O)ur age is an age of technique because, concerned with the means, it has forgotten the ends; or, to express it differently, because it sees in an artificial formation of means, final ends."
> (Sombart, 1937, p. 233)

In this attitude there is a high valuation on instrumental technique, and a corresponding general interest in technique for its own sake, and in the position of influence of the technician, especially the engineer. "All this without asking as to the purposes to be achieved, without testing the values which are to be realized through these ends." (Sombart, 1937, p. 234) This attitude Sombart compares with the (then current) artistic value in perfect execution, best expressed in the phrase, "L'art pour l'art"--the same principle underlying the construction of mechanical toys in ancient cultures. By extension, an emphasis on "organization", also cultivated for its own sake, comes to be an overriding characteristic of the modern "technical age". Thus, it is a mistake to see the "misery" of the modern age as an effect of modern technique, for

> "How can technique effect anything in social life--when all effects proceed from motives--otherwise than through men who are served by it? What it does effect is not technique but men who apply technique....Business men only are responsible."
> (Sombart, 1937, p. 236)

There is thus support for our argument that technological innovation--particularly innovations in communications and records-keeping technologies, such as computers--may or may not induce structural transformations in ongoing organizations, depending upon the objectives for which these technologies are introduced, and the manner in which the change-over is effected. We have much evidence that the Industrial Revolution did effect a dramatic transformation in the social structure of traditional European societies. However, those organizational transformations we have so far experienced in the structure of modern society as a consequence of technological innovation--including that of computer technology--have been more powerfully influenced first by the continuing development of the methodology of systems engineering (which, as we have seen, considerably pre-dates World War II) and, second, by the social-organizational innovations which emerged in the course of World War II, but which have their roots in earlier--wartime--experiences in American history.

We are suggesting that, far from the asserted "computer revolution" which is popularly thought to have been the impetus for transforming organization structures since World War II, these structural transformations in what we might consider a presumptive institutional view of organizations can be traced as far back as the Revolutionary War in American history. Since Eli Whitney's time, engaging in wars has provided both the necessity and the opportunity to apply scientific knowledge to innovation and problems of production, undertaken cooperatively with government and industry.

In this "mission-oriented" research, a concommitant outcome has commonly been the development of emergent social organizations for research and coordination in the service of military objectives. In each instance, organizational arrangements have been put in place which constitute a joint cooperative relationship between government, industry and universities in applying scientific knowledge to the war effort. It is this endeavor which transforms traditional hierarchies into more flexible networks of project teams, which arrangements are contingent upon the type of problems faced, and which bring scientists into the role of policy-maker and change-agent on the strength of their technical expertise.

This form of cooperative participation is not only characteristic of the American form of organizing for war, but has had significant impact on the development of new products, technologies and production processes throughout American history. A common phenomenon in each war has been the transformation of "private" industry by "public" support (and control) through the influence of military objectives on the part of the central government, which intervenes in the exigency of war: 1) to create and support essential new technologies and services, and in the process support the development of new industries--railroads, telephone and telegraph and basic utilities, and now computers. 2) In so doing, governmental direction limits the type of control which can be exercised within those industries, and may effect transformations in the processes of managerial control by introducing new methods and forms of organization, which may come to be preferred--partly on their own advantages, and partly because of the contractual requirements imposed on those industries by government sponsors of research and development.

The continuing popularity of "systems thinking" in business literature, with its emphasis on planning and problem-solving, may largely be due to the highly visible successes of the operations research movement, which moved rather smoothly in the U.S. into postwar private enterprise, both in the well-publicized aerospace program, and in other "high technology" industries, most of which are involved in one facet or another of the emerging computer industry. It is in this context that computer technologies first emerged in modern times as a viable technical and commercial enterprise. The scope and depth of this technology have generated the emergence of new products and industries, new production processes, and new concepts of organization--especially a self-conscious system design approach which mirrors the type of cooperation which emerged among researchers during World War II. The formalization of systems engineering concepts and methods which facilitated the development of computers in the course of wartime research, was extended into the development of applications for computer technology: 1) in the further mechanization, and in some cases (such as refineries and feedlots) automation of manufacturing and production processes, and in CAD/CAM applications which mechanize much of the design work in specifications and materials inventory management. 2) In offices, this formalization is being extended--through computerization--to administrative processes and clerical work. Some managerial decision-making applications have been developed, especially in military "war-rooms" and their corporate counterparts, but the full application of computers to strategic planning and decision-making is only beginning. It is in this sense that the introduction of computer technologies into organizations, and particularly into administrative offices, has been significant--as a vehicle for the application of systems engineering methods to management, more closely approximating the criterion of integration by which automation is distinguished from mechanization. Clearly, current efforts at industrial and office automation have not been created by the introduction of computers alone, and are thus demonstrably not simply a product of post-World War II systems engineering.

We must conclude, in agreement with Herbst, that computerization to date does not constitute a new Industrial Revolution, but rather is a continuing chapter in the (first) industrial revolution, begun in the 19th century, but having its roots in the 18th and even 17th. That industrial revolution had, as essential characteristics: 1) an emphasis on mechanization, and particularly the mechanization of human labor, in which current efforts at computerization are but an extension of the mechanization and integration of the total production process, through developmental stages of craft production, mechanization and automation; and 2) a deliberate effort to apply scientific and technical knowledge in designing and managing organizational processes in carrying out corporate objectives, which is conventionally associated with bureaucratic administration, but which we have seen may also be associated with a form of decentralization-centralization in project teams coordinated by a central authority, a pattern we have come to recognize in wartime as especially favorable for development and coordination in complex problem situations.

The remarkable activity taking place in the marketplace at this writing can be attributed in part to the relatively recent influence of office automation applications, and especially word processing, which renders computer technology more widely accessible and lower in cost. In addition, although centralized process control applications to administrative records-keeping and word-processing are now exhibiting signs of strain, newer word processing applications--especially those employing microcomputers--appear to "soften" the organizational-structural requirements for computerization, and this may account for an upsurge of professional and managerial applications in existing offices in the past two years.

Computer technology may lead to far-reaching transformations in the social structure of firms and occupations in modern society through the scope and power of computers as multi-purpose machines the functions of which can be adapted for a wide range of working environments. Structural changes may also take place through the interactions of smaller, bounded changes in different components of the total production process, as well as through interactions with other systems in the environment of the firm or society, and structural changes of this nature have been ongoing in offices for the last generation. However it is also possible, and there is evidence to support the claim that computer technology--regardless of its theoretically-based power and scope--has to date been implemented in such a way as to preserve traditional organization structures by increasing the capacity or control in existing hardware, structure and procedures. This latter type of "change" may indeed transform the entire production process, and yet leave the essential direction and structural organization of the firm unchanged. This type of change is not necessarily adaptive, but rather may represent a progressive limitation of possibilities, a foreclosure of alternatives which may lead to an acceleration of decline and entropy within existing firms undergoing change.

We will argue in the next chapter that the further development of computer technology-in-use in offices is being inhibited by a technological style which embeds the development of applications in a deeper context of systems engineering in the traditional designer-based mode, which continues to be a dominant model of industrialization in the U.S. The success of the wartime operations research movement, and particularly the development of computers, revitalized two areas of industrial management and research of central relevance to office automation: management science and human factors engineering. In their classical form, these models may not necessarily be compatible with the organization for inquiry associated with the full methodology of systems engineering necessary for the successful implementation of computers in administrative offices. In the absence of a rigorous systems analytic methodology as a framework for the design and implementation of complex office processes--which fundamentally will involve the design and organization of work, with implications for the structure of modern organizations--the use of computers may continue to be limited to a repetition of the old mechanistic models of management science, identified much more closely with the type of informal systems engineering as practiced by Eli Whitney and Henry Ford, than with the complex system design methodologies which grew out of some of the operations research projects during World War II, notably the development of rocketry, and the development of computers.

Conclusion

This chapter has endeavored to show that a set of ideas, identified in Chapter I as systems analytic methodologies, have a history. These ideas have emerged in an ongoing historical context, in which scientific and technical knowledge has been both applied and generated in service of mundane or practical human endeavors. In the modern development and formalization of the concepts of "system" and "feedback" it is clear that the practical and pure aspects of scientific knowledge are inseparable. Among practical objectives, the occurrence of war can have a dramatic effect on innovation and development of new technologies, and we have seen that military research in World War II provided both a favorable context and the need for developing computer technology, both critical factors necessary to move this technology beyond its "toy" status. The U.S. took the lead in the commercial development of computers following World War II partly because of its fortunate position unscathed among the victors in the conflict, and partly because the type of flexible, well-funded systems engineering sponsored by the government in conjunction with industry and university-based scientists which characterized the operations research movement in World War II was an established blueprint for development as early as Eli Whitney's day.

Moreover, ideas in having a history are also instruments; they are constructive as well as reflective of "reality". As these constellations of ideas differ, so also the outcomes of their instantiation should differ. We will argue that this is dramatically the case in the instance of systems engineering methodologies--especially those applied to human factors research--which were transformed technically and socially by the sheer complexity of problem-solving in World War II. The designer-based model of development, characteristic of traditional American systems engineering, has a tendency to overlook local organizational 'cultures', structural arrangements and technological 'styles', and to overlook as well the process of change, often with adverse consequences for the process and for individuals working in it.

This rich background underlies Wymore's emphasis on the irreducible oscial elements involved in the identification and selection of design alternatives, and it is necessary to extend Wymore's insight and ask how it is that ideas come to be "implemented" in ongoing organized contexts, and how those contexts exert their influence on the nature of the technology-in-use, and, conversely, how the use of such technologies influences the structure of organizational processes in those ongoing contexts, with implications for the quality of working life within those environments.

CHAPTER III

CONTINGENCY

The connection between the concept of models, or formal structures, and that of sets of constraints and regularities represented in organized social systems may be expressed in the following arguments:

1. The heart of the systems analytic conception of organization is contingency, the notion that the value or behavior of one member of a set is dependent upon the value of the others. Such contingency is representable in logical notation, in directed graphs, or in matrices. The concept is, moreover, both relative and reflexive; that is, the value of one element is determined by that of another only with respect to some third contextual value (frame) by which these two elements may be considered related (in time).

2. Referring to the relativistic nature of the concept of organization, the contingency which can be identified or specified by an observer is relative to his model of the system--a third or relational system with respect to which system elements may be ordered and/or compared. As identified in models, systems are contingent with respect to purpose (expressible as some control criterion), which implies outcomes of organizational processes and may imply "intentions" of organizational actors. Systems are also contingent with respect to the observer's knowledge of the possible relations which are symbolized as obtaining, or can be made to obtain, among the various elements

identified as members of the organizational set, such that the total combined actions of these elements are expected (by an ideal observer) to result in certain outcomes, one or more of which may be designated the goal of the system, or of any of its constituent members. Systems, therefore, may be considered well-defined--whether for purposes of design or explanation--only to the extent that they effectively and comprehensively incorporate both social and formal aspects of contingency, or interdependence (recognized as systematicity).

3. Referring to the reflexive nature of the concept of organization, consideration of the goals and means by which the members of a system accomplish those goals implies a knowledge of the set of conventions as to the internal structure of relations describing the system--which structure may be taken to refer to an objectified (consciously and provisionally reified) characterization and idealization of the relations (to be) in effect among the elements of the system over time. The operation of any organized social system is thus a function not only of the formal structure of the model which informs the actions of its constituents, but of the operational translation of that formal system into specific statuses and behaviors--such translation taking place in the context of "objective" outcomes following from earlier sequences of definition and action. This operational translation is itself a social process, operating reflexively on the formal system with respect to the outcomes produced in a given time frame. In practice, the specification of the social and formal/technical relations characterizing an organization is also a "political"

question, involving the relative capacity of one group or individual to effect its definition of the system in material constraints.

4. The reliability of the system in producing its outputs is a function of the predictive accuracies of the indices of action, of the predictive effectiveness of outcome defined in the relation between some set of initial conditions and some set of projected outcomes, previously identified as "objectives," and of the process of operationally translating general objectives into specific activities. Both the efficient operation of the system in producing such stipulated "ends" as may be ascribed to it and the unanticipated consequences which also attend the operation of systems are functions of the definition (modelling) of the system, taking place on two dimensions--formal/technical and social/behavioral.

5. The concept of control and coordination--directionality--applies to the selection and maintenance of one subset from among the total set of operational relations possible for the system over time. Control is enacted through the network of communication described by the status and functional relations among members, and is effected through the application of feedback relations represented in social and impersonal controls relative to some intended purpose or objective (control criterion set) to which the system is directed. In social organizations, this control is embodied in the process of definition and decision which constitutes, operates, and describes the system, including its "payoff" matrix of rewards and sanctions. The description of the structure of an organized social system must, therefore, include a characterization of the formal relations entailed in the plan or policy or theory upon which the system is defined, as

well as the operational relations--or decision rules--and their "implemented" effects by which that definition is realized and maintained. Consideration of structure in these terms illuminates the essentially transformational, morphogenetic--adaptive--nature of living systems responding to the exigencies presented by environmental inputs. This control may be distributed throughout the decision system--represented in managerial and engineering functions--and/or it may be centralized in varying degrees in planning and programmed operations, with a "fully" automated organization at the limit.

The Study of Organizational Change

We argued in Chapter I that conventional organization theories in the fields of sociology and management have been inadequate to explain fully the phenomena associated with complex and ongoing organizations, especially as they undergo technologically-induced change. In particular, such theories are unable to account for structure in ongoing and dynamic systems; neither can they explain the processes by which such structures are produced and transformed. The study of organizational change, is, furthermore, embedded in contexts of purposeful and directed activity--purposeful both from the points of view of the agents of the organization as an object of study, and that of the observer who conducts such studies (who is often a member of some other organization).

Social scientists are accustomed to recognizing the inherent problems of objectivity and reflexivity in their studies of organizations, and part of the difficulty in developing predictive explanations for organizational change may be attributed to the fact that research on organizations undergoing directed change (whether that change is engineered by the observer or not) is inherently fraught with such problems. The dangers of reification are often unrecognized by the designers of new technologies, which is understandable; however, these problems have not been faced squarely by social scientists who tend to "respond" to them by avoiding issues of "real-world problem-solving" in their theorizing, issues which they hand over to the "applied" branches--environmental study, the study of management, social work, and human factors engineering--ducking the issue by a division of labor between "theory" and "practice."

In spite of this timidity on the part of conventional sociology, effective and responsible design and implementation of change in on-going organized contexts require a successful resolution of these methodological problems, problems which must be addressed as an integral aspect of organization theory, rather than as residual caveats on research design. There are, however, special difficulties in conducting this type of investigation--in this case, in studying the ergonomics of office automation--which fall within the scope of traditional issues in social science having to do with an inescapable relativity and reflexivity in the study of organization. We can describe these problems at three levels:

1) For all the reasons cited in Chapter I, the empirical study of complex organization is anything but straightforward, because of the constructedness of organizational phenomena, and because the "structure" of organizations cannot merely be described, but can only be inferred or attributed to the data. Thus the "structure" of

organizations is a function of the "structure" of thought, which varies according to the context of observation--in place, time, and purpose.

2) The study of "high-technology" organizations, and of the development and implementation of new technologies in organizations, involve a further problem in accounting for the processes of directed research, such as those which were formalized in the operations research movement during World War II, and which are entailed in the development and implementation of computer technology--a technology which received its first practical impetus in the context of World War II operations research. Current research in office automation is no less inherently "applied," or directed. Accounting for organizational structure as a product of research directed to certain objectives requires a consideration of the nature of objectives and value criteria associated with the special quality of purposiveness characteristic of research in and about complex organizations--specifically, the type of "local inquiry" associated with the implementation of technological change. These considerations are generally excluded from conventional approaches to organization theory and methods of research on organizations, in which tenets of objectivity require that the investigator begin by taking for granted (and thus by presuming away) the purposes which organize the activities ongoing in the organizations under study, and, by extension, the objectives of research about organizations. The argument is that references to purpose involve one in speculation about goals and intentions and other non-observable "mentalistic" phenomena, thus leading to a third aspect of the problem of objectivity in the study of ongoing organizational change.

3) Carrying out research in and about ongoing organizations is inherently intrusive in two ways. The intervention of the researcher into processes under study through participation in activities associated with the conduct of investigation itself, and/or the application of the products of such research as an outcome of the investigation both tend to impose a sense of order upon ongoing organizations which is derived from a source external to the phenomenon of interest. This amounts to a particularly active version of the traditional problem of reification. Investigators may not only be imposing some "reality" on the phenomenon under study by saying that it is the case that some organization is structured in some way (an unavoidable problem in social science research) but may also be imposing this reality in ongoing contexts by using various methods of inquiry in research and development on new products and production technologies, techniques which have the power of instantiation actually to make changes in organizational arrangements and processes. Research methodologies based upon a rationale of "analysis-by-design" are explicitly "constructive" in this sense, and, therefore, are themselves part of the organizational phenomenon which they purport to study. Unfortunately, the scope of traditional organization theory does not include (and thus cannot account for) the phenomenon of organization at this level of complexity and reflexivity.

Theoretically, then, we must ask how we can account for the influence of context on the process and outcomes of innovation and development--context to include consideration of the assumptions guiding the research activity as well as those defining the organizational context undergoing investigation and change. In what sense is it meaningful to talk of assumptions constraining development? How can we characterize these assumptions with any degree of inter-subjective validity, and how can we account for processes of implementation and, by extension, for constraints on such processes and their outcomes, in a manner which will be theoretically relevant and testable?

We have seen in Chapter II that context can be associated with the notion of "style" or organizational "cultures," which influence the kinds of choices made in developing new technologies. In the operations research movement we described a social movement taking place through the emergence of a set of ideas, which (although they were not all necessarily novel, even at that time) were publicly articulated in strong programs of research and development in the course of World War II. In this way these ideas came to be generally recognized and shared, and in some cases subsequently were codified and institutionalized in new technologies and in new methods of research and development as well. Thus in the unfolding activities in the history of the operations research movement, and in the early development and use of computers in service of practical needs, we see an example of the way in which a set of ideas may come to be institutionalized in new scientific conventions and in novel organizational arrangements, both of which are determined in the course of practical problem-solving.

We have argued that it is just this type of practical problem-solving which is systematically excluded from the scope of sociological theory. We are in the odd position in which our practical experience is considerably richer than our theoretical tradition; and thus, in spite of the significance which has been accorded to the lessons learned in the experiences of "science-in-industry-and-war," and in spite of the proliferation of organization theories in all the disciplines of social science, there is as yet no overarching theory of organization sufficient to the task of accounting for the implementation of technological change in ongoing organized contexts. Although we have (many) design methodologies--of which Wymore's is exemplary--we do not yet have an accepted theory of organization design and development, although the need is increasingly recognized. There are some indications, however, that the long-standing separation of "theory" and "practice" in social science may be breaking down in ways that will permit the emergence of new types of theory.

Partly in reaction to what has been perceived (at least within the social sciences) as an endemic weakness in conventional organization theories and methods of inquiry, and partly influenced by the achievements of the World War II operations research movement, the conception of "organization" in modern organization theories is gradually changing. With the introduction of "systems thinking" into the disciplines of social science, the rationale for conceiving organizations as objects of inquiry has shifted from an emphasis on organization as something (some property or function) added to a set of components, to an abstract and contingent view of organization as a reflection of constraints or limitations on a set of possible structures or interconnections among a set of elements in some context or environment in some period of time. The recognition of organization as a constraint on a set of possibilities is central to the cybernetic definition of organizations both as servo-mechanisms and as socio-technical systems.

This transformation in the underlying rationale for conducting organization research has been described by C. H. Waddington (one of the "grand old men" of early operations research) as a divergence between the "Thing View" and the "Process View" of order in the universe--a rationale which points to the fundamental connection between our view of scientific inquiry and that of organization structure and process. Waddington argues that "...the only way to make a robot anything more than an adding machine is to provide him with a philosophy..." and that the "thing" and the "process" views represent two great philosophical alternatives to the problem of order. The thing view holds that the world is made up of objects, and that changes are really secondary, arising from the way that we notice things interacting with each other. The thing view, which he also associates with reductionism (and, we would add, with the verificationist position in empiricism) is based upon a presumption of a world of unchanging material particles or atoms, the nature of which is discoverable through the methods of the physical sciences. This perspective is thus embedded in a "mechanism" which views living things--including persons--as essentially "very complicated machines."

Waddington argues that in spite of the fact that this view has contributed to our understanding of living things by providing us with a "recipe" for investigation, the thing view provides little but "some rather empty theories about evolution; and hardly anything at all about the mind." The alternative rationale for inquiry is the "process" view, in which the world is conceived as made up of processes, in which the "things" that we observe "are only stills out of what is essentially a movie." According to Waddington, concentrating on the "thing" aspects of living systems tends to lead one to forget that animals develop, and, further, that there is a still slower kind of change implied in the evolution of species. (Waddington, 1977, pp. 17-29)

The "thing view"--which we will identify in the concept of order-presumptive theories of organization--is associated with the conventional paradigms of research which we have referred to as inductive empiricism and general systems theory, neither of which (as we argued in Chapter I), is adequate to account for the process of organization or the transformation of structure in ongoing organizations. Even the question of how to identify structure in ongoing organized systems has yet to be answered satisfactorily by the methods employed in either school.

We rejected the type of descriptive study of organizations which factors a system into its "constituent properties" and reconstitutes its structure on the basis of a correlation of those properties. Whether this decomposition is performed conceptually or experimentally, analysis by simple inductive empiricism destroys the referents of our concepts and measures, and with them any possible view of the coherence or pattern in the system under study. (As we will argue, the same type of inductive methodology when applied to the task of factoring sentences into their constituent elements has tended to transform or destroy the meaningful organization or coherence in sentences and discourses by methods which rearrange words in lists or series according to their grammatical properties.

In this way the logic of inductive inquiry has constituted a serious block to natural language translation efforts in past years.)

We also rejected a conception of organizations as "natural" systems constituted of functions, needs, and other definables. These terms--in general--do not refer to particular systems, and thus the use of this perspective amounts to a translation of our observations into a "systems" vocabulary, the interpretation of which can be quite idiosyncratic and incommensurable. In the same vein we also dismiss typological "theories" of organizations which are constructed on the basis of observed characteristics or properties (such as Etzioni's forms of compliance, or Blau's typology of organizations on the basis of their beneficiaries). This rejection extends to uninterrupted "teleological" theories of organizational development (such as Boulding's) in which a set of ideal stages recreates empty functionalism not adequate to the task of explaining the emergence and assimilation of modern technologies.

The transformation to which Waddington refers, in the ways in which organization is conceptualized, has come about largely since World War II, greatly influenced by the emergence of modern systems engineering methodologies. Although the antecedents of modern systems analysis and automation extend at least as far back in time as World War I (and we have argued that they can be traced in America to the early forms of systems engineering practiced by Eli Whitney and others), the reasons for the recent influence of systems thinking on organization theory lie in two directions, both relevant to our understanding of how organizations themselves are developed and transformed:

1) Systems thinking influenced organization theory first through the ongoing process of technological development and change per se, which was broadly experienced in all areas of modern society. Ongoing technological development became a major factor in the growth of modern organizations in the postwar period and, by extension, a major feature in descriptive organization studies, and an increasingly important element in the education of social scientists and management.

2) Second, many of the ideas and experiences, as well as the formal theories and methods employed by scientists taking part in the operations research movement (broadly conceived) have been formalized and codified in the form of reports in the scientific literature and business press, and in the emergence of computer technology itself, at several levels. In these various institutions, a "methodology of systems engineering" has come to represent a recognized system of ideas, distinguishable from the methodologies of inductive empiricism or general systems theory (although sharing certain assumptions and methods with both, which is a source of some confusion).

In the version of systems engineering methodology represented by Wymore's "tricotyledon theory of system design" we have an especially progressive example of a design approach to systems engineering, which recognizes the irreducible social element in the engineering of designed systems. This social element logically derives from the range of choice entailed in processes of inductive generalization, but is also suggestive of the social process of systems definition central to the process of engineering real-world systems. An especially important factor in the social process of systems engineering (which appears both in Baxter and in Wymore) is the role of scientists and engineers as change agents working in the context of ongoing systems.

Wymore's methodology amounts to what he calls a "language format" for systems design (to be implemented in a computer program) which facilitates both analysis and communication among members of multi-disciplinary systems engineering teams. By providing a means by which substantive ongoing systems can be characterized and analyzed by combinatorial and network (and other) methods of inference which preserve the complexity and rigor in systems analysis, this methodology has the advantage of making assumptions explicit, and of specifying the connecting relationships and boundaries delimiting systems--thus providing a common reference for the complex of activities involved in systems design and implementation. In addition, the recognition of the social element in design allows for the identification of design alternatives and for the explicit selection of criterion values, and thus provides a basis for those party to the design to come to agreement concerning the particulars to be incorporated in the "resultant system."

We are arguing that Wymore's system design framework (as exemplary of the methodology of systems engineering) is an excellent vehicle not only for supporting the design process per se, but for performing preliminary organizational analyses in complex, ongoing systems. Because so much is articulated, and since the presentation of alternatives is captured in "artefacts" produced in the design process in the form of supporting documents accompanying each abstract system, the analyst can trace the parameters of a given system in its boundary-defining assumptions. And in the various protocols or connective assumptions, one can see evidence of the structure in a given system in a given period of time, in a way that comes much closer to an identification of "the mechanism" in ongoing organizations than can conventional organization theories and methods of inductive generalization.

The utility of this approach to organizational research suggests the kinds of lessons about organizations which have been learned from experience engineering systems, for these are precisely the assumptions which must be made explicit in order to predict (and to regulate) the outcomes of systems design and implementation. These outcomes include: 1) the configuration of hardware, people, and activities which we identify with the characteristic structure of some organization in some place and time; 2) the commodities or products, which reflect the values external to the organization as well as the process which is designed to produce these commodities (designed with reference to those external values) as its outputs; and 3) the state of the organization as it is generating those commodities, reflecting the quality of the working environment as well as the hierarchy of rewards associated with individuals in the process.

What we are suggesting in the extension of Wymore's language format for systems engineering teams to be used in investigating organization structure as a social context in which new technologies are implemented is a reflexive adaptation of the methodology of "analysis-by-design" to the process of organization study, particularly in those cases in which the organizations which interest us are those undergoing technologically-induced change. However, Wymore's tricotyledon theory of system design is not a theory of organization, but a design methodology which can function as a tool for organization analysis. He does not entertain in his theory the process by which they are implemented. There is an impression that the work is "over" when the design is complete, and that implementation is a straightforward process of translating that design into action. And although he drops a hint of conflict, he does not suggest how these conflicts are resolved, either in the process of design or that of implementation.

There is thus an important distinction to be made between a methodology of systems engineering, and a theory of organization. As a design methodology, systems analysis can produce a variety of forms, and the methods employed can be used as well to characterize the differences among alternatively possible forms of organization. However, a design methodology does not (necessarily) account for--or articulate--the processes of design and instantiation (implementation) and, therefore, relies upon the addition of a theory of organization to fill in the elements associated with the production and communication of knowledge, the selection of criterion values and objectives, and the actions of persons in these systems at all levels and phases of design and implementation. This is why the state of current organization theory is of especial importance; some such theory must be at least implicit in the design and introduction of new technologies, and the range of available theories is poor indeed, even where organizational assumptions are made explicit. Assumptions of organizational structure may just as well reflect the common-sense experience of designers, managers, engineers and consultants, understanding which is often at best particularistic and which, at worst, reproduces all the prejudices and ignorance of the traditional informal model of engineering and industrialization brought forth from Whitney's day. In order to go beyond attributive theories of organization associated with the design of organizations and to account for processes of research and development as part of the process by which organizational structures emerge and are transformed, we must develop a more powerful theory of organization than is available in conventional--even socially articulate--systems engineering methodologies.

Recent attempts to develop a theoretical understanding of the dynamics of organizational learning (development and change) have been hampered by two fundamental problems associated with the extension of systems engineering methodology into the study of organizations undergoing change.

1) There is a tendency to slip into blind general systems thinking, which essentially is a variant of an outdated and discredited functionalism. This type of explanation is characteristically static and empty of critical content, and tends to be interpreted (by friend and foe alike) as conservative of the status quo. Unfortunately, sociological theory is strongly--if presumptively--identified with this type of thinking.

2) There is also a paucity of rigorous methods for characterizing, measuring, and analyzing the full range of relationships and interconnections comprising the mechanism or structure in ongoing

organizations, partly because these relationships are imbued with choice (or interpretation) insofar as they are constructed or directed to some end, and partly because these relationships are often not explicitly articulated or are expressed in terms not amenable to formalization or quantification.

Examples of the latter problem abound in the "practical" areas of applied social science. Environmental scientist, Bill Sims, argues that the current state of the art in planning and design is still quite primitive, reflecting a mix of knowledge, including techniques from a few relatively advanced fields (such as engineering, economics and geography) and a set of informal verbal descriptions which generally can be described as "untested conventional wisdom." This state of affairs accounts for the fact that we have but limited ability to explain the full range of social, psychological, and physiological "impacts" associated with designed systems of different types as they are implemented in real, as opposed to hypothetical, environments. We can predict some of the physical aspects or dollar amounts of different organizational configurations, in part because these are the areas in which methods are most developed. Conversely, because of the importance of quantification in these design methodologies, the tendency has been to consider only those impacts which can be quanitified, and to overlook as "intangible" or "subjective" those aspects of development which cannot be quantified, and which thus cannot be predicted strictly on the basis of these design models. Moreover, support for further research and development follows those areas which are already well-developed at the expense of efforts to focus on the more difficult areas of social, psychological, and physiological factors in design. (Sims, 1978, p. 71)

Sims advocates an approach to improving the methodology of planning and design which involves a simultaneous effort both to conduct substantive research into the environmental needs of users and consumers, as well as to promote more "procedurally oriented research" which might improve or provide a "problem-solving technology" as a format which design researchers and practitioners might follow in engineering environments which would be supportive of a desirable quality of life. Such a format must overcome a central fallacy in much of our current understanding of (environmental) design, he argues, which is precisely that of focusing selective attention on the objective aspects of design while overlooking the subjective context representing people's beliefs and preferences about the world they live in--beliefs which, no matter how inadequate they might appear on the face of things, still "form the basis for their behavior." (Sims, 1978, pp. 67-68)

We must agree with Sims and others that developmental processes are not well-treated in conventional organization theory, which leans heavily to descriptive and prescriptive accounts of organizations as if they were concrete entities--a perspective which, we have argued, is consistent with the "thing" view of organizations, and which presumes the very phenomena of development and change in which we are interested. We would also agree that the study of organization could profitably be extended to include a focus on the process of research in and about organizations as well as the more technical aspects of design and management. In his empirical study of organizational decision-making, Cyert (1963) referred to "organizational learning" but he did not define what that meant, either in theoretical or in empirical terms. In more recent accounts of the implementation of computer-based technologies in organizations, reference is made to "learning curves" at the individual and organizational levels, but again the concept is not well-defined, nor is it related to the type of explanations which might account for differences in the manner of generating and transforming structure which underlies processes of technologically-induced change. Walter Sedelow comments that it is remarkable, given our dependence on advanced technology and the products of scientific inquiry, that such a comprehensive theory of man-machine systems has not yet emerged, and speculates that one factor inhibiting such a "full system" understanding is an excessively narrow definition of the "focus of attention" for the study of organization. (Sedelow, 1976, p. 218)

There is, therefore, a recognized and growing need, not just for a better organization theory, but for a broader theory of organization, which would comprise both 1) a theoretical framework which could account for the full range of organizational phenomena, and 2) an improved set of methods for characterizing and analyzing complex, dynamic, and often intangible evidence of organizational structures and processes. We would take a step beyond Waddington's distinction of the "thing" and the "process" views of organization, and argue that we cannot make progress by opposing these two views (as has been customary in recent years) but only by integrating them in a way which is empirically demonstrable in ongoing--i.e., living--systems. Thus we will argue that the central theoretical issue underlying organizational research and development lies in recognizing and accounting for variations in form (or structure) both in human social and technical organization and in the organization of thought; and of accounting for the generation (or production) and transformation of the structure of organizations and of thought itself--realizing that "order" is recognized and manipulated in terms of just these characteristics of structure and form, and thus that organization study is essentially social and necessarily reflexive in nature.

Reification and Objectivity

We must ask why it is that we must rely on engineers and designers to develop a theory of (social) organization, and why there is not yet an accepted theoretical tradition in sociology which would "cover" the phenomena emerging from the development and implementation of computer-based technologies, and the emergence of an "information industry"--as an ongoing natural experiment or demonstration of the process of organizational development and change. There are, of course, many organization theories in sociology, but because each defines the phenomenon of interest from within the (constraining and directing) assumptions of established fields of organization study, the theories and findings which result from this research tend to be partial and competing views of the field. We have argued as well that the exclusion of practical considerations from the study of organizations--on grounds of objectivity and validity--has created a gulf between sociological theory and the practice-oriented fields which makes it impossible for us to account for development and change in what are essentially "ideal" structures of organization, standing in pristine opposition to one another.

Operations research--as exemplary of science-in-industry--is by definition directed, or mission-oriented, research. The objective of investigation is to support decision-making, and thus to "cover" the element of choice in system design. Applications of scientific knowledge are based upon an analytic paradigm for generating theories, concepts and methods of data collection and analysis. Computerization is a logical extension of this practical framework, both in the development of computer technology itself and in the use of computer technology as a mechanized support for the information-processing requirements entailed in the stipulations of that analytical paradigm. For this reason, we must account for the processes of research in such contexts in order even to describe the outlines of organizational structure and process.

Where the problem for research is explaining the process of doing science in industry, and on this basis accounting for the outcomes of processes of inquiry on organization structures and outputs, conventional organization theories and inductive empiricist methods of research are insufficient to the task, for several reasons:

1) Such methodologies fail to account for the context of discovery, or for the implementation of the products of inquiry. Traditionally, organization theories do not include the "pre-formal" characteristics of the development of new technologies and new organizational arrangements, nor the penetration of inquiry into processes of organizational change and control.

2) Conventional organization theories are limited in their capacity to explain ongoing processes of development (including the normal operations of a firm) or those of change in existing organizations. Formalization tends to generate simplified explanations of organization, explanations which are too narrow and restrictive to account for the full range of complexity encountered in "real-world problem-solving."

3) Conventional sociological theory excludes considerations of purpose in the interests of the objective validity of explanation, which must be independent of the point of view of the observer--i.e., free from bias.

The problem of objectivity is a central issue in directed research, in which it is necessary to account for values and for choice without biasing the reliability of the investigation. In this sense, the problem of objectivity and reification in organization study involves two separate issues: The object status of the phenomenon of observation, and the status and influence of the observer in the process of investigation. The way in which we view the object of research is critical in organization study, where the distinction between a "thing" view of organizations and a "process" view is especially relevant in accounting for change, especially that brought about as a consequence of the process of inquiry. Insofar as an organization is defined in the process of investigation, it is also important to account for the point of view of the observer in directing that inquiry: Is the observer a member of the organization, an advocate of one position or another, a disinterested (or even hostile) outsider? The observer's purposes in carrying out the research are very much a part of the interactive uncertainty inherent in investigating any social environment.

The problem of reification and objectivity involves the imposition of form through observation in two ways: 1) In the reification of social phenomena in our observation of them, through the categories we recognize; and 2) the reification of social phenomena interactively through the involvement of the investigator in the environment of study. These two problems are inherent in the social sciences, because of the reflexivity involved in studying our own behavior and its constructions, and because human beings construct order out of their experiences by defining and acting upon them. At this level, the criterion of objectivity is unattainable in principle, and the only way of dealing with the penetration of perception and purpose into the knowledge gained is through adherence to an accepted set of conventional procedures defining research as an activity, and through intersubjective confirmation of the outcomes of research by one's colleagues, an explicitly social aspect of knowledge production. These precautions lead us to a third sense of reification, which can be eliminated by rigorous attention to these two strategies, namely 3) the biasing of inquiry by directing the collection of data and analysis of that data to produce and communicate knowledge which advocates or supports a position or outcome which has been decided in advance, and which thus can only be determined on other than observational grounds. Here the objection that the data is reified is not only that the knowledge necessarily reflects the perspectives of the observer, but that it reflects a set of intended objectives, which render the inquiry invalid and the outcomes unreliable.

Thus we see that objectivity can mean a number of things, when applied to the problem of accounting for research done within the context of ongoing organizations:

1) Objectivity can mean the quality of being a "thing," a concrete material object, which implies knowledge ultimately derivable from simple observation of concrete material objects as primitive atoms in more complex observations of systems, consistent with the verificationist position in modern empiricism.

2) Objectivity can also refer to goals, in which "objectives" are conceived as the object of an action on the part of some subject, some desired "end" or a state of affairs (intended to be) constructed out of a set of elements. In this sense, objectivity refers to a social construct produced as an outcome of effort and organization, to which an organization is directed. We have seen that this sense of objective as purpose is necessary to make use of systems analytic methodologies in analysis and design.

3) Objectivity can also refer both to the knowledge we have of the world and to objects as having an existence independent or separate from their observation. In this sense, objectivity refers to a phenomenology implicit in the recognition of objects and events, not necessarily as concrete physical entities, but as phenomena with some ontological status independent of our perceptions and desires. It is this sense of objectivity which is sought (in vain) in formal sociology, and which, we will argue, has such adverse consequences in change situations.

It is necessary, then, to distinguish between bias and directedness in considering the issue of reification in accounting for organizational phenomena as comprising scientific inquiry in service of corporate objectives. When we use the term "reification" to refer to information, we may be implying one of two possible ways in which information is reified: 1) In the sense of knowledge as a social construct, imbued with purpose and limited by the scope and process of observation; 2) or in the sense of knowledge being biased and unreliable, which refers to the conduct of inquiry and the conditions of observation supporting the intersubjective validity of the findings. Merely advocating a position, or engaging in directed, mission-oriented research does not automatically entail an invalid reification of the phenomenon under study, as long as established criteria for observation and confirmation are upheld. The major problem in conducting scientific investigation within corporate contexts is advocacy--in the case of human factors or ergonomic research in office automation, the problem is shared both by unions and management. In each, there is a deliberate orientation of the inquiry which is based on a partial "theory" of the firm, and which renders the outcomes invalid beyond the limits of scope imposed on the study by pre-determined objectives, and thus renders the knowledge relatively useless by biasing the outcomes and systematically overlooking discrepancies. As a description of the structure of ongoing organizations, such reification obscures (presumes) organizational realities. Furthermore, as a description of the process of developing plans and methods for carrying them out, reification in this sense is not merely a product of observation, but a process in action. It not only cannot be eliminated--invalid or not--but must be accounted for within any complete theory of organization.

Thus the norms of objectivity, as an ideal for scientific inquiry, withhold legitimacy from that knowledge which is produced in order to support some objective, knowledge which stands as an ad hoc conclusion to be proved (upheld) and which is not subject to test and falsification. In Popper's and Hempel's terms, this means that such knowledge cannot function in a law-like capacity, meaning that it cannot support prediction (much less control) of outcomes. Withholding the attribution of validity from the knowledge thus gained is not enough, however, for as we have seen in Chapter I it is logically adequate for a designer to specify a set of conditions in which the hypothesis could be instantiated, in effect producing the scope or domain which would be confirmed by the data after the fact. It is a short step, as Beer has pointed out, to searching out those environments in which the assumptions in the model can be supported, and then limiting construction to those environments, or alternatively to controlling and constructing the requisite conditions in the environment by more or less explicitly coercive measures. (Beer, 1959, p. 21)

Reification in the pejorative sense, as we have argued, carries the implication that ideas are "merely" subjective beliefs, which are projected (or imposed) upon other thinkers on no other basis than self-reflection (and/or self-interest) which is subject to bias, advocacy, and error. To reify in this sense is to attribute an objective "reality" to one's own observations, first by externalizing them and thereby removing them from the context of observation, and second by imposing them on others through some form of presumptive (if not coercive) universalization. The purposeful imposition of such ideas overlaps with the constructedness of that type of scientifically-grounded knowledge which can be said to be universally valid, and therefore renders the invalid sense of reification inseparable from the acknowledged relativity of perception and action. We must go a step beyond criticism, then, and account for the phenomenon of reification itself in order to incorporate the reality of directed research in our theories of organization.

People objectify their ideas in the course of expression and acting on them, which, at least for the actor (speaker) constrains one's awareness of possibilities--a constraint which is generally recognized as order. Furthermore, ideas become intersubjectively verified in communication, and at some point their expression reflects that social agreement in the establishment of conventions, the simplest of which is giving a common name to a shared experience. Finally, ideas, once recognized and named can be codified, externalized in some enduring form which establishes a physical representation of that agreement implicit in naming, and which is further reinforced as the reference for action at all levels. These social processes produce implicit constraints in the environment of action through the patterning of understanding which underlies the actions of people in those environments.

Analysis-by-Design as a Form of Productive Reification: The "Process View" of organization is associated in many ways with a socially-grounded, constructivist, relativist view of organizations as provisionally-reified structures of thought. In this view, the focus is on the manner in which social and technical realities are created (or constructed) over some period of time and in some context, defined in part by people's perceptions of their situations as somehow problematic. Understanding organization as a process makes it possible to take into account the directedness and intrusiveness associated with research in organizations, and to be aware of the reflexivity in our understanding, to be conscious of the process by which organizations are transformed through the activities in which we engage in inquiry, both as organization members and observers of organizations. This view provides a basis for extending organization theory to an investigation into "real-world problem-solving," and thus to account for the role of particular theories as they organize thought and action in the context of a larger process of adaptation in living systems taking place at the level of human culture. We adopt here such a relativist constructivist perspective on organization theory, which perspective is grounded in the sociology of knowledge, and which explicitly treats science itself as a proper object of investigation--both as systems of knowledge and as processes of inquiry.

The methodology of analysis-by-design, as the logic of inquiry in designer-based systems engineering, demonstrates W. I. Thomas' assertion that that which is believed to be real, is real in its consequences. It also illuminates two different senses in which the problem of reification can be interpreted. Reification in the conventional sense cites a criticism of some descriptive explanation that it attributes to "the data" existent characteristics which are not "real," a criticism generally based on the charge that understanding is biased by the particular interests of the observer. It is in this sense that Benson criticizes organization theory and research as inherently biased, and biased in the favor of management.

Benson argues that research on complex organizations has been based upon a positivist methodology and a series of rational and functional theories. These "scientific theories" are largely representative of participants' explanations for organization structure, and for this reason "are tied to and tend to affirm the present realities in organizations." Such theories cannot develop a critical posture because any significant transformations in organizations would "undermine corresponding theories." (Benson, 1977, p. 1) In this sense organization theories function as hyperstable belief systems--i.e., as ideologies.

Benson has special disdain for the work of Blau, Heydebrand and Stauffer, who he represents as exemplary of the conventional school of organization theory. In conventional organization research, he argues

> The pattern of correlations is ... explained as a result of a rational or functional arrangement of the organization's parts. The actual process of adjustment, the sequence of events producing the pattern has not been observed but has been inferred to be one of rational or functional adjustment. (Benson, 1977, p. 11)

This type of "rational reconstruction" of organization structure is necessarily limited, analysis "ripped from its historical roots and societal context." Conventional research methods then cannot avoid implicitly accepting as guidelines the conventional presumptions of order in the system being studied, and for this reason cannot focus on those "extra-rational" processes lying outside the official definitions of structure. (Benson, 1977, p. 11)

While we would certainly agree with Benson's criticism of conventional organization theories and the methodology of positivism (which we have referred to as inductive empiricism) we do so on lotical and not ideological grounds. The relativity of perception and the constructedness of organizations are issues separate from questions of bias and conservatism, a distinction which is based on the dichotomy between the thing and the process views of organization. The processes of developing and transforming organizations are far from "extra-rational," and the constructs which emerge from those processes are eminently "real," and not merely the unfortunate manifestations of subjective--and therefore self-interested--beliefs.

However, while crude inductive empiricism can be charged with simple reification in the sense of attributing to the "status quo" some enduring reality, newer methodologies of organization research exhibit a more substantial form of reification which cannot be dismissed so easily. Analysis-by-design, as a process of making something real in order to study it, definitely qualifies as a kind of reification which is far from ephemeral or merely an artifact of analytic reasoning and/or bias. The connection between ideas and artifacts or action lies in the act of definition, by which some understanding of reality is expressed, and is therefore made observable. As an outcome of the process of definition, "a definition," as instantiated in some material form, is an objective reality in at least two senses:

1) It is expressed by someone in some medium, which renders it concrete--i.e., gives it an existence independent of the instance of observation--as a physical phenomenon of marks on a page or a verbal utterance. When others use that definition, or express similar utterances, they are attributing to that idea a reality which is founded on recognition and use as tacit agreement. That idea can, moreover, be transmitted without necessarily being present in some person's consciousness--for example, as it is printed in a book or stored in some electronic medium. This view is consistent with Weber's concept of "sociological verstehen," or understanding, expressed not as specific ideas in the mind of some individual actor in some place and time, nor even the average or aggregate of ideas held by an identifiable group of actors, but as expressed in some ideal-typical view of the world as representative of the knowledge characterizing a given context or situation in which actors participate.

2) Ideas can also be made real in the consequences of action which is oriented to them. This is an area in which the type of (invalid) reification which attributes independent ontological status to the presumed objects of one's own ideas shades into what we might refer to as a "provisional reification," meaning the instantiation of an idea in some form, including action, a process which may or may not be conscious on the part of some actor. In procedural knowledge, for example, we can see a direct relationship between ideas and action which renders those ideas real through instantiating or embodying them with reference to some algorithm, or

set of instructions, on the basis of which behavior is routinized in producing some object, action, or idea. Thus ideas can become commodities, and ideas can be presumed to underlie behavior, with some productive force.

Organizations as Constructs of Human Consciousness

Given a "process" view of organization, and an appreciation of the methodological difficulties involved in studying phenomena which are inherently subjective and dynamic in nature and interpenetrated with purposeful intervention, the least reifying perspective one can take on the study of organizations is to focus on their historical status, and on the importance of organizational methods and processes in generating a succession of organizational forms--which we recognize as organizations. David Silverman defines organizations as artifacts which 1) arise at some ascertainable point in time, and 2) are consciously established to serve certain purposes. As artifacts, organizations can be characterized by a "patterning of relationships," which patterning is not only not taken for granted by members of organizations, Silverman argues, but which is itself a focus of considerable attention as people plan, debate, and execute changes in social structural relationships and in the "rules of the game upon which they are based." (Silverman, 1971, p. 14)

The "reality" of organizations, therefore, lies in their recognition as such by some relevant audience as expressed in the literature of a given field; and as embodied in methods and practices employed by corporations, and in the actions and expressions of organization members over time. In the absence of such "recognition," these institutional entities do not exist in any meaningful way; recognition attributes existence and order to the presumed objects of people's observations. Without this recognition, commonly ephemeral social collectivities--such as incipient firms--fail to achieve an "objective" status. In the U.S., the historical status and social recognition of corporate organizations can be seen in the institution of the corporation as a legal entity (created as a by-product of the passage of the 14th amendment), and in the establishment of the practice--and profession--of financial accounting (created as a by-product of the institution of corporate taxes in 1887). (Cochran, 1977, pp. 64, 82)

As these examples suggest, social recognition must be augmented by a process of formalization and codification of that social recognition in order that incipient firms of would-be entrepreneurs may achieve institutional status as corporate entities--or formal organizations--ratified by charter, bounded by obligations of taxation requiring systems of recordskeeping and accounting, and further defined by other forms of (legal) relation established among buyers and sellers, employers and employees, and corporations and communities--all established as a function of the relationship of the corporation to the state and federal governments as the source of legal recognition.

Just as organizations may come to exist as corporate entities through the public and legal recognition of certain patterned social arrangements, it also happens that groups and organizations which have enjoyed recognized corporate status can lose that recognition, as in the event of bankruptcy, which reflects the withdrawal of support and affirmation by external legitimizing and regulating agencies. In cases of bankruptcy or corporate divestment or acquisition, the organization in question does not "cease to exist" in the sense that its physical plant, personnel, and products suddenly disappear or are destroyed. Rather, a given named organization, recognized in terms of certain characteristics, is transformed in such a way by business failure, death of a principal, or natural disaster, that it ceases to exist as recognized. Its parts may be rearranged, or redistributed through other systems, or replaced in such a way that an entirely different recognized system--or several such systems--may take its place. A common occurrence among certain types of voluntary organization--such as churches and political parties--is the splitting up of a recognized organization into parts, with cleavage falling on the lines of disagreement among the members over certain issues, issues which as Silverman suggests are generally articulated as the focus of controversy. Those issues--as they are defined by the relevant audience of any given organization--should be the basic focus of the study of organization, for it is through this articulation that the arrangements which we observe are constituted and rendered meaningful, and thus that organization as such is recognized and acted on.

This "subjective" element implied in the recognition of organizations as corporate entities was an important focus of Max Weber's now-traditional theory of organizations. Weber defined organizations as systems of "continuous purposive activity." He distinguished corporate organizations from other forms of organization on the basis of an explicit recognition of a bounded social relationship 1) which is defined by a restrictive admission of outsiders according to certain rules, and 2) which is governed by an administrative staff whose function it is to enforce the corporate order in the interest of ensuring the continuous purposive activity to which the organization is directed. (Weber, 1947, p. 151) Rational bureaucratic organizations are further qualified as those organizations based upon the acceptance of the legality of a codified body of rules--or formal structure--which is specified on the basis of an identification of offices and a set of conditions which individuals must meet in order to fill them.

While Weberian organization theory is (today) conventionally associated with an articulation of those rules determining (bureaucratic) organization structures, it is important to emphasize the subjective basis of the association of individuals with corporate order--an association which for Weber is to be found in the meaning which that organization holds for the individual and which thus underlies and informs his actions. Weber suggests that the concept of such collective (corporate) entities as states, business corporations, and associations has "a meaning in the minds of individual persons, partly as something actually existing, partly as something with normative authority." (Weber, 1947, p. 102) Whenever an individual orients his action to certain rules, the social relationship implied in that orientation thus constitutes "an order;" whenever action is meaningfully oriented to a set of norms or beliefs which define that action as somehow binding and desirable, then we may speak of a "legitimate order"--a phenomenon reflecting something more than the mere redundancy or patterining of social action. (Weber, 1947, p. 124)

Holzner accounts for the phenomenological status of organizations in terms of the meanings which persons attach to situations and on which they base their actions. These meanings, he argues, are not "given" in the environment, but are a product of active definition on the part of observers, who "...assign a variety of meanings to their environment in order to interpret it and render it meaningful for their actions." (Holzner, 1979, p. 81) Reality is socially constructed by actors in the process of attributing order to their environment. The stability reflected in this socially constructed reality reflects the fact that the understanding of our experiences ultimately rests on an "externalized version of what is taken for granted as objective reality." (Holzner, 1979, p. 85) Thus social reality is constructed in action, and objectivity is attributed to that reality after the fact.

The meanings expressed in these social realities, according to Holzner, are inescapable sources of data for sociological analysis--which should be directed not to a descriptive study of beliefs and attitudes, but to an analytical study of the "shared construction and social diversity of experienced reality." The knowledge which is the object of inquiry is that which is considered to be knowledge by those possessing it; and this can only mean, he argues, the "mapping of experiences of reality by some observer" rather than the "grasping of reality itself." (Holzner, 1979, pp. 93-94) Organization study should proceed from the recognition that it is in their symbol creating and symbol using capabilities that humans are capable of transcending the limitations of their "immediately given" environments, and Holzner advocates what he calls a "reality constructionist perspective" in which

> Systematic attention to the processes by which observer and subjects come to share the reality of their worlds and identities--producing the intersubjectivity of reality--is the fundamental basis for the study of social action.... (Holzner, 1979, p. 83)

This focus on the intersubjective--or phenomenological--status of organizations as recognized corporate "orders," reinforces Burns' argument that organization study should not be based upon descriptive classification of observable characteristics, but upon an explication of these (presumed) understandings, which render these characteristics meaningful both to members of organizations and to their observers. Thus

> The objects for classification are not organizations or parts or attributes of organizations but analytical concepts and frames of reference within which methodological procedures can be designed and comparative studies usefully made. (Burns, 1967, p. 127)

Reification as the Separation of Knowledge from Knowing: Given an explanation of the ontological status of organizations as instantiations of some model, it is necessary to inquire into the relationship between organizational forms as instantiated and the process by which they are produced--a relationship of central importance in analysis-by-design and systems engineering. The discontinuity between an order-presumptive "thing view" and an adaptive "process view" of organizations can be observed in sociology as well as in cybernetics and engineering (and the other modern "information" sciences party to the analytic tradition).

In 1966 Berger and Pullberg presented the thesis that sociological theories divide into those which define society "as a network of human meanings as embodiments of human activity" and those which conceive of society as a "thing-like facticity, standing over against its individual members with coercive controls and moulding them in its socializing processes." In this contrast, they argue, the fundamental problem for sociology is determining the manner in which "subjectively intended meanings become objective facticities," and they argue for a phenomenological view of society as a dialectical process whereby human activity creates a world of things, which may come to be attributed ontological status in their own light, and which--perceived in this way--may constrain human possibilities in spite of the constructedness of social life. (Berger and Pullberg, 1966, pp. 56-57)

Berger and Pullberg distinguish between objectivation and objectification, as anthropological concepts referring to the instantiation of human consciousness in material and social artifacts, common to all experience; and alienation and reification as products of human activity from human consciousness, thus representing social structure as coercive instrumentalities. Objectivation refers to the process of translating subjective ideas into products commonly available to people in the world. Within that process a moment of objectification occurs at which man recognizes the object he has created, making of it an object of consciousness--a commodity, or idea in itself--and thus establishing the distance between the product and his creation of it by his activity. Objectification is man's recognition of the existence of the things he has made, which he ratifies by giving them names. Alienation, then, is the "process by which the unity of the producing and the product is broken," which amounts to a forgetting. It is this forgetting which is the key to reification. In reification, the reality is attributed to the object in itself, and objectness is attributed to the standard of reality, forgetting the realization--the objectivation which brought about this result. (Berger and Pullberg, 1966, pp. 60-61)

In the construction of intelligent machines--automata--Zopf notes that what we recognize as intelligence--and thus as control over the process--in a machine is a product of our forgetting that this machine is produced out of a fully determinant blueprint, created by persons.

> We must know in full deterministic detail what we are doing to build a complex machine; to call such a machine intelligent, requires that we forget or ignore our knowledge of just how it does what it does. (Zopf, 1962, p. 328)

In this way the intelligence of computing machines comes to stand over that of the human user. The machine does not have consciousness or self-awareness of its own processes; rather, as a finite state automaton it embodies its processes in full, just as the human embodies his physiological processes without awareness in most cases. However, the human can gain that awareness, can reflect upon himself in a fashion which the finite state automaton cannot without adding something to the system.

Stated in just this way, our fears--common in the 1950s and 1960s--of reifying human consciousness in intelligent machines which would then stand independent of--and in some possible sense, in opposition to--human consciousness and purpose, have little grounding in the kind of machines which we are (now) actually capable of building. The problem is closer to common sense. It is not the case that "the machine" will replace human consciousness, but that the design of the machine, and by extension the determination of the division of labor and tasks and working relationships, will be made ever more remote from the activity of the work itself. When we attribute intelligence to computers, we recognize that we are unable to explain their internal functioning; however, the functioning of the computer can be explained by its designer, whose ideas are embodied in its architecture and processes. It is this person from whom the ultimate user is remote, which is a matter of social structure--and not the loosing of a "Golem" with super-human powers.

What is being reified here is two-fold: First, the consciousness of the designer of the machine is embodied in its construction--in the sense which Berger and Pullberg define as objectification (when viewed from the perspective of the designer) and objectivation (when viewed from the perspective of the observer, a person who is aware at least of its constructedness, and possibly also of the manner in which it operates). Second, in designing the machine, the designer incorporates ideas (which he gains by participating in his own culture as a person with a certain social status) into the design for the machine, particularly as regards the applications and objectives for which the machine is designed.

Where the machine is designed to replace or to supplement human work, a model of the human and of the work is also embodied in the workings of the machine, and this constitutes a second objectification--in this case, a reification of the conventional social roles existing within the larger social structure. In designing and marketing word processors (particularly in the designer-based mode of development) the features of the hardware and its applications software are built with a model of the role of the unskilled clerical worker in mind. From the point of view of the unskilled clerical worker who will someday fill that role, the design and construction of that machine embody a reification of her role in a set of assumptions built into the design of the machine, assumptions which will constrain her to that static representation as long as she works with it. Thus, to the operator, the machine is a controller and not a tool.

However, Berger and Pullberg argue that as a social being, man creates and names the world with his very activity. How can man at once be the creator of his world and oppressed by it?

> The reality of such a world is given neither in itself nor once and for all. It must be constructed and reconstructed over and over again. That is, the world must be continuously realized, in the double sense of this word, as actualization and as recognition. (Berger and Pullberg, 1966, p. 62)

The social structure which makes up the statuses and positions people fill is part of the produced, objectivated world created by human beings, and thus, they argue, "social structure is nothing but the result of human enterprise." As such, the social world presents a wide horizon of possibilities for social action, as a medium for creativity and the product of human activity in each moment in time.

However, our experience of the world as limiting, of social structure as a constraint on human possibilities and ultimately as a coercive instument standing independent of human will and human needs, points up the role that consciousness, learning, and forgetting play in sustaining the reality of a constructed world of human choice. Experience is consciousness aware of itself, and this self-reflexion or self-knowledge is broken up into "finite provinces of meaning," each representing the context in which particular types of action take place. Experience is always incomplete, and through experience man creates the world continually, not once-for-all. If we humans create the very world which coerces us and from which we are alienated, this can only be through consciousness--or through our knowledge of the world--and this happens, according to Berger and Pullman, by a devaluation of the human in favor of the objects created by humans, and in the process a devaluation of human activity itself. It is this kind of consciousness--and not consciousness in general--which is reifying, and it is the objects of this devaluating, de-humanizing, consciousness which are reifications. (Berger and Pullberg, 1966, p. 64)

Reification in this sense--in contrast with the objectivation which goes on in all creation of knowledge and construction of objects and institutions--is a de-humanization of its object, whether that object be a tool or an institution. In this de-humanization, human beings are defined as embodiments of abstract qualities, and thus become fragments in their own consciousness, their existence distributed throughout these provinces of meaning, and stripped of its self-consciousness. At the perceptive level of "pre-reflective" experience, reification takes place by the alienation of expressions from the intentions of the actor. At the theoretical level--in which actors reflect on the world of their perceptions--reifications (of pre-reflective and pre-theoretical experience) become themselves reified and rigidified into dogmas, which limit our possible options for understanding the world. Theories and explanations become reifications because they are cut off from their authors--and thus from experience--and presented as facts-in-themselves, and their authorship is forgotten in a series of ever-remoter references, such as takes place through generations of schooling.

The objection is not that such knowledge is "false" but that it is separated from experience, and in some sense comes to stand above it. Given Berger and Pullberg's analysis, reification takes place when we attribute ontological status to roles and institutions as detached from human expression and intention, and which thus stand over and above human experience--which happens when we eliminate the recognition of the irreducible subjectivity and social embeddedness of knowledge and action. What is lost in the translation is choice--or, more specifically, the consciousness of choice--which is false consciousness. The businessman is not, as Sombart claims, to himself the author of the applications of technology; he is the instrument through which the economic system acts rather than its creator. Thus in the reification of these roles and their activities and outcomes, human actions cease to express human meanings but instead come to stand for a body of abstractions which they allegedly embody, but which stand apart from human consciousness, as independent "facts of nature." (Berger and Pullberg, 1966, pp. 65-66)

Reification operates on our experience, and the end result is that the sense of the constructedness and dialectic of social life is lost and replaced by a sense of mechanistic determinism; self-regulation is replaced by external controls, which are more distressing by far than mere physical hardship, because this externalization removes all dynamic and thus all hope from our consciousness of social order.

> Reification, on all levels of consciousness, converts the concrete into the abstract, then in turn concretizes the abstract. (Berger and Pullberg, 1966, p. 67)

In so doing, reification of consciousness rigidified those aspects of the world which are taken for granted, and none moreso than customery social roles and institutions, in which reflection and choice are objectified (by someone other than the actor!) into prescribed channels, converting action into process, which defines action without the actor, and which is thus intrinsically de-humanizing. (Berger and Pullberg, 1966, p. 67) In the design of word processing equipment, in the predominant designer-based mode of development, we are not only producing machines which correspond to social roles and relationships which are taken-for-granted--and which imply a forgetting of fundamental controversies in the division of labor, already contested long before the introduction of computer technology--but we are also reifying the methods and concepts by which these roles are defined and taught to people and embedded in the definitions of tasks, jobs, and working roles and relationships.

Provisional Reification

How is it possible to account for this objectivation of human knowledge and experience in the development of technologies and institutions which we see embody a set of assumptions--now forgotten--in constructs and artifacts which stand in some sense apart from human consciousness? We have argued that we cannot discover in any straightforward manner those ideas or models which we presume underlie social behavior, although Whyte, for example, argues that all action is based upon some type of underlying model.

> When we set out to build an organization, we have in mind a theoretical model of that organization. When we set out to change an organization, we have in mind a theoretical model of what the reorganized structure should resemble. (Whyte, 1967, p. 22)

The problem of objectivity in social science emphasizes the difficulty of identifying a set of ideas (or model) once objectified in material artifacts and social structures. Ideas cannot be inferred from action alone. We cannot simply describe these underlying models or understandings (to which Whyte refers), for the same reasons that we cannot identify organizational goals from simple observations of conventionally-recognized organizations--reasons which Weber noted long ago, and which Silverman has reiterated more recently: Actors may not know or be aware of these underlying models, and thus will be unable to articulate them; there may be any number of these models operative at a given time, not necessarily mutually compatible with each other; actors can lie and be mistaken; and, finally, observers may infer in any given situation any number of hypothetically possible models (and goals) which, consistent with the notion of equifinality, are all equally possible of leading to some observed actions and outcomes. According to Silverman, these problems underlie several--equally unsatisfactory--alternatives for identifying goals as a basis for identifying organization structure: accepting as definitions of goals those originally stated, those currently held by the leadership, those which can be inferred from behavior in different parts of the organization, and those which can be inferred from constraints or requirements on the part of the organization as a whole. (Silverman, 1971, p. 8)

The problem of objectivity, then, is that we know (through introspection on our own experiences) that people create ideas out of their efforts and interactions with others in some historical and cultural context, and then act on them, and that this activity is the basis for adaptation among human beings, and the basis upon which they construct artifacts, including social organization in all its manifestations. However, these reasoning processes and models of reality are by definition subjective, and we can only know of another's reasoning by indirect means, in their utterances and actions, and those means are notoriously unreliable.

The traditional position on the problem of objectivity in social science has held that this subjectivity is inherently ideological in nature and thus bound up in bias, error, and self-interest.

Subjective knowledge is, therefore, illegitimate as a basis for scientific understanding, and the question of goals is customarily suspended in the absence of any reliable method for taking this subjectivity into account. On these grounds, conventional methods of inquiry sought to exclude as much of the subjectivity as possible in inquiry, suppressing the objectives of actors and of researchers, avoiding any references to ideas or intentions on the part of subjects, and concentrating on controlling observations in such a way as to nullify the effects of this subjectivity--largely through quantification, and the myth of value-free (and thus context-free) investigation. Thus the methods of positivism--or inductive empiricism--are employed not just in order to preserve a biased view of the status quo, but also in order to protect against a biased view of organizations in the process of conducting research from a point of view within the status quo.

This paradoxical situation is indicative of the logical reasons why formal social science has so far been unable to account for real-world problem-solving. Whether because the context of research is overlooked due to a presumption of order based on adherence to the status quo, or because it is overlooked due to a desire to avoid biasing the research by avoiding subjective expressions of that status quo, formal theories and methods of social science fail to explain either the form or the process of generating social structures, because each categorically excluded subjective information as well as information which is specific to a particular time and place, in an effort to render their analyses universal in scope--and thus objective. Presumptions of universality and objectivity, however, force us to see patterns in human behavior and society as the outcomes of blind forces, referring to some underlying but unspecifiable social--or natural--order. This epistemological position--which underlies both inductive empiricism and general system theory--obscures the development of a truly substantive theory of organization in a presumption of some underlying (and unobservable) order. The failure to develop organization theories which are at once objective and which can account for empirical realities in specific contexts is thus a consequence of 1) a presumptive methodology lifted from a 19th century conception of the physical sciences, based upon the desire for objectivity thought to be attainable through formalization, quantification, and measurement; and 2) a closely related theory of organization which translates objectivity as universality--meaning independent of any particular context.

An alternative position on objectivity and reification begins by accepting the inevitable subjectivity inherent in organizations viewed as socially constructed, and contingent upon the constraints and opportunities present in different contexts or environments. Given the constructedness of social organization, three distinct schools of thought can be identified in sociology, each of which accounts for the emergence of the social structure of organizations in social action: A dialectic or conflict approach, an entrepreneurial or managerial approach, and the contextual approach of the (classical) sociology of knowledge.

The Conflict Approach: As a representative of the conflict approach, Kenneth Benson argues that organization theories are inextricably involved in the construction of organizations, and that as such they are a product of the social context in which they are developed and of the practical interests of those who create them. "There is then a dialectical relation between organizational arrangements and organizational theories." For this reason, he advocates a dialectical perspective in the study of organizations which focuses on the processes through which actors establish spheres of rationality (i.e., those processes through which production, reproduction, and destruction of organizational forms takes place) and which grounds the study of organization on the recognition of organization theories as guidelines or models for administrative control. In this perspective, these theories represent solutions to organizational problems of technical and practical importance, and serve to support prediction and control, in the sense we have been arguing is central to the phenomenon of directed research as a fundamental element in the constructedness of organizations.

> Dialectical analysis of organizations, then, should be concerned with conditions under which people may reconstruct organizations and establish social formations in which continuous reconstruction is possible. (Benson, 1977, p. 18)

Dialectical theory is based on the notion of process, and views the social world and its apparently fixed patterns and arrangements as only one among a number of possibilities in a continuous state of becoming. The object of dialectical analysis is to identify those "fundamental principles which account for the emergence and dissolution of specific social orders." This emergence and dissolution of form takes place as people construct their social world through their interactions with each other. Out of this interaction social patterns and institutional arrangements are established, modified, and ultimately replaced. These patterns are constructed in the context of mundane, everyday tasks and problems viewed from within the constraints of the existing social structure. In this process the efforts of individuals to overcome their present limitations may ultimately bring them into conflict with established social structures, and the interests of those favoring reproduction of those structures, and the outcomes of this social conflict may well lead to social change. This change is sometimes planned and coherent, and sometimes it is not; where it is planned, it is based upon a recognition of the limits of current social structures and their purposeful rearrangement. (Benson, 1977, pp. 2-3, 17-18)

In this approach, orderly patterns are seen as representative of a wide range of possibilities, the determination of which depends upon human action--or "praxis"--a concept which emphasizes the possibility for deliberate human intervention and transformation of social structure, and emphasizes the role of human choice and responsibility in constructing and reconstructing social relations. Thus organizations are always in a state of becoming, "...a product of past acts of social construction..." in which the notion of praxis reflects "...the free and creative reconstruction of social arrangements on the basis of a reasoned analysis of both the limits and the potentials of present social forms." (Benson, 1977, pp. 5-6)

Because organizational arrangements are seldom clear-cut, and because organizational components are interconnected in complex ways, dialectical analysis addresses the study of social arrangements as complex wholes by focusing on those events or divisions of social structure associated with divergent and incompatible productions and relations of dominance. In the process of social construction, Benson argues, a social order is produced which contains contradictions and incompatibilities in the social fabric--inconsistencies which make radical breaks in structure possible. In some cases, these contradictions may be destructive of the system itself (in the sense in which Marx developed so fully) frequently bringing into conflict established systems of economic relations with advances in productive forces or technologies. (Benson, 1977, p. 4)

These contradictions come about because in the process of social construction--or rationalization--many organizational elements are typically overlooked in the development of a "rational plan." Furthermore, the process of rationalization itself creates structures--or patterns of interaction--which may themselves entail significant internal contradictions which may resist further rationalization. One way in which this differentiation comes about is in the separation of divisions within an organization, and a concommitant separation of reward and control structures into divergent spheres of social action. In these separate domains, people develop their own distinctive models of the organization as reflections of their experiences, experiences which are embedded in a particular occupational or departmental context. Thus is is that large and complex organizations can comprise a great many structural inconsistencies, and as Cyert argues encompass a number of distinct and often conflicting goals and objectives. (Benson, 1977, p. 14; Cyert, 1963, p. 43)

Given these (conflicting) possibilities for action and social construction, Benson shares with Krupp the recognition that those ideas which underlie the social constructedness of organizations vary with the relative power of different organizational participants to control the direction of the change process. (Benson, 1977, p. 7; Krupp, 1961, p. 172) Dialectical analysis thus focuses on those positions in which incumbents enjoy power or influence over situations and/or other people, asking the question,

> How are some groups better able than others to extract advantages and privileges from the organization. (Benson, 1977, p. 8)

This power or influence does not necessarily follow established lines of authority, he argues, first because those with the authority to implement some model of organization enjoy a power base which extends beyond organizational boundaries, and thus cannot be accounted for from within the context of organizational authority. Furthermore, those in positions of authority may themselves introduce innovations which generate alternatives to present organizational arrangements, innovations, which may even contradict established patterns of organizational authority per se. Thus

> Increased use of computers for purposes of coordination and control, new budgeting procedures, and other innovations from above may stand in opposition to previously constructed arrangements. (Benson, 1977, p. 14)

This type of dialectical analysis which Benson advocates--emphasizing the centrality of some underlying process of social construction based upon theories of organization held by participants--seems to be identical with what we are here studying as the process of implementation of change in ongoing organizational contexts. In implementation studies, the introduction of some technological (or legal) innovation into an ongoing organization serves as a "natural experiment," demonstrating those events and processes which are associated with assimilation of novel inputs. Benson's objective is to explain the ground upon which rational and functional processes currently in effect have been established. In the case of office automation, this perspective suggests an inquiry into the origins of the official definitions or order associated with office technology--including the configuration of positions and roles, and the qualifications and benefits attached to those roles. However, Benson falls back upon a presumption of some substructural network (of influence) to account for what he considers to be a "...nonrationalized sphere of organizational action, a complex network of relations linking participants to each other and to the larger social world...." This nonrational sphere, he argues, underlies the formation of latent social systems within organizations, and thus is reflected in the emergence of organizational arrangements and processes which affect the process of social construction by continually producing tensions and conflicts. These conflicts limit the possible alternatives which can be actualized in any given time, by producing crises which may initiate change, and ultimately, by defining the boundaries or limits of the organizational system, viewed as a rational sphere of action. (Benson, 1977, pp. 12, 16)

Benson's opposition of the rationality of organizations and the nonrationalized sphere of organizational action from which these tensions emerge shows an order-presumptive aspect to his theory of social construction of organization which can be seen as the familiar concept of "informal organization" necessary to account for those aspects not included in his exclusive focus on formal--official--definitions of organization structure defined in terms of patterns of authority. At the same time, he focuses only on conflict phenomena as built into the relation between this rational and non-rationalized dichotomy in the process of social organization.

We will argue that it is possible to view these underlying theories of organization as comprising a number of alternative presumptions about organizational form and process, that organization theories can be either presumptive of some underlying order or based upon an explicit presumption of the adaptive constructedness of order. Moreover, processes by which order is constructed in ongoing systems may serve to preserve the existing structure arrangements, or they may contradict and undermine that existing structure. Breaking out these alternatives makes it possible for us to recognize a range of alternative outcomes as following from the introduction of technological change, outcomes which are determined not only by the form of that technology in relation to a given context of application, but also by the process in which that technology is introduced--especially the direction of that process as it serves to bring about change or to stabilize current arrangements. Thus we would argue that applications of computer technology, such as budgetary control models and other transactions-processing methods, not only do not necessarily stand in opposition to established structures of authority and control (as Benson implies) but often they are deliberately introduced in order to enhance those structures by extending the power of new technology into new media for translating traditional objectives and practices in ways which change the surface structure of organizations while at once preserving the efficiency of production processes and the predominance of certain positions in those processes.

Benson's presumption of conflict can be contrasted with a competing view of the social constructedness of organizational realities which sees in this process a positive development of organization structure in which the interests of organizational participants are presumed to be harmonious as long as they can be openly articulated. This is the view of Argyris and Schon, as representatives of the action school of organization theory.

The Action Approach: The social action approach to organization theory also begins with the premise of the social constructedness of organizations. According to Argyris and Schon, what we think of as "organizations" are in actuality artifacts of the ways in which individuals represent organization. For this reason, they argue, organizations are not static entities, but are dynamic and reflexive phenomena which emerge through processes of organizational learning, which they identify as an active, cognitive process of organizing. (Argyris and Schon, 1978, p. 16) They define these individuals; representations of organization as "theories" and distinguish between an individual's "espoused theory of action," as that view of organization in a given situation which is articulated to others; and the individual's "theory-in-use," which is that theory (or set of beliefs) which "actually governs his actions"-- a theory which may or may not be compatible with espoused theory, a fact of which the factor may be unaware. (Argyris and Schon, 1978, pp. 10-11)

Their major objective is an effort to improve the competence and effectiveness of "practitioners of management, consultation, and intervention," and thus the rationale guiding organizational research in this perspective is a focus on issues of interpersonal interaction in professional practice. In this focus, Argyris and Schon advocate a theory of action approach which assumes that all deliberate action shares a common set of characteristics:

1) Action is based on individual cognition, which involves norms, strategies, and assumptions or models of the world which are held by actors in organizations;

2) It focuses on human learning as the production of knowledge through construction, testing, and reconstruction of ideas;

3) It includes a focus on control, from the point of view of the agent for whom the explanation of deliberate human behavior also constitutes a theory of control; and from the point of view of the observer who, in studying organizational actors, takes on the role of explanation and prediction of the behavior of that

actor (again with the implication of control). (Argyris and Schon, 1978, pp. 10-11)

According to the action approach, an organization is essentially a political system--"...an identifiable vehicle for collective decision and action..."--in which individuals act within the collectivity on the basis of rules which determine membership and decision-making authority. It is these rules which constitute their "organization" as something more than mere association. The essential characteristic of organization, then, is that "...members' behavior be rule-governed in the crucial respects." These rules are the basis for the persistence of organizations (as entities) and for the identification of members' theories-in-use by observation of their behavior in organizational decis-on-making and action. Insofar as those decisions and actions are governed by rules carried out in the name of the organization, then we can say that those actions and decisions are organizational. When this condition is fulfilled, they argue, we may then attribute "agency" to the organization as a whole--as an agency or entity through which the actions of individuals are rendered into an instrument for collective action.

They define an "agency" as "the solution to a problem," which solution amounts to the continuous performance of some complex task. They way in which this total "complex task" is patterned into interconnected roles and activities reflects some strategy for the division of labor and the design of work, which constitutes a (corporate) theory of action (as distinct from those of its members). (Argyris and Schon, 1978, pp. 13-15) It is this corporate theory of action which the authors identify with organizational learning systems. The role of consultants is to articulate, and hopefully to improve on, this theory of action on behalf of their clients, identifying its underlying norms, strategies, assumptions, and patterns of control and communication.

Organizational learning is based upon the articulation of these underlying theories--or organizational maps--in order to identify incongruencies between participants' espoused theories and their theories-in-use and to facilitate prediction of the consequences of action over the long run.

> ...(O)rganizational learning (is) a process mediated by the collaborative inquiry of individual members. In their capacity of agents of organizational learning, individuals restructure the continually changing artifact called organizational theory-in-use. Their work as learning agents is unfinished until the results of their inquiry...are recorded in the media of organizational memory.... (Argyris and Schon, 1978, p. 20)

These organizational maps are being constructed continually through inquiry and testing on the part of organization members, who base their inquiry on their own private images and available public maps of the organizational theory-in-use. These public maps or representations reflect the "memory" of the organization, which consists of just these norms, strategies, and assumptions which define the design of work and the division of labor. (Argyris and Schon, 1978, pp. 16-17)

Organizational learning, then, involves the examination of problems in the light of organizational information constituted in these formal and informal maps, and these problems are reflected in (the perception of) a mismatch between actual and expected outcomes--

outcomes which do not seem to fit the presumed organizational theory of action at all. (Argyris and Schon, 1978, pp. 55-56) Organizational learning involves the resolution of these uncertainties--or conditions for error--through

> ...a process in which members of an organization detect error or anomaly and correct it by restructuring organizational theory of action, embedding the results of their inquiry in organizational maps and images. (Argyris and Schon, 1978, p. 58)

Argyris and Schon's fundamental argument is that organizations have a tendency to develop learning systems which inhibit the type of learning necessary to discover and transform sources of error in reasoning and planning, because to focus on such sources of error is to expose basic norms, objectives, and organizational policies. In modern industrial societies, this type of questioning violates conventional theories of action in which most people are acculturated. They note from their experience as intervenors or consultants in ongoing organizations, that these organizations typically are characterized by learning systems in which: 1) learning is limited to instrumental problem-solving (which they call "single-loop inquiry"); 2) norms are generally articulated and changed (if they are) through eruptions associated with adjustments to changing environmental conditions; and 3) organizations do not in general reflect upon their internal processes and criterion values, beyond the attempt to account for the results of "single-loop learning." (Argyris and Schon, 1978, p. 309)

Because the examination and articulation of the underlying assumptions and policies in an organization itself violates conventional organizational norms, organizational learning in this approach depends heavily on the role of consultants--or intervenors--whose task it is to help the client to focus on this underlying structure. (Argyris and Schon, 1978, pp. 4, 186-188, 207) They identify this process as one of combining advocacy with inquiry in confronting organizational learning systems through public testing and experimentation; this approach they refer to as "good dialectic," to be contrasted with the "bad dialectic" which emerges from the tendency for that information to be undiscussable which is necessary to discover and correct errors. In the "bad dialectic," organizational learning is inhibited as individuals perpetuate problems by acting unilaterally in attempting to solve them on the basis of norms such as self-protection and restriction of communication. In this unfortunate situation, the conditions for error are further reinforced by the tendency for those underlying theories of action held by participants to be inaccessible, obscure and inadequate to the task--all factors which cannot be discussed. Thus it is the role of consultants to help organizations to identify these inadequacies and inconsistencies in their learning systems, ideally without subjecting members to personal risks in the process. (Argyris and Schon, 1978, pp. 3, 42-47)

Argyris and Schon are concerned to explain how it is that these irrational tendencies can emerge out of the "...apparently rational process of organizational design..." and they argue that the problem lies in just the sort of competition (and implied conflict) which is the focus of Benson's dialectic theory of organization. According to Argyris and Schon, the prevailing tendency for organizational designers to think hierarchically is a response to the type of limitations known to characterize individual rationality (limitations having to do with the quantity and complexity of information which humans can comprehend, as discussed by Miller, Simon and others). The conventional strategy of organization design is, therefore, to factor complex tasks into simpler ones, a strategy which at once improves the ease of recognition and control while at the same time creating problems for organizational control and coordination, problems which have to do with limitations on the rationality of managers and supervisors. The conventional mini-max design strategy creates organizational structures which entail "inner contradictions" which stem from the fact that applying such strategies to human beings undercuts their sense of personal competence and effectiveness in the performance of their tasks. In particular, while individuals are educated to enhance and develop their abilities, the design of organizational tasks--especially at lower levels--makes it unlikely that they will be called upon to use these abilities. The outcome is an increase in organizational "gamesmanship" and mutual mistrust among organization members, which is countered by increasing emphasis on "...the use of rationality, direction, control, rewards, and penalties...", intensifying into an atmosphere of "management by crisis." (Argyris and Schon, 1978, pp. 121-122, 125)

Benson and Argyris both posit a fundamental dichotomy between rational and irrational forces--or contradictions--in ongoing organizations, albeit on the basis of opposing perspectives. Both approaches presume a hierarchical form of organization already in place, a form which derives from the limitations on human rationality and the micro-division of labor in industrial organization. This presumption of form then leads both to a presumption that the conflicts or contradictions which they have identified are somehow inevitable. Argyris and Schon place the origin of this conflict or competition in the narrowness of the design of work design which does not allow to lower participants the same sense of agency which it attributes to "the organization" itself, a point with which we are in agreement, and which does not contradict the conflict perspective when the latter seeks to examine the empirical nature of these contradictions. Watson and Benson would argue that Argyris and Schon espouse a view of organization which is itself a reification; however, the latter acknowledge that it is individual actors who think and act and not organizations. However, while it is the case that Benson and others in the conflict school presume a conventional hierarchy with built-in contradictions and conflicts, Argyris and Schon's theory of organization tends not just to presume such order, but to impose it as well.

There are several basic problems in the action approach. First, the theory of organization in action theory is itself obscure. In Argyris and Schon's view, organizational theory-in-use is to be inferred from organizational behavior, which is constituted of the actions of a number of individuals in a collectivity, each referring to underlying theories of action which are either espoused or presumed-in-use. How can organizational theory-in-use possibly be defined separate from the multiplicity of individual theories-in-use held by these actors--even if they could articulate them? Argyris and Schon resolve the ambiguities between organizational theory-in-use (which they agree cannot be identified with any notion of an "organizational mind" that literally remembers, thinks, and learns, which would be a reification). Rather, they recognize these individual theories-in-use by identifying individuals' actions and decisions as organizational when they are governed by collective rules making up the organizational theory-in-use. This is merely a theoretical hedge which amounts to identifying organizational theory-in-use with the standard notion of the formal organization as embodied in its rules and standard operating procedures in already established institutions.

A second problem in the action approach derives from its explicit advocacy of one point of view, and its sponsorship by managerial clients. Writers in the British school of contingency theory, such as Toby Watson, share with Benson an assumption of the fundamental constructedness and inherent conflicts of interest in organizations, as well as a special disdain for the work of the (American) social action theorists (who also refer to themselves under the rubric of contingency theory). Watson refers directly to Argyris, McGregor, Likert, Blake, and Herzberg as "behavioral science entrepreneurs," and argues that they can be distinguished in virtue of the fact that "...their work is designed to sell, whether in the form of books, management seminars, training films or consultancies." Watson argues that the work they do for their clients is primarily concerned with "hygienic" factors in organizations, and that its internal logic is "...reductionist, partial, evangelistic and sociologically highly inadequate on the explanatory level..." because it fails to sufficiently recognize structural, cultural, and economic factors in organization--a point on which we would also agree.

A final irony in this school of thought, oriented as it is to action within an industrial setting--is the advocacy of participative styles of organizational management delivered in an authoritarian manner--as a product bought by management and delivered to subordinates through the hierarchy. This is a highly marketable approach, and is what managers have come to expect from social science consultants in industry. In fact, we would add, this is the dominant approach in current "implementation theories" developed and disseminated by consultants working as intervenors in organizations undergoing change via the introduction and upgrading of computer-based technologies. In practice, Watson argues, both participative management and management science approaches can "work" (although ostensibly they are direct opposites) largely because each of them has the quality of a self-fulfilling prophecy, in which "...their very advocacy can bring about that they first claimed or pretended to be inevitable." This tendency is equally reflected in a Marxian approach and in the old human relations approaches, he argues, and we would agree, adding that the human relations approach (which is close to that of the social action theorists) is based upon this very reflexivity which is what is named in the "Hawthorne effect." (Watson, 1980, pp. 38-40) We could go Watson one step further by noticing that he does not consider the consequences for organizations attempting to implement both the management science and the participative management approach simultaneously, which is common in implementation studies for office automation.

We have argued that it is not the directedness, but the bias and advocacy in such research approaches which weakens the validity of their inquiry, and would add to those limitations the narrowness of the conceptualization of the research problem itself, as reflected in such self-fulfilling prophecies. Both the conflict and the entrepreneurial approaches suffer from a fundamental partial-ity, which is associated with the restriction of the investigation by contextual factors associated with the sponsorship of the research and with the advocacy of a given point of view of outcome on the part of researchers working on behalf of some client and within some perspective. Argyris and Schon's discussion of these "taboos" of intra-organizational communication suggest why it is necessary for organizational learning to be initiated and controlled by external "intervenors." The role of the consultant is created and becomes necessary because of the very norms which preclude a focus on errors, or talking about each other, or expressing negative opinions or conflicts in values, or even the public articulation of goals and objectives per se.

All of this discussion is necessary in order to undertake changes in organizational processes, and this is difficult enough when the changes are associated with problems such as those of identifying and developing new markets and technologies, with the objective of creating new product lines, or even new production processes. The undertaking is especially difficult, however, when the object of change and technological development is the very system of communication and organizational learning itself. It is this system which constitutes the structure of the ongoing organization at its most fundamental level, and it is this system which must be called upon continually in order to guide the process of inquiry associated with implementing that "inquiring technology" in the first place. The implementation of computers in offices is thus a reflexive problem in which uncertainty becomes compounded by the requirements and constraints on communication, and in this process touches upon nearly every undiscussable norm, and conceivably implicates every task in the organization. It is not enough to say that this system is undiscussable under these circumstances, and in the work of Argyris and Schon (and others of the social action perspective) we have a theoretical explanation which is both empirically vivid and consistent with a broad humanistic tradition which emphasizes learning, both at the organizational and individual levels. However, the problems of advocacy and validity are not resolved by noticing the practical importance of the approach.

Both the action approach and the conflict approach lack a rigorous methodological strategy for analyzing and interpreting the verbal and anecdotal data produced in consultation and criticism. Action theorists give little evidence of an appreciation of the role of intervenors as change agents while carrying out their inquiry, especially as that activity affects the validity of the research. Instead, that style of interventionist research which articulates ongoing organizational structures while translating them into some preferred or "improved" model itself constitutes a coercive imposition of a presumptive strategy of some sort. Argyris and Schon prescribe a model of organizational learning which falls within the Durkheimian tradition reflected in modern organization theory by Burns (1963) organic and mechanistic styles of management, and other "good" and "bad" organizational dichotomies, such as McGregor's (1960) Theory X and Theory Y and Likert's (1961) System I and System IV. These organization theories are explicitly prescribed as models for information acquisition and organizational change. Thus even (or especially) in their own terms, the implementation of these models in organizational contexts _is_ a process of organizational change--a change which takes place in the configuration of ideas making up theories-in-use by a wide range of participants and observers, and translated into concrete form through planning and designing of mechanisms and controls, and through social action--including those of decision and communication.

Because there is no rigorous method for analyzing the data gathered in this approach, the collection and dissemination of information blend together into a process of persuasion which reifies the data acquired, first by imposing a pre-determined order in its description and interpretation, and second, in the intervention in the phenomena represented in that data through recommendations and processes of change initiated by the consultant. Order is, therefore, imposed on the system through the process of investigation, in a way which replaces the rational articulation and testing of theories-in-use by organizational members by the same sort of articulation on the part of the consultant-as-expert. This process has a tendency to further polarize decision making and action within the organization, and at the same time to contaminate the data acquired in the process of research and consultation (not least by the characteristic presumption of underlying contradictions and conflicts).

Interestingly, in the presumption of hierarchical order and the inevitability of conflict or contradictions in that order, both Benson and Argyris share the "process" view of the constructedness of organizations, and each gives a plausible--and compatible--account for the process of organizing, or organizational learning. However, both are blind to the issue of context in that learning process, including that represented by the intervention of researchers, whether they inquire on behalf of unions or management. There is little recognition of the variation in form of organization in either approach, nor of the process of organization which they tend to describe not as a process of change or adaptation to changing conditions, but as one of adjustment of errors. The outcomes of this type of organizing are judged in each approach on the basis of value criteria which are particular to a given sub-stratum of the industrial order, and each sees in the other the intrusiveness of research in ongoing organizations in-order-to transform them in some way. However, neither approach entertains the contextual relationship in its own approach to investigation. Thus, from the consideration of the predictiveness of any research approach, there is no sound methodological connection between the formalisms of the kinds of conventional methods of planning, design, control and communication, and the kinds of constituent theories-in-use by actors, observers, and intervenors (each with their own definition of organization) as these theories are characteristic of some particular organizational context in which the research is taking place. We still cannot account for the process by which human knowledge and experience are objectified in the development of technologies and institutions in either the "debunking" stance of the conflict theorists who reject these constructs out of hand as manifestations of false consciousness and error, or the entrepreneurial stance which creates these realities as it studies them. And, we have argued, we cannot infer this experience or knowledge from a mere surface description of the observable features of some machine or institution, characteristic of inductive empiricism as a methodological strategy.

Thus Benson's and Watson's criticisms of organization theory are well-taken, at least in historical terms. The prevailing sociological tradition (and especially the forms popularized in management) had tended to base its arguments on a presumptive universality of social systems, argued on the basis of analogy with biological systems, defined in terms of a systems vocabulary which cannot be defined simply in observational terms, and which is associated with a presumptive teleology of development. This is the paradigm which Watson associates with the systems approach in sociology, which, he argues, is identical with the human relations school of industrial sociology, and characterized by a common inheritance of the

> ...old organic analogy in social thought: the metaphor which views society or the enterprise as some kind of organism or animal which constantly seeks equilibrium or stability and is always fighting off pathological and disintegrative influences. (Watson, 1980, p. 41)

The importance of this organic paradigm is indeed consistent with a long tradition of Social Darwinism and functionalism in American sociology, a tradition which was only recently broken down with the introduction of European positivism, which, Waddington argues, was brought into U.S. education in the 1950s and 1960s with the immigration of European scholars to the U.S. after World War II. (Waddington, 1977, p. 23) The influence of this newer systems analytic perspective was also reinforced by the transference of systems engineering technology from World War II operations research into methods of research and development in American business and education during this same period. Therefore, as we argued in Chapter I, this simple general systems view of organization, as it is criticized by Benson, Watson, and others, is by no means the only available systems approach, and indeed it is considerably weaker than cybernetics and the methodology of systems engineering as they have developed in the period since World War II. Moreover, Social Darwinism and functionalism are not the only available theoretical traditions, even in American sociology. Thus we must dismiss these "straw man" criticisms of systems thinking which recognize only a primitive form of general systems theory (as presumptively identical with the tradition of functionalism), and explore the broader sociological tradition--reflected in

Mannheim's sociology of knowledge and in American pragmatism--as a basis for explaining the constructedness and directedness of organizational development and change.

Mannheim's Sociology of Knowledge: Karl Mannheim outlined an alternative perspective on the use of scientific objectivity, a perspective which has been significantly reinforced by the development of systems engineering methodology, and one which accepts the problem of subjectivity as inherent in the study of social organization. His sociology of knowledge incorporates two components, in the manner which Sims and other practice-oriented researchers advocate, and in a tradition of accompanying substantive theorizing with methodological explanations which was more common in the era of classical sociology. This approach combines: 1) A theory of (social) organization which accounts for the emergence of forms of thought and 2) a method for investigating those forms of thought as they are instantiated in different contexts.

For Mannheim, the sociology of knowledge is focused not on the thinking of individuals as such in order to reason forward to the nature of "thought as such" (in the manner of the inductive empiricist approach to artificial intelligence, characteristic of the Simon, Newell school), nor does it dissociate the "concretely existing modes of thought from the context of collective action" (as does nearly all formal theory). Rather, the objective is to explain these modes of thought in the context of concrete historical-social settings. (Mannheim, 1936, p. 3) Against a tendency for both epistemological and psychological approaches to thought to separate individual mind from the group or context, Mannheim argues for a sociological approach which views individual thought as understandable from a perspective which takes into account the relationship between thought and the matrix (or context) in which individual experience takes place, a matrix which is inherently social.

> ...(K)nowledge is from the very beginning a co-operative process of group life, in which everyone unfolds his knowledge within the framework of a common fate, a common activity, and the overcoming of common difficulties. (Mannheim, 1936, pp. 28-29)

Because these forms of thought--which he identifies with understanding--arise out of some particular context, and in response to the "will to change or to maintain" (which takes place within a particular context), it follows that people will view the world differently, depending upon that particular context. (Mannheim, 1936, p. 4) It does not follow that this knowledge can be judged as incorrect or false, on these grounds, which, he argues, is the claim of relativism.

Mannheim distinguishes between relativism and relationism--as the method of sociology of knowledge--on the basis of a distinction between two separate meanings for the term "ideology." It is the particularist conception of ideology, he argues, which is identified with the skepticism which is accorded to the ideas of one's opponent, ideas which are considered to be "more or less conscious disguises of the real nature of a situation"--hence, false knowledge which serves to cover one's true interests from recognition by others. The particularist version of ideology focuses uoon a "psychology of interests" as the ground of deception, with the individual as the point of reference. Thus in the particularist version, the embeddednews of knowledge in its social context "refers to a sphere of errors" and a sphere of "lies" as the poles signifying the distortion of knowledge associated with the contamination of thought by the interests and conduct of the thinker in the concrete here-and-now.

The total conception of ideology shares with the particularistic version the assumption that ideas reflect the position in the social milieu of the person who espouses them. However, the total conception of ideology views not only the specific and concrete ideas held by individuals as conditioned by their social context, but Mannheim also includes in that view the broader conceptual apparatus--or Weltanschauung--as corresponding to and growing out of collective life, such that we are not referring merely to isolated cases of "false knowledge," but rather to "fundamentally divergent thought-systems," indicative of modes of "experience and interpretation" which differ widely.

The total conception of ideology is much more akin to Weber's "sociological verstehen," as representing a reconstruction of the essential aspects--or constituent elements--in a given worldview (or Weltanschauung). Mannheim argues that in this total conception an attempt is made to "reconstruct the whole outlook of a social group" as underlying the particular judgments of any individual, none of whom can be expected to be the bearer of an ideology or thought system taken as a whole. The total conception seeks not to explicate the particular attitudes and judgments of individuals, but rather to understand the theoretical implications of a mode of thought, and in so doing not merely to elucidate the content but also the form of thinking represented by the conceptual framework of a thinker as a function of his life-situation. (Mannheim, 1936, pp. 53-59)

Mannheim's distinction between the particular and the total conceptualization of ideology underlies the differences which he describes between relativism and relationism as the method of the sociology of knowledge. In relativism we see the outgrowth of methodological problems in modern historical-sociological inquiry, problems which reflect this awareness of the relationships between all historical thinking and the concrete position of the thinker. The idea of relativism is thus bound up with the problems of objectivity, and Mannheim argues that we should attempt to emancipate ourselves from this type of relativism by recognizing that it is not possible to entertain the notion of absolute truth which can be identified as somehow independent of the social context and of the values and positions of the thinking subjects. Thus objectivity is an unrealistic and unattainable goal, and the type of relativism which merely asserts the embeddedness of observation in the position of the observer is not as helpful as would be an account of the relational character of all historical knowledge--which begins with the premise that absolute truth is not meaningful,

> ...for what is intelligible in history can be formulated only with reference to problems and conceptual constructions which themselves arise in the flux of historical experience. (Mannheim, 1936, p. 79)

In keeping with this general conception of the sociology of knowledge as the position which holds 1) that all thought, and indeed all forms of thinking are embedded in an ongoing social context, characteristic of the total conception of ideology, and 2) that such thought demonstrates a relation with its context, Mannheim argues against viewing the knowledge of others as "false," as comprising lies and error, and against the type of relativism which judges the correctness of ideas, in favor of a recognition that

> ...the ideological element in human thought...is always bound up with the existing life-situation of the thinker. (Mannheim, 1936, p. 80)

The meanings which arise out of a historical sequence and a social context are continuously developing structures which "are in no sense absolute." This insight into history, insofar as it is non-evaluative and non-judgmental, may be characterized as relationist as opposed to relativist, reflectionism indicating only

> ...that all of the elements of meaning in a given situation have reference to one another and derive their significance from this reciprocal interrelationship in a given frame of thought. (Mannheim, 1936, p. 86)

The fact that every point of view is specific to some given situation seems to pose special problems for scientific knowledge, especially scientific understanding of social life itself, which is characterized not by concrete objects and entities, but by fluctuating tendencies and strivings. Moreover, in the flow of social life, the composition of these interacting forces or tendencies is continually changing, new forces constantly entering into the situation, "and forming unseen combinations." Most problematic for the social scientist as observer is the realization that the observer does not stand apart but is rather a participant in the flow of such conflicting forces, and in this participation he is equally bound to a particular view given his position and interests.

This realization contradicts the claim made by Mulkay (1979) and others that Mannheim exempted scientific reasoning from the scrutiny of the sociology of knowledge on grounds of objectivity--an exemption which would preclude any real investigation of the substance of relationism proper in the study of ongoing organizations, and which would reduce the study of thought to polemics, in which all ideas are ideologies. It is clear that Mannheim is at great pains to distinguish between mere "unmasking" of ideologies and a sociology of knowledge which seeks to account for forms of thought in the context of life-experience, and it appears that these "neo-sociologists of knowledge" have misread Mannheim's many references to scientific knowledge.

In his dicussion of the possibility for a scientific study and guidance of political life, Mannheim argues that while truth and falsity are established in the social sciences--as in any science--by examination of the object of inquiry, the examination of that object (in this case a social or political institution) necessarily occurs "in a context which is coloured by values and collective-unconscious, volitional impulses." This context is the basis for the general questions to which research is initially directed, and it provides as well the "concrete hypotheses for research and the thought-models for the ordering of experience." Thus

> ...(T)he particular manner in which the problem presents itself to him, his most general mode of thought including even his categories, are bound up with general political and social undercurrents. (Mannheim, 1936, p. 117)

It is just this problem, according to Mannheim, which poses the greatest difficulties for a science of politics. However, far from excluding scientific knowledge from the rigors of observation, he clearly indicates that scientific knowledge, like any other, is embedded in a context of meaning, logic and interest separable from the activities of scientific observers. Furthermore, he does not exempt the sociology of knowledge itself from such scrutiny, arguing instead that the general form of the total conception of ideology as advanced by the sociologist of knowledge is characterized by "the courage to subject not just the adversary's point of view, including his own, to the ideological analysis." (Mannheim, 1936, p. 77) He does indeed apply his version of scientific analysis "to the facts which are current in scientific as well as popular discussion," and in so doing does--in his own terms--define a form of objectivity in social science which is based, not upon excluding or invidiously contrasting the base instances of empirical phenomena, nor yet the still baser instances of deliberately engineered realities, motivated by mundane and selfish interests, but which is based instead on bringing such forms of thought into awareness. (Mannheim, 1936, p. 5) It is this awareness--as the opposite of Berger and Pullberg's "forgetting"--which can provide the control or reliability in our research on organizations.

Not only does Mannheim not abandon the ideal of a science of politics, but he questions the existing framework of science itself on these grounds, advocating a revision of our conceptions of science, particularly in those cases in which we have been unable to field adequate theories "where the science in question is closely concerned with practical problems." It is the difficulty of applying scientific methods to the understanding of these concrete problems which is central to the narrowness of the dominant scientific methodology. Specific and relevant knowledge must increasingly be obtained and communicated in the course of actual conduct, but the exclusion of the consideration of such conduct from the purview of science renders scientific knowledge narrower in scope than popular knowledge, an objection which is not answered merely by referring to such concrete knowledge as "pre-scientific" or "intuitive." The problem arises, he argues, from the fact that "only certain sciences, for historical reasons, have become models of what a science should be," based largely on the dominant role of mathematics in our modern scientific ontology, which holds that only that which is measurable "should be regarded as scientific."

This ontology, which he identifies with "modern positivism," an ontology which seeks to discover general knowledge by the processes of measurement, formalization, and systematization of a set of axioms, was relatively successful when applied to those aspects of reality which could easily be quantified or generalized, and in which the subject matter is relatively homogeneous. However, such phenomena "by no means exhausted the fullness of reality," and in particular they exclude the phenomena of the cultural sciences, which are characterized by unique and concrete structures not amenable to explanation through general laws of positivistic science nor the model of modern mathematical natural science. (Mannheim, 1936, p. 166)

Mannheim notes that this dominant positivist view of science is itself "rooted in a definite Weltanschauung" and has developed in conjunction with identifiable political interests, specifically those of the rising bourgeoise intelligensia, which for reasons of its own, sought to sever the "organic connection" between man as a member of society and historical subject, and the thought or manner of thinking which characterizes him as a member of that society at that place in time. For Mannheim, this separation "constitutes the chief source of error" which arises between the notion that formal knowledge is that which is equally accessible to all and unaffected by the interests of the observer, and the recognition that wide areas of knowledge are accessible only to certain subjects and in certain periods, and that this knowledge, indeed is obtained in the course of individuals carrying out their social purposes, which are permeated with self-interest. Thus he argues,

> Self-awareness and awareness of others are inseparably intertwined with activity and interest and with the processes of social interaction. Whenever the product is isolated from the process and from the participation in the act, the most essential facts are distorted. This, however, is the fundamental feature of the kind of thinking which is oriented towards a dead nature, in that it wishes at all costs to cancel out the subjective, volitional and processual relations from active knowledge in order to arrive at pure, homogeneously coordinated results. (Mannheim, 1936, p. 169)

The sociology of knowledge opposes this "dead" objectivity with the recognition that the evaluative and active elements of knowledge cannot be separated from formal knowledge, and that, furthermore, at any point in history there is a wealth of knowledge which is only accessible from certain social perspectives. The problem which

Mannheim presents for inquiry is not to identify knowledge as "truth in itself," but rather to identify the way in which man copes with his problems of understanding from within the context of his (limited) experiences. A theory of the "social or existential determination of actual thinking" does not arise out of pure logical possibilities or from immanent laws embedded in the nature of things. Rather thinking emerges out of a range of extra-theoretical or existential factors, which means that thought will not necessarily be the same for all people. Whereas the particularist conception of ideology refers to specific assertions as falsifications or errors without inquiring into the "total mental structure" underlying such assertions, the sociology of knowledge focuses precisely on this total mental structure as it is manifest in different groups and "currents of thought." In so doing, methods of relationism in historical-sociological analysis are directed to the question of

> ...when and where social structures come to express themselves in the structure of assertions, and in what sense the former concretely determine the latter. (Mannheim, 1936, pp. 266-67.

Mannheim's sociology of knowledge gives a clear direction to the problem of accounting for the relation between theory and practice. Ideas do not arise and change, he argues, either at the level of ideas alone (which was the argument of the older intellectual historians), nor are they the product of the inspiration of isolated geniuses or heroes (a thread which is still strong in the history of management thinking). Rather, the existential determination of thought involves three premises:

1) The formulation of problems arises out of actual human experience in the past which has involved similar problems;
2) Understanding the multiplicity of data which is available involves a selection, and therefore an "act of will" on the part of the knower; and
3) The direction of problem-solving is significantly affected by factors which arise out of living experience, factors which are not merely individual, but which are associated with the collective purposes of the group of which individuals are participants. (Mannheim, 1936, p. 268)

As does Sombart, Mannheim refers to "styles of thought," and suggests that the methodology of the sociology of knowledge is capable of approaching the existential determination and transformation of forms of thought as the historian of art approaches the various styles in this realm, which can be "dated" and thus associated with various historical conditions characteristic of different places and times. By extension, forms or styles of thought can be associated with social movements, involving the interactions of people as bearers of ideas. Thus, we can identify certain modes of thinking with different historical settings--as in the case in the emergence of a full-blown computer technology in the context of modern systems engineering methodology as such a style of thought, in which context computerization developed as something more than the construction of automata as simple extensions of their constructors. (Mannheim, 1936, pp. 271-272)

The social action approach and the conflict approach both share a view of organizations as socially constructed, and of organization as a process of socially defining reality, such as is associated with social movements. There is an unstated prescriptive or utopian directedness or bias to these perspectives, however, which renders them unreliable as a methodology for organizational analysis and offensive as a philosophy for organizational design and change. The presumption of some ideal order, of whatever form, pits the constructedness of social organizations against the process of organization itself, casting that process in the mold of conflict and conquest--more reflective of our western heritage, perhaps, than of any logical constraint in the engineering of systems. Even Kuhn's recognition of the transformation of scientific paradigms posits a process by which scientific knowledge advances through vanquishing prior--and competing--forms of knowledge.

More useful as a basis for understanding organizational development and change--organizational learning--are those theories of organization which focus on learning as the vehicle by which living systems adapt to their environments. We must look to the early American tradition of pragmatism to find such theories of organizational learning, capable of accounting for the process of organization and for organizational forms as outcomes of that process, including as outcomes, the process of establishing and changing forms itself. This is what Whitehead was referring to as the "process of invention," which, it has been argued, is itself an invention of the modern--or industrial era--itself. The process of invention hardly needs to be invented but rather is the fundamental basis of adaptation in the human species; what these writers are referring to is the formalization and codification of that process itself, in modern science and logic.

Pragmatism, Experience, and Organizational Learning: The focus on experience in Mannheim's sociology of knowledge is consistent with a similar emphasis on experience and learning in American pragmatism. As a proponent of this school of thought, George Herbert Mead rejected the dominant mechanistic view of the universe in favor of an attempt to demonstrate how certain forms arise--a question for which mechanical science provided no explanation. In contrast with the older Aristotelian and Kantian tradition which conceived of forms as given in the mind prior to experience, Mead viewed the forms of mind--or thought--as arising out of, and varying with respect to experience. In the spirit of Darwin's The Origin of the Species, he argued for a distinction between life-processes and the forms which those processes may exhibit over time, explaining the form or character of particular objects as arising out of the conditions of the life-process in which they are embedded. It is the process, according to Mead, which is unitary and which has continuity; forms--or things--are ephemeral and transitory, depending upon this process as a "fundamental fact" appearing over time in different forms. (Strauss, 1962, pp. 5-9)

In the human species, this evolutionary life-process proceeds to a point at which the human animal can achieve control over the environment, not as individuals, Mead argues, but as a collectivity--

human society. And no aspect of human society points so directly to the evolutionary and adaptive nature of human society as the evolution of the scientific method.

> ...(T)here is nothing so social as science, nothing so universal. Science is inevitably a universal discipline which takes in all who think. It speaks with the voice of all rational beings. It must be true everywhere; otherwise it is not scientific. But science is evolutionary. Here, too, there is a continuous process which is taking on successively different forms. (Strauss, 1962, p. 16)

Science for Mead is a method of generating knowledge, a method which is embedded in the world or environment of immediate experience, and which is based upon the search for solutions to problems which arise in the field of conduct. Problems, which are necessarily experienced by individuals, take on common importance through becoming objects of reflection. Such objects of reflection--or expressions--achieve the status of universal forms by being formulated in terms which would be universal to any experience--exhibited in the use of universal or common terms and in the search for universal solutions. (Strauss, 1962, pp. 55-57)

Because knowledge--and scientific knowledge in particular--is based upon this reflection and universalization of experience, scientific inquiry begins (and not ends) with data and produces as outcomes not "facts" but theories or working hypotheses which "enable us to carry on where a problem has held us up." The object of knowledge, then, is the understanding of some "newly discovered present," and not a rehearsal of past objects. This present can only be known through interpretation in the context of that which is past, and "...the only reason for research into the past is the present problem of understanding a problematic world...." (Strauss, 1962, pp. 61-65)

In Mead's approach to pragmatism, scientific inquiry is based upon the search for knowledge through understanding of present problems. Mead defines science broadly as an activity undertaken in order to continue an action interrupted by the experience of problems. The correctness of that knowledge or thought is judged in terms of the uses to which that knowledge may be applied. In this process "the past is a working hypothesis" which is valid for the present only insofar as it "works" as a guide to conduct.

Mead expressed eloquently the disdain which many held for pragmatism and for its emphasis on solving the mundane problems of everyday conduct:

> Pragmatism is regarded as a psuedo-philosophic formulation of the most obnoxious American trait, the worship of success; as the endowment of the four-flusher with a faked philosophic passport; the contemptuous swagger of a glib and restless upstart in the company of the mighty but reverent spirits worshiping at the shrine of subsistent entities and timeless truth; a blackleg pacemaker introduced into the leisurely workshop of the spirit to speed up the processes of thinking...a Ford efficiency engineer bent on the mass production of philosophical tin lizzies. (Strauss, 1962, p. 66)

In spite of this traditional denigration of practical inquiry, Mead argues that that aspect most critically involved in the advancement of science in the modern era is its becoming conscious of its experimental method as a means for controlling the environment. It is this consciousness which enables science--and hence society--to change as is necessary to respond to exigency, and at the same time preserve the order or structure in society. Conscious reflection on the problems and changes in society and in knowledge is what makes the scientific method so effective and which enables it to operate in those situations in which the goals cannot be known in advance, by focusing directly on problems or constraints in the environment and developing methods for their solution. Progress, therefore, comes through the solution of problems, and the scientific method, in this view, is "...only the evolutionary process grown self-conscious." (Strauss, 1962, pp. 17-21)

For John Dewey, pragmatism--which he defines as the application of methods of scientific or experimental inquiry to practical concerns--is fundamental to human adaptation and survival. According to Dewey, man seeks for security by two principal methods: 1) propitiation of the powers which determine his destiny, and 2) by the invention of arts by means of which man "...constructs a fortress out of the very conditions and forces which threaten him." (Dewey, 1929, p. 3)

Dewey notes, as does Mead, that understanding of the construction of knowledge by means of acting upon the world of experience through the mechanical arts and methods of experiment has been impeded for centuries by the dichotomy, traditional in western philosophy since Greek civilization, that the action of doing and making is associated with work, work which is "...onerous, toilsome, associated with a primeval curse...done under compulsion." This mundane work is contrasted with intellectual activity which involves leisurely contemplation of that which is secure and changeless. Beyond the onerous quality of the activity of work in doing and making, it is the uncertainty associated with acting upon a world of changing experience which gives to the mechanical and practical arts their traditionally inferior status in respect to Being, which in its immutable certainty is separated from the world of action and appearance.

> Thus the arts by which man attains such practical security as is possible of achievement are looked down upon. The security they provide is relative, ever incomplete, at the risk of untoward circumstance. The multiplication of arts may even be bemoaned as a source of new dangers. Each of them demands its own measures of protection. Each one in its operation brings with it new and unexpected consequences having perils for which we are not prepared. The quest for certainty is a quest for a peace which is assured, an object which is unqualified by risk and the shadow of fear which action casts. For it is not uncertainty per se which men dislike, but the fact that uncertainty involves us in peril of evils...Quest for certainty can be fulfilled in pure knowing alone. (Dewey, 1929, p. 8)

Practical activity, while it is an activity directed to the search for security, cannot provide complete assurance, Dewey argues, for activity itself brings about change, change which is, moreover, associated with situations which--being individualized and uniquely associated with the action in place and time--are never exactly duplicated. Thus practical activity is associated with the changing and uncertain, while traditional philosophical knowledge has concerned itself with the changeless and certain. Practical activity stimulated by the perception of uncertainty and insecurity leads both to an increase in knowledge about the world, but also to a situation of further uncertainty associated with the act of knowing through that activity. Thus Dewey notes, the depreciation of practice is based upon the location of practical action in the realm of "generation and decay," based upon that which changes, which varies, thus giving to the realm of the practical an uneliminable element of contingency--associated with material, physical objects which become and which pass away, and which are therefore defined by change in contrast with the knowledge of unchanging, rational, necessary forms. (Dewey, 1929, pp. 19-20)

The classic distinction between the subjective and objective, Dewey argues, then amounts to a contrast between the definite and the doubtful in which doubt is attributed to persons, and certainty is attributed to objects as complete and fixed, independent of their observation. Experimental procedure stands this dichotomy on end; through the operations of science, situations experienced by persons are transformed from problematic or doubtful to settled and coherent. Thus

> The relegation of the problematic to the "subjective" is a product of the habit of isolating man and experience from nature. (Dewey, 1929, pp. 232-233)

The consequence of this tradition, dominant in our thinking yet today, is that a "spectator theory of knowledge is the inevitable outcome," knowledge which is based upon that which is valued as antecedent to and unaffected by the act of observation, a form of idealism which requires that inquiry must "exclude any element of practical activity that enters into the contruction of the object known." (Dewey, 1929, p. 22)

By extension, the belief that science discovers or discloses inherent properties of "existence at large," or that which is ultimately real, is thus a holdover from an older metaphysical tradition. The persistence of this traditional philosophy (still quite alive in 1983) Dewey attributed to 1) the traditional appeals and associations embodied in dominant institutions, notably the church; 2) to the "persistence of social conditions" underlying the dichotomy between practical and theoretical knowledge, i.e., the conditions separating the servile laborer from the liberal, free, and socially esteemed activities of the man of learning; and 3) to the neglect of the learned to establish any coherent intellectual expression of the conditions and forces which characterize "in actual fact" the dominant character of the modern world. (Dewey, 1929, p. 77)

The experimental model, in contrast, is a manner of knowing by operating on the objects of ordinary experience in such a way that our knowledge is based upon their mutual interactions, rather than on the notion of qualities or properties immediately present to the senses. This transformation in approach greatly increases man's control over the objects of his experiences through the ability to change those objects and direct those changes through "knowing...as a mode of practical action." This is the manner in which "natural interactions become subject to direction." It is only in the experimental organization of our activity of knowing in this way that the progress of science, and the accumulation of knowledge of nature, has become in any way secure--and that security is based on the means by which we know, rather than the knowledge of things which is somehow independent of human perception and action.

In Dewey's pragmatism, "...conceptions are definitions of consequences of operations..." and the operations performed upon experience are decided in terms of the nature of the problem to which inquiry is directed. The first step in experimental analysis is thus identification of such a problem. By reducing objects as directly experienced to data, one transforms observations into objects of experimental manipulation, through which the operation of scientific method discloses relationships which are of practical importance as "instrumentalities of control." Scientific reasoning is not, therefore, a method of "discovery," but is instead an "effective way to think things," a way of framing our ideas about the world and of formulating meaning, which Dewey argues has proved itself as a better instrument of conceptualization than other means of understanding simply by "working better." (Dewey, 1929, pp. 141, 123-128, 134)

Uncertainty, Dewey argues, is a practical issue, directly connected with present experiences, inherently objectionable because of the potential for danger. Intelligence signifies the transformation of direct apprehension of experience to a type of indirect action which constitutes a tentative exploration of conditions in preparation for action. Problems are recognized in those aspects of situations which are dubious or risky, and thus intelligent action translates that quality or character of risk or uncertainty into the object of inquiry itself, as the basis for formulating a course of appropriate action. Only living beings respond to things as "problematic," which implies an orientation to a description of events which goes beyond what is immediately present in experience. While inanimate objects only react to what conditions are directly given, living systems effect a continuity in their acts over time, in which "preceding ones prepare the conditions under which later ones occur." The chain of "causes and effects" reflects a cumulative continuity necessary to the preservation of life; where that chain is broken, "death ensues."

When human beings respond to doubt, or uncertainty, as doubt, their acts achieve a mental quality, which when directed to a resolution of that uncertainty through inquiry and action, take on intellectual quality as well; thus the conceptualization of uncertainty and doubt unified in mental activity the emotional, volitional and intellectual aspects of experience, providing both the motivating direction and the intellective mechanisms for effecting control over those experiences. (Dewey, 1929, pp. 224-225) This intellectual and volitional aspect of experience is the basis for human adaptation, which takes place largely through processes of social organization.

Adaptation in Living Systems

Two common themes running through the writings of the "process" school of organization theory are 1) a criticism of conventional Newtonian physics as the model for scientific inquiry, and especially the emphasis on quantification and single-valued linear causality which dominates contemporary scientific reasoning; and 2) the importance of the process of organization per se and the problem of the opposition of organizational forms to that process. There are reasons for thinking that ongoing socio-technical systems cannot be described adequately from within a quantificational notation system; and, moreover, that complex and dynamic processes of adaptation via social organization cannot be captured fully by mechanistic and instrumental theories. We have argued that not only are there ideological objections to such organization theories, but logical objections as well to theories of organization which focus exclusively on negative feedbacks--the maintenance of form--but which do not account for (and thus cannot order) the generation or transformation of forms. What is needed, according to Rene Thom, is a theory of morphogenesis which includes two elements: 1) A methodology of scientific inquiry which can account for the complexity and ambiguity involved in the determination, the generation, and the persistence of form, which we recognize as organization; and 2) A recognition of the fundamental process of human rationality by which people seek to understand and to act upon the situations in which they find themselves. (Thom, 1975, p. 323)

The "mechanism" of causality in living systems can be described as an "interdependence" relation--referring to an interconnectedness or contingency among living organisms and populations, making up the environment of any individual system. Maruyama argues, in keeping with Thom, Mead, Sims and others, that conventional mechanistic and quantificational models and method of analysis are unable to explain this interdependency. He proposes instead to explain adaptation in terms of the concept of "mutual causality," by which he means a reciprocal, two-way relationship between living systems and (other systems in) their environments.

The key to recognizing mutual causal relationships is the presence of a loop, through which the influence of one element of a relationship is returned to it through the other elements--thus exhibiting the reflexivity of the basic feedback relationship. In such a loop there is no hierarchically causal priority among any of the elements; rather each influences all the others, either directly or indirectly, and in so doing each influences itself through the actions of all the other elements. This feedback relationship with the environment is the basis for self-regulation in living systems, which is represented in the cybernetic theory of communication and control. This cybernetic approach can be viewed in two ways, however, just as we argued in Chapter I that there are two fundamentally distinct approaches to systems thinking. Cybernetics can be viewed methodologically as a formalism--a calculus or notational system especially suitable for characterizing and analyzing complex and dynamic systems in interaction with their environments over time. Cybernetics has also been associated with the older "naturalist" version of systems theory which defines adaptation as unreflective problem-solving by which systems adjust incrementally to disturbances in their environments. Thus Steinbrunner describes simple cybernetic theories of adaptation as based on a limited repertoire of actions and a short-cycle feedback mechanism which monitors only a few variable factors in the environment. The "decision-maker" (or living system) then stands in a relation of opposition to the environment, screening out information which this established repertoire and set of responses is not programmed to accept. (Steinbrunner, 1974, pp. 51-57) What Steinbrunner is describing is the form in which negative feedbacks are exhibited in maintaining the structure of some system in interaction with a complex and changing environment.

Maruyama notes that many systems thinkers have tended to focus exclusively on the deviation-counteracting aspects--on negative feedbacks--as the mechanism of stability in self-regulating systems, and to overlook deviation-amplifying factors--or positive feedbacks--in those same systems. (Maruyama, 1963, pp. 163-164) For example, J. G. Miller defines cybernetics as the "study of the methods of feedback control," and argues that all living systems are characterized by some range of stability (or variability) for each of a number of factors, making up what have been identified in other contexts as "critical variables." Inputs which drive the values on these variables beyond the range of stability constitute a stress on the system, which produces strains in the system. In this way, "...outside stresses or threats are mirrored by inside strains..." as systems and environments change in continual interaction with each other. Stability in living systems is, therefore, controlled by negative feedbacks, and living systems are self-regulating, Miller argues, because inputs are adjusted by outputs as well as affecting outputs, with the result that the system "...adapts homeostatically to its environment." When variables which have been kept within some steady or equilibrium state--or range of stability--fail, then the system structure and process change markedly, "...perhaps to the extent that the system does not survive." (Miller, 1978, pp. 34-37)

A focus on negative feedbacks alone, however, only addresses the maintenance of form--or stability--in systems once established, but is insufficient to account for the generation or transformation of systems which takes place in processes of adaptation. At the level of the organism, Maruyama points out that neither the conventional "law of causality" nor negative feedback mechanisms can account for circumstances in which similar conditions product outcomes which are dissimilar--a phenomenon we have come to recognize as "equifinality."

Maruyama defines mutual causal systems as made up of both deviation-amplifying processes (morphogenesis) and deviation-counteracting processes or mechanisms (morphostasis). The various outcomes associated with equifinality in systems can be explained, he contends, even within a deterministic perspective, as the product of deviation-amplifying processes. Evolution--or "phylogenetic morphogenesis"--is a deviation-amplifying process, which accounts for the fact that the amount of information required to describe the adult structure of an organism is considerably larger than that carried in the genes. Rather than storing all the information which is represented in the complex mature system, the genes generate that information from a set of rules. In complex and dynamic systems, then, pattern is generated by the rules and by the interactions between the component parts of a system in conjunction with those rules. Information describing the resultant system is thus strictly deterministic. However, it cannot be recovered once the pattern has been completed. (Maruyama, 1963, pp. 168-171)

The key to explaining organizational learning as the basis for human adaptation through social organization in ongoing environments lies in the problem-solving responses to perceived uncertainties in those environments and to errors in the processes by which human beings interact with other systems. In the adaptation of living systems to their environments, and particularly in human adaptation through social organization, changes in form and/or behavior are driven by the experience of uncertainty and error.

At the collective level, the experience of uncertainty in the form of problems or contingencies for which we do not have an answer drives the effort to devise or to adopt new models and methods of control. Unintended--and undesired--outcomes are anomalies when viewed in the light of the methodologies on which control is predicated, and collective endeavors are therefore undertaken with the acknowledgement of a certain degree of risk or uncertainty in our understandings and in our ability to control processes of interest given those understandings. Efforts to coordinate people's activities which imply changes either in the objectives or methods of control by which collective endeavor is directed make explicit the underlying division of labor, and therefore render it open to change. Among the uncertainties in systems engineering, it is now coming to be widely recognized that the process of devising and implementing new methods of control in problem-solving and technological innovation and implementation involves activities which are inherently uncertain, and which introduce uncertainties into ongoing systems through the articulation of possibilities and choices, among them changes in operational arrangements, including the definition of work and working relationships. Such uncertainties arise at the collective--social structural--level of organization, and are thus significant factors making up the context or environment of living systems--in this case persons--at the individual level.

At the individual level, the perception of error takes place in human beings on more than one level of awareness--i.e., through more than one input channel. Humans are different from other species in the degree to which error and order are translated through feedback relations with the environment in symbolic as well as physiological processes. Living systems in constant interaction with other systems (which is what we mean when we refer to "the environment") adapt to changes in that environment through what systems engineers would call input-output mechanisms, which give to the individual organism a sense of disturbance--the experience of errors or problems. Error in the technical sense means that some criterion value among a set of critical factors maintaining the existence of a living system drops below some acceptable level. The operation of feedback mechanisms implies that this critical level is somehow monitored and communicated to the organism, and, further that there are mechanisms in the organism that adjust its behavior and/or physiological state--thus restoring the values on those critical variables to a satisfactory range in support of life.

Adaptation thus takes place in the human species both at the level of the individual, where we are concerned with the man-machine (or human-system) interface or relationship, and at the level of the group, where we are concerned with socio-technical system structure.

Because social organization is fundamental to the adaptation of humans in their environments, it is inappropriate to consider feedback mechanisms involved in the resolution of uncertainty and disturbances from the environment in purely psychological terms, referring to individuals in such a way as to imply that motivation and sentiment are the basis for social organization. Individuals do not adapt to their environments as individuals; they adapt to their environments by coordinating and directing their efforts collectively to selected and shared purposes.

Thus as individuals, human beings have two distinct types of what psychologists or systems engineers would call "affectors," or sites of sensory input--physiological and symbolic. Physiological processes may not be recognized, articulated, or understood by individuals, who may be "aware" of their experiences but not necessarily conscious of what is involved in that experience in the self-reflective sense which we associate with rationality or intelligence.

On the other hand, where the process of adaptation involves the organization of people in groups for purposes of effecting changes in the environment in order to sustain life, uncertainties--problematic experiences--are articulated and resolved through the use of language--or what Narasimhan calls language behavior. Through symbolic behavior human beings achieve understanding as a primary form of feedback between individual organisms and groups and their environments, enabling them to modify their behaviors and to create new objects (artifacts) including social organizations. At the organizational level, self-regulation is effected through symbolic expressions in a shared language which is the vehicle for the mutual construction of social realities. Symbolic constructs both represent action and are embodied in action (including social interaction and the construction of artefacts). In the case of office automation, underlying symbolic constructs or conventional understandings are embedded in the design of equipment, and in the processes and procedures constituting the kinds of investigations involved in design and use of that equipment.

The use of symbolic language involves understanding, production, and communication of information in the construction of social and technical realities which we think of as organizations. Social organization depends upon the articulation of a set of shared understandings as the basis for collective action--and thus for adaptation in ongoing environments. From this perspective we can focus on models of organization, not as an analytical or heuristic device for explaining what we cannot observe--namely, the ideas in people's minds--but as expressions or embodiments of symbolic conventions or constructs making up the environments and systems recognized by individuals and groups of people. Thus we can speak of an "organizational culture," as tacit models of organizations as mutually constructed, shared social realities emerging out of common orientations and common responses to a common environment.

Environment as a Meaning Context

We have viewed feedback as a reflexive relationship between individuals and their environments. This reflexivity is the essential characteristic of adaptation in all living systems. For G. H. Mead, the basis for the relationship between human beings and their environments is to be found in action, and the reflexivity involved in action in the environment is expressed in thought, as people reflect upon their experiences. Explaining human adaptation therefore requires a theory of mind, which recognizes ideas as arising out of conduct or action in a changing environment.

The essence of mind, according to Mead, is a reflection on the relation between things in the environment, and "the individual as another thing." In immediate experience--or perception--he argues, there is no reflection, and hence no mind and no knowledge.

> The locus of mind is not in the individual. Mental processes are fragments of the complex conduct of the individual in and on his environment. (Strauss, 1962, p. 84)

The relationship between individuals and their environment is mediated by their experience--and hence consciousness--and by the manner in which they focus their attention on certain events and objects. Mead argues that this relationship is a two-way street; the individual organism is affected causally by the environment, while the environment is determined selectively by the understanding of the individual in the context of his purposive action. Everything upon which the individual acts is functionally identified as part of the environment. This relationship holds for social objects--even persons--as they are perceived, or represented in immediate experience. The nature of that experience depends on the direction of individuals' attention, evidenced in action.

The self-consciousness that accompanies reflective--or intelligent--action of individuals is selective; those aspects of experience which are recognized are largely dependent upon the actions of the individual as an "agent" and, according to Mead, "In this sense the environment of the individual is relative to the individual." (Strauss, 1962, pp. 72-79) Thus is the environment constituted as an idea in the experience of some agent, which amounts first to a selective focus on those elements or stimuli which are relevant to some activity, and second, to the consideration of those elements as making up a certain local structure as objects and events. It is the activity of the individual which is central to understanding the adaptation of humans in their environments, and hence predictions based upon abstraction of physical particles and their motions from ongoing processes are insufficient to account for "the reality of a living being, for a living being acts." (Strauss, 1962, p. 93)

Langer reinforces this view of the selectivity of experience and the relativity of the perception of the environment to the experience of some actor. All action in the environment is both selective and reflexive, she argues, and regardless of the consciousness that may be implied in action, "The impingement of any act on another...affects a change in the situation of the new act impinged upon; and a change of situation is what motivates a new impulse." (Langer, 1972, pp. 141, 263)

It is the action and not the outcome of understanding and interpreting the environment which is critical to human social organization and adaptation. The activity of understanding and action upon the environment is fundamental to human beings as "agentive organisms," which R. Rarisimhan defines as living systems in continual interaction with their environments. Agentive organisms possess a "repertoire of actions" by which they are able to "explore, monitor, and manipulate the environment in various ways," producing out of the interaction a learning or developmental process which adds to their capabilities, and is reflected in the acquisition and production of language.

Narasimhan is concerned to explain the nature of language-behavior and argues that these interpretive and active aspects of language use are almost completely overlooked in conventional linguistic approaches to the theory of language, as well as in conventional methodological paradigms based upon the model of the physical sciences. Conventional methodologies fail to account for the agentive aspects of living systems and, therefore, cannot account for the interaction with the environment, which alone could explain not only the productions or expressions in a language--in the sense of an outcome of the process of language use--but the process of language use and production itself. The methods of the physical sciences are unable to account for the characteristics of agengive organisms because of the structure of the experimental method--customarily including quantification and mathematical analysis of physical object systems and their attributes as apprehended in closely controlled experimental systems. This approach necessarily closes the situation under study and builds into that situation all the elements of perception, selection, and action on the part of observers which are precisely those behaviors which we want to study in the subjects.

However, these are precisely the behaviors which are excluded by conventional experimental design. When seeking "to account for particular occurrences of actions or states of agents," Narashimhan argues, it is necessary for us to understand not just the types of actions agents are capable of and the manner in which they perform them, but also the basis on which the agent interprets and selects the information from its environment in such a way as to form an "appropriate" response. (Narasimhan, 1981, pp. 13-15)

Living organisms require the ability to learn, to add to their repertoire of actions and to modify their actions in the face of exigencies and new situations presented by the environments in which they find themselves. Learning depends upon past experiences, on evaluation, and on the contexts in which experiences occur. Furthermore, learning depends on the ability of the organism to discriminate among those experiences according to what he calls a "computation of relevance" as one of the "central aspects of the pragmatics of language behavior." Language behavior, according to Narasimhan, is one of a set of human, or socially-conditioned behaviors by which the human being interprets and acts upon his environment. He hypothesizes three closely related and interdependent processes--imitation, rehearsal or recounting one's experiences, and analogizing or role-playing--as underlying the "development of the child in general, and the development of language capabilities of the child in particular. Adaptive organisms grow through interpretation and learning--i.e., through an increasingly complex understanding of the environment taking place through the acquisition of language, human beings increase their repertoire of actions and capabilities. (Narasimhan, 1981, pp. 18, 133)

Herbert Simon (1962) argues that complexity lies in the environment, that the information-processing system itself is and should be as simple as possible, and that complexity arises from its interaction with those complexities in the environment. However, he never really accounts for how this simple organism can successfully interact with that environment, and even Ashby tells us that a controller must have at least as much variety as exists in its environment if it is to be able to manage or process or account for all the inputs entering from that environment. Simon's account of complexity is insufficient to resolve human understanding and social organization, and says nothing about the process of learning, nor how it is that this complexity in understanding and capability in human organisms grows to match the requirements of the environment. It is precisely this process of adaptation--of learning or self-regulation--that we have not been able to build into machine systems, and which is required to understand the adaptation of human beings into the introduction of machine systems in their environments.

Narasimhan argues instead that there is a natural scale for "delineating the complexity of behavioral pragmatics and development." Complexity increases in the agentive organism as that organism learns first to interpret static objects and then properties and states of those objects in the immediate present, and then to recognize the dynamic aspects of those environments--to recognize relations and processes, again in the present, to recognize events by abstraction of those relations and processes in space and time, to be able to talk about attributes and states independent of objects and events, and to account for events by describing and selecting, by naming and interrelating those relevant aspects important to understanding the environment. Complexity increases in the person in a natural way, Narasimhan claims, to match the complexity over time that that person encounters in his environment. By extension, one can imagine even a simple environment, relatively unchanging over time, supporting a building up of ever richer and more varied interpretations and expressions of associations, which is indeed what we find in sedentary societies and cultures with well-developed linguistic, artistic, philosophical and even scientific forms of expression in spite of--or perhaps because of long experience in relatively unchanging traditional societies. (Narasimhan, 1981, p. 129)

Thus Narasimhan advances the view of learning, of the acquisition of language and social organization, first outlined by writers of the Pragmatist tradition, including Dewey and Mead, who also focused on the central role of language in human understanding and action. According to Narasimhan, adaptation takes place through action which is based upon an awareness of the total behavior environment, which awareness is represented or characterized in those aspects of the environment which are recognized by the system, or those which are built into non-linguistic interfaces and physiological mechanisms registering state changes. He bases his arguments on two premises of central importance to the understanding of computer ergonomics: 1) Behaviorally, an organism is characterized by a set of "interfaces" which define those various aspects of the total behavioral environment of which that organism is aware and which determine the behavioral acts of which that organism is therefore capable; and 2) Those aspects representing the behavioral environment at the interfaces are "given" in terms of schemata and state-variables. Schemata are abstract structures--what we would call symbolic expressions, paradigms, frameworks, knowledge bases, indeed, languages themselves. State variables, on the other hand, are defined by "unstructured parameters" that characterize certain aspects of the environment and hence determine the behavior of the organism which is not symbolically structured. Those non-linguistic interfaces--for example, physiological mechanisms registering state changes such as heart rate, pupil dilation, salivation, or other physiological states and changes reflect interaction with various aspects of the environment. This interaction may not be consciously understood and may not be articulated by the organism.

The schemata, on the other hand, are the underlying expressions or abstract structures of language in which observations can be couched, and which therefore make it possible for people to articulate their experiences as representations of these behavioral aspects. (Narasimhan, 1981, pp. 36-39) The relationship between schemata and state-variable changes is not yet known, he acknowledges, but we would argue that it is precisely this interrelationship between such schemata as are represented in managerial ideologies and procedural definitions of work and state-variable changes characterizing the features-in-use of office equipment and the limitations in human physiology which is involved in the study of computer ergonomics. Here one of the central issues is the development of "software" and "human interfaces"--i.e., machine controls--which define the man-machine (or human system) environment.

Narasimhan argues that one of the major limitations of computational systems defined by programming languages, is that the programmer is required to learn what the system can understand and to write programs to match that, rather than to build a system which can understand and generate such programs as the user may require. (Narasimhan, 1981, p. 155) Contrast this situation with the fundamental role that language behavior plays in the adaptation of agentive organisms:

> Learning or acquiring a language involves acquiring (or more precisely, building up) the schemata available in the language to generate descriptions, commands and controls, and questions, and using them to describe, manipulate, and explore particular behavioural environments.... The interpretational system that gets built up as a child acquires his language behavior (then) consists of utterances... and their structured relationships. (Narasimhan, 1981, pp. 51-52)

Narasimhan's explanation is a solid alternative to the conventional formalisms of linguistic theory, which he criticizes; however, in spite of his emphasis on agency and on learning, we find little discussion in Narasimhan of the social-cultural, contextual aspects of language behavior. For humans, however, actions are social, and the reflexivity of action with state changes in the environment is augmented by the meanings which arise out of a broader context of action.

For Mead, these meanings arise out of actions and interactions in what he calls a "conversation of gestures," in which elements of action become stimuli to adjustments or further actions, which, in being enacted, come to take on the status of stimuli calling forth subsequent adjustments and actions. Meaning does not, therefore, inhere in physical or psychical states, but in "the structure of the social act," which involves a relationship between: 1) a gesture, 2) a response, and 3) a completion of the act initiated by the gesture and hence fulfillment of a social process. (Strauss, 1962, pp. 169-170, 183-184)

It is clear that experience involves action--understanding and communication if only with oneself--and thus that experience is a social phenomenon, and not merely a matter of personal motivation or interest. Mead argues on this ground that experience should, therefore, be approached from the "standpoint of society, at least from the standpoint of communication as essential to the social order." Meaning arises out of this activity of communication in experience and action within the larger context of cooperation in a human group via the use of language, and thus it is clear that meaning is inseparable from the active processes of living human beings in communication with each other. (Strauss, 1962, p. 128)

Reflexiveness, therefore, is an essential condition for the development of mind out of the social process of communication. The development of intelligence in human beings takes place through the symbolization of experience, made possible through the use of gestures--a process which is highly specialized in the human species. (Strauss, 1962, pp. 211, 140-141) Reflexiveness in the relationship of humans to their environments via the use of language is also the basis for the development and transformation of society.

Social order is a product of the process of human adaptation--taking place through the mutual construction of shared realities. In the evolution of human society, according to Mead, institutions embody social habits which have become established as a means for acting in the environment. Just as scientific technique is "doing consciously what takes place naturally in the evolution of forms," this ongoing life process is reflected in the conduct of individuals through their self-consciousness. (Strauss, 1962, pp. 18, 28-29)

> The structure of society lies in these social habits, and only in so far as we can take these social habits into ourselves can we become selves. (Strauss, 1962, p. 32)

As social habits, institutions represent attitudes assumed by individuals under varying social conditions. These institutions are thus reflected in the process of thought, which Mead defines as an activity which occurs through human intercommunication and participation in society. Social order emerges through the vehicle of language, by which humans are able to take on others' attitudes, talk to them, and reply in their language. Order in society, and the recognition of the existence of the individual self, both thus depend on the organization of common attitudes which are communicated in language and embodied in institutions. It is through the process of education, Mead argues, that these institutions--in the form of shared or generalized social attitudes or responses--are taken over by individuals. Education creates the social self as a member of a larger community, and, by extension, creates in the development and use of significant symbols a "universe of discourse," both as a reflection of and a basis for the social construction of reality in the form of a coherent social order and a meaningful social existence in the context of that social order. This "universe of discourse" is reflected in the language of the community, making possible the self-expressions of individuals as well as the coalescence of "those organized attitudes which represent the life of these different communities into such relationships that they can lead to a higher organization." (Strauss, 1962, pp. 261-262, 270-271, 290)

This ability to take on the attitudes of others and to rehearse them in ourselves is the basis for social control. Social control--or social order--is based upon the commonality of attitudes between individuals and the "community" in which individuals participate. On this basis Mead argues that evolution in human society is a process which occurs--not through the development of physiological responses as in other species--but through the development of "significant symbols," predominantly expressed by means of vocal gestures, which are significant both to others and to the actor uttering those gestures, thereby allowing them to focus on those aspects of mutual importance in their cooperative endeavors. (Strauss, 1962, pp. 35, 40-42) Thus the development of mind--or consciousness--is a process of social evolution of a piece with the development of institutions, both of which serve the ability of humans to control their environments.

There is also, in the universalization of those organized attitudes characteristic of particular communities, the basis for the annihilation of subjugation of individuals and communities, as one individual is capable of "realizing himself" in others. In the realization of self through others, there is the power to direct social undertakings and cooperative activities, which may lead on "...to the development of a higher community, where dominance takes the form of administration." Thus, Mead notes, the colonial form of organization leads to a transformation of the fluid exchange of communication among individuals in adaptation to their environment, into a situation in which the actions of one individual or group become functions--or derivations--of a subset of fixed meanings, objectives, and actions of another. (Strauss, 1962, pp. 271-272) In this way conflicts of interest and perception are inherent in organized societies, and for this reason human adaptation does not necessarily lead to ever larger and increasingly stable and complex systems, depending on the ways in which this process is worked out given such conflicts.

The Transformation of Reciprocity

Maruyama argued in general that mutual causal relationships could be either structure-generating or structure-preserving. Those loops in which the number of negative influences are even will be deviation-amplifying, while those in which there is an odd number of negative influences will be deviation-counteracting. (Maruyama, 1963, pp. 168-171; 174-179) The Weinbergs agree that not every feedback loop leads to stability, and (with Brand, 1974) criticize the simple notion of feedback "control" associated with the target metaphors arising out of World War II systems engineering. Feedback control systems are only a subset of all feedback systems, they argue, which systems also include those in which "no explicit set point or 'controller' can be found." (Weinberg and Weinberg, 1979, p. 192) Under certain circumstances it can also be the case that deviation-counteracting loops may become deviation-amplifying, a situation with significant implications for the implementation of change.

There are two ways in which this transformation from negative to positive feedback can take place: through competitive exclusion which represents a conflict or competition situation between two or more systems in interaction, and through a transformation of exchange relations among independent systems into hierarchical relationships of subordination and dominance between systems and subsystems. In the former, where the structure of a system is characterized by competition among its members, the limiting behavior of the members of the aggregate with respect to each other increases exponentially. If one component is smaller than the other, or grows more slowly, then a situation of competitive exclusion can take place, in which the larger or more rapidly growing component will effectively displace the other. (Weinberg and Weinberg, 1979, p. 146)

With respect to hierarchy, the Weinbergs argue that the best "surviving designs" will "always be found at a compromise point," reflecting the limiting factors on any regulatory strategy. What they call the broader "folk wisdom" of mutually opposing norms and values which "can cover all the bases" reflects the primacy of social control and normative convergence of opinion and action as the fundamental mechanisms of order in human society--underlying the development and imposition of all manner of formal, impersonal controls. Human beings are characterized by their generalized adaptation to the environment, and, according to the Weinbergs, are specialized only because of the limitations in machine systems. These limitations in designed or machine-based systems constrain the adaptive capabilities of both the human and machine components in socio-technical systems regulated by such impersonal controls. Machine systems cannot be considered as self-regulating because design, control, and maintenance must be continually provided by persons; thus the regulation of humans by machines limits the adaptive capabilities of the former in machine environments, and, by extension, limits the adaptiveness of machine systems in situations not conforming to the stipulations of the model. This adaptiveness or survivability is relatively weaker (or more constrained) in strongly integrated systems such as hierarchies in which the breakdown of one control mechanism--or critical variable--"will generally lead to a sudden failure of the others." (Weinberg and Weinberg, 1979, pp. 174-175, 183) In fact, competitive exclusion, and the development of hierarchies are not unrelated.

Competitive exclusion is a deviation-amplifying process which can be stated as an ecological principle, such that given two populations of self-reproducing systems, if one system having reproductive advantage is allowed to grow exponentially unchecked by some form of selective control it can expand to a point at which the other is altogether eliminated. In this way it is possible, in the case in which one powerful subsystem is able to consume unlimited resources, to produce a suprasystem in which competition among subsystems can drain resources from the total system to such an extent that the survival of the whole is threatened, as well as that of its individual parts.

Berrien defines adaptation in terms of a relationship between components and subsystems relative to inputs coming in from the environment, and outputs produced by the system in that environment. He defines components as units of the system which in combination with other units serve to compose inputs (by comparison, combination, and separation) to produce outputs. It is difficult, he notes, to distinguish components and subsystems, particularly once the elements of a system have been coupled. The key to the separability--and hence structure--in systems, and thus the identity of distinct components, is to be found in the strength of attraction among those units in a system. Forces of mutual attraction permit the components to function together as a unit; repelling forces preserve the identity of the components. Where the repelling forces are weak or absent, the original identities of the separate components are lost in the total system. (Berrien, 1968, pp. 17, 20)

Inputs to a system include the energies absorbed by the system and information introduced into it. These inputs may be either maintenance inputs, which energize the system and make it ready to function, or signal inputs, which provide the system with information to be processed. In social systems, the feedback relations between components and subsystems and the suprasystem of which they are a part produces a relationship between the output or task-accomplishment requirements (FA) of the suprasystem and the input or maintenance requirements of the various subsystems (GNS). In a balanced system there is a reciprocal relationship between effects stemming from internal and external sources; subsystems limit and thereby control the upper limits of the output or total system demands and the lower limits on their own maintenance requirements. The suprasystem, on the other hand, controls the upper limits of the provision of maintenance requirements to the subsystems and the lower limits of the requirements for task accomplishment at the total system level. (Berrien, 1968, pp. 117-118)

The relationship between the elements of a system in processing those inputs may take on one of two possible forms: 1) coupling with other collateral systems (which we might refer to as exchange relationships) and relations of subordination and superordination (which we might refer to as hierarchical relations). In coupling relations there is a mutual reciprocity of inputs and outputs in which the rate of exchange is limited by the channel capacities of each. As subsystems become integrated into ever-larger systems, however, their interdependence through collateral coupling declines in importance, and maintenance needs come to depend more heavily on feedback loops providing supplies from the suprasystem to the (now-dependent) sub-system. Thus, Berrien argues, that while the initial organization between systems may have been characterized by mutually-supporting coupling relationships, once organizations become established and begin adding more subsystems, the variability and uncertainty between these subsystems begins to increase overall, which requires some feedback mechanism to maintain the stability of the components and their interactions. (Berrien, 1968, p. 62)

This transformation from collateral to hierarchical coupling takes place because growth leads to increasing complexity which requires alteration in component structures and specialization of functions. There are consequences which accompany this type of structural transformation--or development--both for the individual components and for the organized system as a whole: 1) For the total system, there is a higher probability of successful adaptation and survival where specialized subsystems in performing their functions all contribute to the survival of the whole, which is thereby enabled to function more widely than can any of its individual components. It is necessary, Berrien argues, that suprasystems prevail over their subsystems in order that the total system grows and remains viable in its environment. This it does by blocking possible harmful (maintenance) inputs or errors which have a potential for disturbing systems at higher levels. (Berrien, 1968, pp. 84, 73, 136)

2) For the individual components in the system, however, the increased probability that the total system can deal successfully with the variability in its environment is met by a reduced ability to adapt successfully at the individual level. This is a consequence of specialization, which becomes necessary with increasing size, but which also "...carries with it a cost in restricting the subsystem's capacity to survive independently." From the point of view of the component, there is the composition of suprasystems a loss of

identity, as it is integrated into a subsystem of the whole. Initially, the components of a system are more tightly integrated and resistant to dissolution than in the total system; however, as hierarchical relations develop, at some point the integrating and dissolving forces between sub- and suprasystems is balanced, and beyond this point the individual components are merged into the totality, losing their separate existence.

At this point, the second consequence which arises in the transformation of exchange into hierarchical relations is that the component has become increasingly dependent on the larger system for its survival, while at the same time the chances for survival are qualitatively better for the whole than they are for any of its constitutent parts. According to Berrien, it is therefore, "...inescapable that in all growing systems a conflict will exist between the supra- and subsystems." (Berrien, 1968, pp. 84-86)

Odum defines this dynamic in terms of system capacity for self-repair. For Odum, evolution means the maintenance of order in the face of disordering influences--or entropy--through duplication, selection, and feedback. In general, he argues, we cannot preserve the structure in organizations unless order is continually being restored. All living structures maintain their characteristic form through their capacity for self-repair, which consumes energy. Some of that energy is contributed to the reorganization of units whose arrangement has brokwn down, others to replenish depleted energies. In general, the amount of energetic resources which must be diverted to repair and restoration of a system increases with increasing numbers of organisms which need to be maintained. If too little energy is contributed into duplicating and selecting system structure, error will come to exceed and repair and restoration and the system will progressively lose order. It is still possible for a system having experienced a loss of order at one level to continue to survive at a lower level of organization, providing that the balance between ordering and disordering forces in the system is restored at that level. (Odum, 1971, pp. 149-151) However, there are two potential dangers associated with such transformations--one for the individual components and one for the system as a whole.

In the case of the individual members of the system (and mindful of Berrien's relationship between the task-accomplishment needs of the total system and the subsystem maintenance requirements of its individual members) we can predict that those individuals or species whose work efforts are not reinforced will shortly be eliminated from the system, as they run out of raw materials and/or energetic inputs. For the system as a whole, Odum argues

> This tendency for runaway competitive exclusion of one part of a network is fearsome, ever-present danger against which all surviving systems must be protected by organizing influences. (Odum, 1971, pp. 53-54)

For the organization as a whole, power flows can become highly concentrated in monolithic, rigidly hierarchical organization structures, leading to a type of organizational "senescence" in which the physical attachments among units become so complex that the costs of repairing or of disengaging and replacing components and subsystems becomes too high to justify continuing maintenance of the system. (Odum, 1971, pp. 140-143) Thus it can happen that

> When a well-organized system is disrupted and its controls are destroyed, the parts may go into Malthusian competitive exclusion and in the process destroy the remnants of the system and itself. (Odum, 1971, p. 54)

Thus, Odum notes, in producing and maintaining organizational relationships, the cost of change involves the costs of making choices and selections which are required to hold the order stable. (Odum, 1971, p. 151) For Berrien, these costs will be associated with conflicts initially focused on the competition among collateral systems for scarce resources, and subsequently directed to the dependency-dominance relationships between subsystems and suprasystems which emerges through the extension of the integrative powers of the suprasystem. (Berrien, 1968, p. 175)

Implementation as a Process of Organizational Learning

We can now define organizational learning more fully as an adaptive process in which individuals' assumptions of reality and order are translated into actions and artifacts through processes of system definition, broadly conceived as a form of self-regulation. Organization can be seen as a process of constructing and legitimizing some definition of reality, which ultimately comes to be accepted or rejected by some relevant group of people through a process of operationalization--or implementation--having the status of an instantiation of a set of definitions. This instantiation takes place by articulating or embodying a set of ideas in some observable, concrete form--an artifact, such as a design for a machine or a machine itself. Formal organizations, defined as recognized social systems having a deliberate purpose, a definite boundary, and a specific origin at some point in time, are artifacts in this sense. The structure of formal organizations is thus fundamentally defined by custom and convention, perhaps forgotten or never explicitly articulated, but rather assumed by actors as they perform their roles in some process.

Implementation of technological innovation is a form of organizational learning which can be defined as a social process through which a designed system is introduced into some context over some period of time, through the selection of social and technical arrangements which constitute the interconnections--or structure--of that organization, and which then serve to constrain all further organizing. Implementation represents the instantiation of some system of ideas which establishes a mutual relationship between technological design and social action, the outcome of which is reflected in a socio-technical system--which can be understood after-the-fact by analyzing the models or systems of ideas in which that organization is expressed.

This suggests that the key to the study of organization is to be found in the concept of definition, which is a social process linking the organization of ideas and the organization of action, reflected in the dynamic structure of any given organization. Human adaptation is an iterative process involving the interaction of established beliefs with new experiences--a process which Peirce described as "abduction." As the essence of scientific reasoning, abduction is comprised of both deductive and inductive inference taking place iteratively over time, as people add to their understanding of their environments. (Buchler, 1955, p. 151) We can argue from this perspective that the basis for the feedback relation--or self-regulation--which characterizes the interaction of human beings and their environments thus lies in the process of attributing order or causality to the environment by building up interpretations of experience through the use of language.

The process of organizational learning can thus be conceptualized as comprised of a set of activities representing the "reality" of organizations in various definitions which come to be shared in action. A definition, or model, tells us about the structure of organizations as understood by people who have experience of them. Articulation amounts to the act of expressing those understandings, or the act of eliciting expressions, thereby making these (presumed) definitions explicit. Thus articulation amounts to the instantiation of a set of definitions in some medium of expression--which process constitutes an ordering relation in establishing organizational conventions, having the status of what we have been calling "provisional reification."

Implementation of new technologies is a form of organizational learning which represents the instantiation of some system of ideas by establishing a mutual relationship between technological design and social action, the outcome of which is reflected in a socio-technical system. Inasmuch as organization represents establishing some contingency--or interdependency--between the value of one component and that of another, organizational learning can be seen as the adaptation of a socio-technical system to contingencies in its environment. The notion of constraints--or contingency--reflects the changing structure of organizations over time as a series of outcomes of action at earlier time periods in the context of a changing set of technical specifications and an "organizational memory" exhibited in the current state of the system and in a body of knowledge making up the understanding of that system by its members and relevant audiences.

Systems Engineering as Organizational Learning: As exemplary of the mutual influence of models and the contexts in which they are implemented, the operations research movement illustrates not only the constructedness--or provisional reification--of organizations, but also the transformation of organized contexts through the institutionalization of broad programmes of research and development.

Research design is a matter of deciding how to collect, analyze, and represent data in order to show patterns--which is what we mean by "information." The methodology used in the process of research and development amounts to an organizing process which defines structure and thereby reduces uncertainty in the environment by producing such information. This methodology comprises an underlying knowledge base for organizing which consists of: 1) a research tradition, including a set of methods and instruments of data collection and analysis, analogous to Thom's kinematic models; and 2) a theoretical tradition, including a set of values, problems, and objectives as the ground for conceptualizing experience and directing action.

Systems engineering--whether consciously articulated or not, reflects the constructedness of systems through the instantiation of definitions of procedural algorithms and functional categories from which these procedures are drawn. These activities or methods can be defined as a conjunction between a body of knowledge and a sequence of actions (to be) performed in the context of this established set of definitions. In the development of organizational knowledge bases, the relationship of mutual causality which develops between systems analytic methodologies and organization theories is manifest 1) in the description of complex organizations which supplies the context of application for the use of analytic methods of design and control, and 2) in analytic methodologies providing the specification of the means of control and production in the form of instruments and methods for effecting organizational objectives, thereby supplying structure in ongoing systems, as well as a set of methods for analyzing that structure in its various instantiations.

Operations research--or applied engineering, models are explicitly normative and prescriptive; directed to tangible goals, they create order, which represents the context for the development of subsequent forms of order. They are designs for building or changing some aspects of the environment, and the process of development is based upon inquiry into the conditions which are required in order to fulfill that design in some context. The operations researchers reified this type of model, by instantiating their designs and formal theories in the development of new technologies--notably computers--understood as the production of knowledge about new products and production processes. They further objectified their work by attempting to define what it was that they did, by formalizing concepts--such as the abstract system, feedback, and automata--and by codifying the methods and requirements of the processes by which they developed these concepts, and from them designs for new technologies. This reification can be seen in the literature of systems engineering, and especially in the more recent historical accounts of systems engineering in the World War II era.

As we have seen in Chapter II, the specific programmes of research--including the underlying models comprised of research objectives and methods for carrying them out--varied widely between different operations research groups. Moreover, the working models and methods of inquiry and design differed according to local and functional (problem) environments in which they were developed and implemented. The diverse activities of the operations research teams and their field counterparts were coordinated through a broader methodology involving, as we have seen, the establishment of cooperative and dynamic committees and agencies, funded and coordinated through overlapping memberships in various networks of association. Through the innovation of such organizational expedients as the "few-quick" organization, and the institution of technical liaison specialists in field applications (both in the initial stages of research and in the implementation of the products of that research), the particular methods of research and development within each problem area were extended at each stage in the investigation into specific organizational arrangements, from basic research to the development of prototypes, to field testing, to production and finally implementation. These activities reflect an underlying methodology, broadly conceived, which represents the context in which the organization for inquiry and production is embedded, and by which development is directed--i.e., constrained and supported.

The reification of operations research as a "movement" was also embodied in the maintenance over time of a network of social interaction among the persons involved in this type of work--the essence of a professional community--and their association with new and existing businesses and governmental organizations. Here we can see informal, social networks of association as the basis for building new institutions and restructuring others (as in the staffing of the NDRC). We can also see the institutional interconnectedness associated with these informal networks through the positions which their members occupied in different organizations, providing access to the resources and constraints in these institutions in the process.

This interconnectedness constituted the context for development and production. In the operations research movement, a set of analytic methods and a broader methodology of directed research became codified in the theory of cybernetics and in the procedures of operations research, and institutionalized in the development of new agencies and technologies, reflected in the emergence of new professions.

In this process of institutionalization we can see the influence of developmental context in several senses: 1) in the transformation of the context of development through the stages of problem-solving from initial conception to field implementation--now referred to in terms of organizational learning curves; 2) in the postwar transformations of the context of development, which came about at least in part by institutionalizing the research into new agencies and organizations and in the newly emerging professions of applied mathematics, including computer science. Historical parallels in postwar transformations of the context of development for science-in-industry reveal two major constraints on further development and on the efficient transference of new methods of research and production in ongoing organizations: A capability for supporting further innovation and development, and an ability to manage the emergence of new tasks and working environments, with implications for the acceptability and effectiveness of new production processes as viewed in terms of health hazards and working conditions. 3) Finally, the postwar transference of the methodology of operations research (broadly conceived) illustrates the influence of differences in the context of application, reflected in the different ways in which operations research was institutionalized following the war.

Operations research methodologies changed the context for production and development in corporate organizations through the introduction of systems engineering methodologies into businesses following World War II--reflecting similar changes following World

War I. By extension, differences in commercial contexts of application following World War II changed the nature of operations research in postwar contexts in various ways as that research was directed to different ends in Britain and the U.S. Finally, the methodology of operations research was extended as those who had become skilled in this area carried out their researches in new problem areas--such as the aerospace and automobile industries in the U.S., and the mining industry in Great Britain. These contexts can be contrasted as a basis for explaining the differences in outcomes for the application of operations research (systems engineering) methodologies in organization theory and management, and in fact can be cited as the basis for differences in human factors engineering and ergonomics--both of which claim a "contingency" approach to the study of organization(s).

The implementation of computer-based technologies since World War II displays such transformations in structure as a function of technological change--in which the process of organization learning is associated with a "learning curve" which represents the progression of systems definition through stages of non-routine, idiosyncratic and ad hoc assumptions or presumptions implicitly characterizing the system in operation, to fully routine, automated processing of a set of designed and tested programs. It has been argued that various administrative structures and strategies may be compatible or incompatible with different stages of this learning curve, and specifically that flexible, slack organizational arrangements and decentralized access to information and decision-making are more appropriate to non-routine planning and problem-solving phases of organizing, while centralized programmed control is appropriate to the operation of routine technologies in stable environments. (Nolan, 1979, p. 118; Bannester, 1970, p. 425)

The practical insight of the process view of organization is that the preservation of strong and inflexible administrative control systems during the non-routine system definition phases of design and implementation inhibits innovation and can undermine conversion to a new system altogether. At the other extreme, the multiplicity of decision points in complex systems, and the looseness of decentralized coordination when coupled with routine, programmed operations can lead to increased waste and slack in operations and therefore to increased costs, and to errors in fulfilling system requirements. The synthetic advantage of a decentralized-centralized form of organization, which is characteristic of the organization for inquiry exhibited in wartime systems engineering, is that it permits (given the presumption of a context-free environment) changes in the configuration of the networks of communication according to the requirements of the process. This flexibility also serves to facilitate social interaction in the coordination of decentralized project teams, which is necessary in order to develop and support non-routine operations, among which are the processes of research and development and the implementation of new technologies emerging from those processes.

Transformation in the Context of Development

The opposition of the constructed world with the process of development has many historical precedents, but perhaps none so dramatic as the transformation of the context of development for new technologies and production systems from wartime to postwar contexts. In the exigency of war, as Thomas Paine noted in his essays on the "American Crisis," experience and misfortune teach us "system and method"; and in institutionalizing this system and method, "the arrangements for carrying on the war are reduced to rule and order." (Paine, quoted in Foner, 1948, p. 195) In postwar contexts, methods of design and control developed in the context of wartime research and development are implemented in civilian production environments, which are altered by the introduction of these new "technologies." In so doing, the conditions for future innovation and development are transformed significantly from those characteristic of wartime, not infrequently resulting in difficulties reproducing original successes and, in some cases, health hazards and adverse health effects on individuals working in these--now transformed--environments.

As a context for technological development, times of war are characterized by several features which contrast in marked fashion to peacetime developmental contexts--i.e., the environments into which new products developed in wartime are to be implemented. In wartime, research is broadly directed to complex, problem-solving objectives ultimately oriented to survival. The exigency of war demands and supports a progressive context for development which is organized to actively seek out and communicate new knowledge. The seriousness of military conflict means, furthermore, that development is not subordinated to considerations of profitability, or strictures of cost-accounting and measures of productivity.

When compared with wartime research and development, post-war contexts are relatively constrained, either through the destruction of physical plant and capital, or through the transformation of problems and objectives guiding the research and development effort--a transformation which accompanies demobilization and the transference of experimental technologies into civilian production environments.

1) In wartime, the overriding objective is survival and winning the conflict, an interest in which all share above and beyond their separate personal and corporate objectives, including productivity. Returning to civilian production, however, realigns priorities to focus on issues of cost and profit. The relationship of individuals to organizations changes, first because demobilization entails a loss of the solidarity and sense of consensus--or community--which exists during emergencies, and second, because a focus on productivity and on lowering production costs--especially labor costs--implies inherent conflicts of interest between employers and employees, conflicts which are directly implicated by changes in production technologies.

2) In wartime, everyone is temporarily released from their customary civilian statuses, and from institutional and class allegiances. All are placed under a single authority, upheld by

the most extreme sanctions, suspending their own self-interests in the interests of the whole. In civilian life, the world is institutionally bounded; war breaks down the fabric of organized social life by destroying or transforming major institutions, and is thus perhaps the most dramatic force preserving the logic of "context-free" development.

While the civilian context is customarily comprised of a multiplicity of separate institutions, each a corporate hierarchy with its own chain of command deriving from ownership, the wartime context for systems engineering (in both World War I and II) was made up of a network of contractual and technical inter-dependencies, managed by agencies of the government and established under the authority of the President. In postwar demobilization not only is labor reduced to a commodity in the total process, but the relationship among individuals who had cooperated on different aspects of some problem becomes fragmented as they are dispersed throughout a number of individual institutions, each bounded by restricted, hierarchical networks of communication, and working on pre-determined problems in the context of ongoing production processes and priorities.

Once survival is no longer in question, optimization becomes the goal. In the postwar period, innovations developed in the course of wartime become routinized and institutionalized in corporate entities and objectives. Once these arrangements have been codified, the environment is no longer context-free and social relations become competitive rather than consensual. The underlying mechanism by which this transformation takes place is institutionalization--through which new corporate entities and professions become established as the context for innovation and development in the future.

Institutionalization and Professionalization: Professionalization and institutionalization are social processes in which outcomes clearly constrain the process of ongoing social organization. In the provisional reification of socio-technical organizations, both serve to rationalize the context of inquiry and production, and in the process to limit access to inquiry and decision-making. The formalization involved in institutionalizing emergent roles and relationships into new agencies or new positions in ongoing agencies, and in the certification of individuals practicing in some new area establishes restrictions on opportunities for occupational recognition and mobility, by imposing and routinizing a set of standard meanings on a definable universe of discourse, thereby reducing the knowledge base and the process of knowledge acquisition to a common framework or paradigm which serves to make that knowledge cumulative and communicable--and thus capable of being incorporated into educational programs, and identified with individuals.

Professionalization takes place, in part, by formalizing the training which practitioners receive, and in part by developing professional associations to recognize and represent the members of some putative discipline. On the basis of an empirical study of the interrelationships among a number of occupational characteristics associated with professionalism, Cullen has argued that professionalism is essentially defined with respect to two distinct criteria: 1) task-required talent in some complex and intricate occupational activity, and 2) the relative power of different occupational groups. Cullen found, holding the effects of intellectual complexity constant, that the organizational power of professional associations increased the ability of occupational groups to achieve the characteristic features and rewards of professionalism--namely, university-based education, higher income, and prestigious social status. (Cullen, 1978, pp. 1-2)

If we do focus on the intellectual rather than the corporate component of professionalism, however, it is clear that professionalism is a form of social organization which defines boundaries around given domains of knowledge and establishes conditions for membership based upon those knowledge domains. Codifying the history, concepts, and methods of emerging disciplines and formally recognizing new professions and agencies transforms loose, shifting, and flexible networks of intercommunication into institutionalized--usually hierarchical--structures. This process of institutional formalization is equivalent to Weber's notion of "rationalization," by which he means the routinization of the ad hoc social arrangements associated with charismatic leadership and extraordinary periods in history.

One striking characteristic which recurs in the accounts of the operations researchers in World War II, and in the discussions of social organization by Berger and Pullberg, Sombart, Dewey, Mead, Russell, and deToqueville is the influence of historical breakdowns in established social order--breakdowns which call into question the ontological status of what had been considered relatively permanent entities in the social world, and which engender a focus on the processes and presumptions which underlie the recognition of some social order.

Berger and Pullberg note that conscious awareness of human activity and understanding in the formation of social roles and institutions is often gained under extraordinary circumstances: 1) the disintegration of social structures, 2) cultural contact; and 3) the experience of social marginality in a cultural context--all characteristics of wartime, as the closest one can get to a "context-free" environment for development. In the breakdown of social order, these roles and institutions suddenly appear as "what they are"--namely the products of human cooperation, choice, and action. Thus it is that "times of troubles" can serve to de-reify and often to de-mystify the world, thus liberating human consciousness and reawakening an awareness of the world "as an open human possibility." (Berger and Pullberg, 1966, p. 68)

Mannheim notes that concern with forms of thought, and a focus of attention on ways of thinking, arise in situations characterized by a multiplicity of divergent definitions of reality--a situation commonly associated with periods of social mobility and change. Experience in such situations destroys the "illusion" of the continuity of forms of thought--forms which prevail in spite of changes taking place on the material plane. The social transformations on which Mannheim focused were the general democratization of society which occurred throughout the 19th century. He argues that horizontal mobility from one community to another within what was essentially the same social status level demonstrated the differences in people's thinking without necessarily shaking their confidence in their own thought. It is vertical mobility between strata in "social ascent and descent"--what Durkheim called "anomie"--which shakes individuals' beliefs in the presumptive validity of their own forms of thought. (Mannheim, 1935, pp. 7-8)

Democratization means not only that vertical mobility is taking place, according to Mannheim, but also that the thinking of the lower strata acquires public significance--and hence validity and prestige--a significance that it had not formerly enjoyed in a well-stabilized society. The public significance of "popular" thought is offset in ordinary times by that of the intelligensia--that class of people which exists in every society, but especially in well-defined, stable societies, in order to "provide an interpretation of the world for that society."

In stable societies, the intellectual stratum can monopolize the knowledge of that society in presumptive and exclusive rights to "preach, teach, and interpret the world"--a condition which is immediately evident in the binding quality of dogma in theocratic societies, and which is striking by its absence in the early stages of colonial societies (which, by their geographic isolation, managed to break the bonds of tradition without the kind of overt struggle characteristic of social change in established societies). Where thinking is monopolized, the orientation to learning tends to be scholastic in nature, and intellectual activity is characterized by "its relative remoteness from the open conflicts of everyday life." In this mode, the progress of thought takes place apart from the activity of problem-solving. Mannheim and Weber both argued that what appears to be a modern emphasis on "systematization" is actually quite in keeping with scholastic thought, as embodied both in the study of science and law, and maintained by the social continuity of institutions of higher learning. The decisive transformation characterizing the transition from the middle ages to the modern world, Mannheim argues, is to be found in breaking of the monopoly of scholasticism. (Mannheim, 1936, pp. 270, 11)

Thus it is that times of troubles--or conditions of uncertainty--may become times of opportunity, and that the process of addressing problems which point up the limitations of established structures of knowledge (as monopolized by the intelligensia) may become the basis for a reorganization of society through individual and organizational learning which improves the capabilities of human beings both at the individual and at the collective level. This is the mark of intelligent adaptation in a changing environment, and the role of science in human adaptation. In this context, then, Dewey describes the disciplined mind as one which takes delight in the problematic, one for whom

> The questionable becomes an active questioning, a search...The scientific attitude may almost be defined as that which is capable of enjoying the doubtful; scientific method is, in one aspect, a technique for making a productive use of doubt by converting it into operations of definite inquiry. (Dewey, 1929, p. 228)

Wartime clearly presents this type of exigency, which overrides established understandings and interests, opening up lines of stratification, and which thus constitutes a favorable context for development. Institutionalizing and consolidating the gains made in wartime research and development, however, turns this organizing process back upon itself, imposing constraints on further development in direct proportion to initial successes. As we have suggested, this transformation takes place through the process of formalizing that knowledge as the basis for establishing new professions and institutons.

In this transformation, the processes of professionalization and institutionalization tend to be accompanied by conflicts and competition among organization members, as new conventions come to be accepted or rejected in established fields of knowledge, and are reflected in the development of new industries and technologies, and in the emergence of new or changed social statuses for individuals in those organized contexts. These conflicts arise out of two fundamental problems originating in the processes of organizational development and change associated with the introduction of new production technologies and methods of work:

1) First, narrowing the range of mobility which can be achieved through innovation and the acquisition of skill contradicts long-standing expectations of mobility, especially in American culture. These expectations may be different in the U.S. and in Europe; in the U.S. they can be traced to a colonial "republicanism" which saw in the applications of science-in-war and -industry a redemptive

value for the organization of society and for the improvement of individuals-both based upon an ideal of perfectability, or constructability, of institutions and persons.

2) Second, the strains which are associated with occupational and professional change and competition taking place through a period of institutionalization--or the implementation of technologically-induced change--have been accompanied in the past by a serious deterioration in working conditions, associated with the introduction of new equipment and new work procedures, and pointing up the limitations in established systems of knowledge for research and design. Hardships for workers in World War I led to the emergence of the field of human factors engineering; similar problems during and after World War II led to an extension of human factors research into the field of socio-technical systems analysis.

In the context of the American Revolutionary War era, republican ideals linked scientific knowledge to enlightenment, and held that the scientific worldview was liberating of both individuals and institutions from the bonds of ignorance and the shackled of medieval institutions. By extension, science was a legitimate ally of policy as the method of improving institutions and making more perfect societies, as it was of the rights of man, by which even the commonest citizen could be improved indefinitely by education. (Price, 1965, p. 88)

In Dewey's pragmatism, perfectability is tied to learning and experience; growth through learning is the link between moral and natural law, and enlightenment is the basis for social justice. This enlightenment comes about through participation in social interactions which constitute the public life of the collectivity, establishing the ground on which individuals learn of the world and of their own identities in it. Identity--or the individual consciousness of self--is based on one's connectivity with the underlying natural order, gained through experience in and of the world. This is the basis for individual liberty and responsibility. The hope of the oppressed is not, therefore, a species of false consciousness, but the expression of the conscious experience of every living person of a sense of the involvement and participation in the social world of which he is a part.

Perhaps one of the keenest observers of the differences in worldview between American and European society was de Toqueville, who attributed those differences to the unique experience of the body politic in colonial societies. He observed that although the ideal of individual perfectability had long been recognized as peculiar to human existence, in highly stratified societies individuals come to define the human condition and the limits of human powers in proximity to themselves. Indeed, they can do no other than to see the world in the images with which it is presented to them. We might argue that it is this limitation in worldview which is the ground of "false consciousness," which is really a boundedness in the consciousness of individuals based on their view of society from within a restricted and static position in the social order. It is under the circumstances of social change--where castes and classes are disappearing or being transformed, as Berger and Pullberg note, that people observe that institutions are neither infallible nor permanent, and

> ...the image of an ideal perfection forever on the wing, presents itself to the human mind.
> (de Toqueville, in Curti, 1960, p. 211)

The spirit of personal liberation which accompanied the beginnings of science and industry in post-colonial America fostered a host of possibilities born of necessity and unconstrained by the kinds of obstacles presented by more rigidly ordered societies. In this liberated environment science flourished for a time, in part because of the wide range of thought permitted and in part because of the heightened sense of urgency, uncertainty, and need which tended to accompany war and the development of colonial society. In colonial and post-colonial America, the opening up of a new world provided a wealth of data to research; during this period of active and significant scientific inquiry, laymen contributed to the natural history of the colonies, and a lively network of scientific relationships linked observers of the American scene with established scientists and educational institutions in Europe. (Greene, 1968, p. 26)

This congenial environment was, however, transformed by the continuing growth in American institutions. In the period following the revolutionary era, ties with European scholars were attenuated, and in the U.S. there was a rising sensitivity about the dependence of American science on Europe. As institutions of higher learning and the learned professions became established, the focus of attention turned away from a cataloguing of the American landscape and toward an appeal to traditional European institutions and styles of thought in an attempt to gain respectability for American science through the credentials of established fields of inquiry.

At the same time, the development of science--like other developments in the U.S.--proceeded along regional lines, and therefore suffered from the tenuousness of communications, which meant that work undertaken at any given location could not hope to achieve national, much less international, stature. Moreover, the process of institutionalizing knowledge, and establishing closed and privileged professions initiated fundamental conflicts between the "lay public," engaged as it was in all manner of problem-solving in complex and changing environments, and the emerging professions. Thus as the new young nation accelerated its striving for respectability, the liberal democratic worldview of Paine, Jefferson, and Franklin--the belief in the constructedness of the institutions of state and the sovereignity of the individual, and with it the vitality of scientific inquiry--gradually came to be replaced by an increasing emphasis on religious faith and morality dominated by evangelical protestantism and social darwinism. According to Greene, by 1820 the colonial efflorescence in American science--which had participated richly in the European, and especially British, tradition--had effectively ended. (Greene, 1968, pp. 38-40; Gabriel, 1940, pp. 19, 25, 131)

According to Daniels, the same cycle of development and decline accompanied the end of the Civil War. The Civil War period was marked by a separation and declaration of independence within the American scientific community from its earlier justification on the basis of practical, utilitarian, and equalitarian values shared in common with the general population. As Daniels points out, this professional independence came about at the highest point of American science, which had become well-entrenched in government and industry. The professionalization which separated the lay public from the scientific community in this era was based upon a rejection of the former "ideal" balance between teaching, research, and practical applications, in favor of an emphasis on professional contributions to the advancement of science, the preparation of entrants for professional work, and the development of a small number of "first class institutions." (Daniels, 1967, p. 1703)

This professionalization process, which Daniels argues was "the most significant development in 19th century American science," took place across four stages: preemption, institutionalization, legitimization, and achieving autonomy. 1) Preemption comes about when tasks which formerly were performed by a given group of people or by laypersons in general come to be the "exclusive possession" of some other group, a phenomenon which occurs as a function of the increasing complexity and esoteric nature of the task. At some point, an "edge of incomprehensibility" is reached, beyond which the knowledge is inaccessible to the general practitioner.

This stage not infrequently has provoked outcries from people's rejected from a particular field of study (which Kuhn also noted) and often occurs in conjunction with changes in the classification and organization of subject matter. In the 1840's, according to Daniels, such a change transformed "natural philosophy" into a body of esoteric knowledge called "science." This change created on the one side a body of professional scientists and on the other a body of lecturers or amateur students who increasingly were portrayed in an invidious light as "popularizers"--people who retailed scientific "wonders" but who no longer participated in advancing scientific understanding--in the mode of Watson's "behavioral science entrepreneurs." (Daniels, 1967, p. 15)

We have argued that the transformation of context from wartime to peacetime divided organizational populations into two groups--those with a proprietary interest in the corporation, and those who were merely its employees, or labor resources. Similarly, the transformation brought about by the emergence of new fields of knowledge--such as computer science--has also been accompanied by a division of organizational populations into two groups--designers and operators--referring to those people having specialized knowledge and those without, and who are therefore limited to carrying out the procedures and orders of others. In office automation, the latter group has largely meant secretaries and managers vis-a-vis programmers and systems analysts.

2) Institutionalization occurs when behavior is structured in patterns which provide a means for standardizing relationships among professional colleagues and with outsiders. This structuring of behavior comes about largely through the organization of professional associations which provides "normative force" for professional aims, especially through the power of these organizations to restrict entry to the field by setting qualifications for membership, and by

controlling the emergence of certified knowledge through the management of publications. Daniels points out the role of codifying--and therefore institutionalizing new knowledge in the publication of articles and books, often undertaken by founding journals of new publishing houses. In the case of computer science, this has meant a proliferation of magazines and newsletters. The institutionalization of new knowledge also often implies the founding of new schools, where the attempt to change curricula in established institutions becomes a major undertaking, calling forth conflicts with established disciplines seeking to control entry to the field and the generation of new knowledge.

3) In the legitimation of an (erstwhile) esoteric body of knowledge and method within the larger socio-cultural context Daniels notes that a major issue which arises in removing a body of knowledge from "the public domain" concerns the rationale for the public support of new professions and their practitioners who must walk a tightrope between unrestrained pursuit of knowledge for its own sake, and demonstration of the utility of that knowledge to the public. In America two traditional arenas for justifying the pursuit of specialized scientific knowledge have been in demonstrating the practical utility of the knowledge to be gained, in demonstrating that such knowledge and its pursuit are religiously significant, and thus important to higher values shared within the society at large. In 'practical' America

> ...one of the most characteristic means of establishing public contact has been the making of extravagant--often irresponsible--overstatements concerning the immediate utility of the research. (Daniels, 1967, p. 161)

There are parallels with the introduction of computers, especially as new knowledge disseminated in books and periodicals published by computer manufacturers and consultants have promised a fully-integrated Office of the Future for nearly two decades, in spite of continuing problems.

4) Finally, the achievement of professional autonomy means the ability of the profession to police itself. Daniels argues that American science became established on the basis of a compromise between practical utility and the ideal of the "free" professions, expressed in the formula that "all science would ultimately prove useful, but that utility was not to be a test of scientific work." (Daniels, 1967, p. 165)

Thus a notable shift took place away from the revolutionary ideals of science as permeating practical activity and accessible to the general population. This transformation had the effect of initiating competition and conflicts in the drawing of boundaries around professional competence and membership in professional occupations, as well as a progressive conservatism and increasing remoteness from practical affairs on the part of those educated in the professions. This remoteness insulated professional knowledge from major changes taking place in the environment, rendering that knowledge progressively less effective in explaining and controlling events in those environments, and thus undermining future progress.

Mannheim argues that although the existence of formalized and abstracted thought originates in situations which motivate the acquisition of knowledge tend to become obscured in the increasing formalization and abstraction of that knowledge. (Mannheim, 1936, pp. 302-305) One simple way in which the context for development can be transformed and constrained is by focusing on the teaching of abstract formulae, leaving out of the textbook accounts any description of the context and objectives which underlie the development of such formulae--thus undercutting the reflexivity which is the hallmark of scientific reasoning. In translating the accomplishments of the work of the World War II operations researchers into education for the professions, for example, it has been the case that the broader methodologies of research--to include processes of social organization for inquiry and the generation and implementation of models in active, ongoing environments--are left out of that which is codified and taught to the next generation. Thus we learn linear programming, CPM and PERT; we master predicate logic and computer programming languages; however, we can't express ourselves and our experiences in those languages because they are not learned in the context of any activity--or experience; they are learned as isolated formalisms and are thus separated from their (potential) practical uses, by which they take on meaning and coherence.

According to Mannheim, the upward progression by which knowledge becomes increasingly formalized and universalized can be attributed to the increasing consolidation of social groups, in which ever-broader bases for the construction of knowledge necessarily pushes the level of abstraction ever higher, and understanding is realized in some formal system of thought representing the phenomenon of experience at a higher level of abstraction. At some point, only the formal mechanism itself is apparent in the operation of such systems; the experience which underlies that mechanism is embedded in the stages of its formation. It is this sort of embeddedness which Berger and Pullberg refer to as the "forgetting" which is the essence of reification, and it is just this type of reification which we see in the operations research movement, in particular, and in the institutionalization of the methods and implements of wartime innovation, in general.

Conclusion

The key element in all of these accounts of the constructedness of organizations--the element which remains when presumptions of some underlying order are foregone--is the fundamental "dialectic" (the dynamic tension and opposition in synthesis or generation of form) between the process of organization and the forms of organization which emerge from that process and which represent the context within which that process occurs.

> The most basic, generic contradiction is that between the constructed social world and the ongoing process of social construction. (Benson, 1977, p. 16)

The transformation in the context of development from wartime innovation to peacetime production brought about a transformation in the flow of exchange of resources and information, and hence in the structure of intra- and inter-organizational relations. The underlying logic of that change process is that of a transformation in the relations of exchange between independent systems into hierarchical relationships based upon fixed objectives and prerogatives. What we see in the transformation of exchange relations into hierarchies is the transformation from deviation--amplifying--or developmental--processes to deviation-counteracting--consolidating and optimizing--processes. What is lost in the "translation" is the reciprocity or mutual causality which characterizes exchange relations, the sense of a two-way feedback relationship with others' systems in the environment.

For the individual, the sense of involvement in the system is lost to an increasingly narrow and instrumental definition of the role of the person in designed processes. For the organization, what is lost is no less than the strategic vitality of the system in its broader--competitive--environment. What we are describing is a positive feedback phenomenon in organizational learning by which the activities involved in systems engineering--i.e., the transformation of structure or context via the implementation of abstract constructs and artifacts which constrain and direct action and outcomes--can interrupt ongoing interdependencies in such a way that the system is no longer directly in touch with its environment. It is in this context that the remoteness of design becomes a significant factor in breaking down the reflexivity which is basic to organizational learning--or adaptation.

As we have seen, adaptation, or organizational learning flourishes in contexts which are relatively unconstrained--in which resources are provided without necessarily accounting for return or profit, and in which few limitations exist on wide-ranging intercommunication among participants. Moreover, the methods and models which organize knowledge in developing new systems are most effective when reflected upon self-consciously, and the information gained from such reflection-before-action is applied to the process of inquiry and organization to guide further development and problem-solving.

The transformation of context turns on two factors amounting to a constraint on this process of organization: 1) a problem- or objective-focus on cost-benefit analysis and optimization of returns on investment, which when focusing on labor relations issues translates into the objective of increasing productivity as the criterion value underlying implementation; and 2) a restriction on the flow of communication and decision-making, which centralizes--or recentralizes--decision-making and access to information, limiting the scope of information at each position in the system and leading to progressive management information crises. Structures which were based on variable networks of communication and cooperation relative to the requirements of ongoing projects are replaced by functional hierarchies based upon the accountability of the workforce and the relative costs and benefits of alternative ways of configuring production processes.

The transformation of the exchange of information and resources throughout the system has the unfortunate consequence of rigidifying the relation of the organization to its environment by fixing input-output relations (especially by imposing restrictive input functions upon the system) and then rationalizing the production process to eliminate alternatives--or uncertainties). In this way the type of iterative inquiry characteristic of multidisciplinary networks of researchers in wartime research and development is constrained by organizational hierarchies with their restricted communication flows, and by cost-benefit criteria deriving from budgetary constraints. Research undertaken under these conditions corresponds to what Baxter referred to as "forced draft research," what Wymore referred to as "system function analysis," and what Kuhn called "normal science." (Baxter, 1946, p. 407; Wymore, 1976, p. 368; Kuhn, 1970, p. 24)

According to Wymore, system function analysis is basically a prescriptive idea that an engineer

> ...should first identify all the functions that the system he is designing is to perform. Then, knowing the functions and their interrelationships with respect to input/output, he assigns these functions to various hardware components of the system by which they will be specifically performed. (Wymore, 1976, p. 368)

The method or procedure for doing system function analysis is to identify basic high-level functions and break down their relationships into a block diagram, decomposing each one of these functions into smaller subfunctions, each related by another block diagram. How is this different from modern systems engineering?

> A system-function analysis certainly implies an input/output specification. A system-function analysis implies, with less certainty, some assumptions about the technology. A system-function analysis says almost nothing about merit orderings or a system test plan." (Wymore, 1976, p. 368)

Given a restricted developmental context, it is difficult to reproduce initial organizing or innovating successes because of the constraints added to the context in the course of the research and development activities. In general, while codification and formalization serve to rationalize and consolidate experimental successes, at the same time the institutionalization of a programme for scientific inquiry and problem-solving which is too narrow can exert a stultifying effect on the development of new forms of knowledge, and on the confrontation of conventional wisdom with experience in an ever-changing environment. Over time, the institutionalization of paradigms for research and for managing organizations may crystallize into static systems of knowledge, reinforced by their incorporation into emerging academic disciplines and professions, and embodied in a set of credentials. The organization of the professions--specifically, the limitation of access and the routinization of practice--can, therefore, serve to defend against new knowledge and new ways of knowing.

In the commercial realm, although continuing rationalization of the means of production serves to increase profits, at some point a threshhold is reached in lowering the costs of production, and "genuine" technological innovation is once again required. We might argue that this "second-stage, product innovation phase" in computerization was buffered in the U.S. by the lack of institutional constraints against economic consolidation, which made it possible for large firms (IBM) to dominate an industry which they were no longer leading technologically. When codified and institutionalized in official procedures and structures, the very organizational innovations which emerge in structuring communications and facilities to support the work of technological innovation in all its phases--including production--tend to inhibit the process of technological innovation which led to establishing those arrangements in the first place. In the implementation of office automation technologies, it is the thesis of this work that it is not the equipment but the methodologies involved in the studies themselves associated with computerization--their objectives, research design, and methods of data collection and analysis--which have brought about the most significant transformation in office work and the structure of organization management, transformations which, to date, have been largely constraining rather than supportive of organizational learning.

It is the process of developing those organizational "innovations" and procedures for cooperative interaction which is valuable to the development of new technologies, rather than any "appropriate form" of organization which might emerge. These forms are transient at best, changing with the requirements of the processes of inquiry and development. As Dewey pointed out, there is a distinct difference between the quest for certainty as security in a world in which men act upon the events of their experiences, and the quest for absolute certainty, which demands that acting and knowing be separate. This is the issue which recurs in the work of Sombart and Berger and Pullberg and others who focus on the constructedness and reflexivity of social organization. There is a basic distinction to be made in that order which is represented in some system (whether a system of knowledge or a system of social organization) and the orderly patterns which are exhibited in the actions of people going about the construction of such systems. In the former case we can see "an organization" (and the organization of information) as a formal or abstract system, an instrument or tool for accomplishing some purpose, which stands apart in an existential sense from its creators and from the ongoing process out of which it was created. In the latter case, the organization of information can be seen to be embodied in the activities of human beings in adaptation to their environments, and any given form reflects one out of a number of possible forms that could be (or could have been) instantiated at any given time or place.

In keeping with the reality-constructionist position on organization, the activity of modelling takes precedence over the most elegant model. We have suggested that with the advent of science-in-war in the course of World War II the focus on practical affairs was (once again) elevated to the realm of respectable inquiry, partly by developing in cybernetics a fully formalized mathematical theory of change based on the notion of feedback in control and communication, and partly as a consequence of the demonstrable successes of systems engineering and operations research methodologies in ongoing problem-solving situations. In this historical context, the building of computers was an undertaking which brought us full circle from the construction of "toy automata" to the wedding of purely theoretical understandings, embodied in mathematical theories--and practical endeavors--the most dramatic and visible of which was the emergence of computer technology and its application to wartime exigencies. The emergence and formalization of systems engineering methodologies in wartime show evidence at the collective level of the vitality of the systems analytic paradigm for inquiry into practical problem-solving contexts.

What is overlooked, however, especially in the education of computer-systems-analysts--is the paradoxical situation in which the products of that organizing may come to constrain the process of system design-and in particular deny to individuals in that process this vital and intellectual action. That vital "instrumentalism" (to use Sombart's terms) is the social process of definition, the act of defining and understanding the world around us as a basis for action. The activity of organization, or collective problem-solving and the resolving of uncertainty, is the basis for adaptive--intelligent--survival and self-regulation in a changing environment.

If we accept the notion of the constructedness of social-technical organization, we must recognize as well that, in Berger and Pullberg's terms, it is not the constructedness--or objectivation--of reality which produces alienation, and conflicts and tensions in the social fabric (conflicts which Benson, Cyert and others argue are virtually inevitable). Rather it is the forgetting which enslaves people to the objects of their creation--the failure to recognize or to remember that it is people who have made, and who continually remake, these social realities, and that there is nothing inevitable nor mysterious in this process.

In this perspective, the contempt for practical work is a major underlying contradiction in the systems engineering, particularly in the designer-based mode or style of development. This separation of knowing for acting--or working!--is the mechanism by which the mental and "manual" aspects of work become increasingly remote. Ultimately, from this orientation follows the deskilling of all work, breaking it down (decomposing it) so finely that the composition of skills involved in using some designed system becomes insurmountably problematic and fraught with problems of coordination. Thus, while the dominant "Newtonian" perspective has demonstrated its efficiency of prediction in many cases, Thom argues that the "human mind" is not likely to be satisfied with a vision of the universe which is governed by coherent but totally abstract mathematical processes. Not only are those processes likely to be indeterminate when confronted with the ambiguity and complexity of real-world situations, but the process of intellectualizing and interpreting those situations is itself fundamental to the human condition.

> In the situation where man is deprived of all possibility of intellectualization, that is, of interpreting geometrically a given process, either he will seek to create, despite everything, through suitable interpretations, an intuitive justification of the process, or he will sink into resigned incomprehension which habit will change to indifference. (Thom, 1975, p. 5)

A fundamental outcome of breaking the feedback relation between systems and their environments--by failing to reflect on the activity of systems in their environments, for example--is a progressive maladaptiveness of that system-in-use, which is perceived as systematic occurrences or increasing levels of error and uncertainty in operations. The notion of the "systematics of error" implies that there are types and sources of error and uncertainty which do not occur randomly and thus cannot be explained (away) through statistical measures of uncertainty. To recognize errors as systematic is to notice (as does Wessel, 1979) that certain types of problems recur in certain types of environment, or in the context of certain developmental stages or processes.

What we are describing is a form of systems engineering in which the processes of system development may come to create systematic--or recurring--errors in certain contexts (such as those associated with remote design and/or autocratic styles of decision-making and supervision in the course of implementation). Over time, these restrictive processes of systems engineering may create systemic--or progressively increasing recurrences--of uncertainty and error in the process of system development and use per se. In either case, the outcome for the organization is not development, but degeneration. Degenerate systems are those in which the very processes of inquiry and organization create conditions which reduce the variety and informational capability in a system, a situation associated with entropy and positive feedback as dynamic processes in ongoing systems. Thus not only does the concept of organization not imply the existence of permanent organizational entities; the process of organization does not necessarily create order and stability, and under certain circumstances, if implementation is done badly enough, it can destroy order--and with it the very contextual requirements or conditions necessary to create it.

A certain narrowness of design characteristic of the designer-based style of systems development creates along with a set of information products, a reification of the development process itself as a "recipe" for inquiry, one which essentially overlooks the context and process of development. This narrowness derives from an exclusively normative focus of attention in design to a restricted range of information, corresponding to the stipulations in some designed--or abstract--system. It is this narrowness of inquiry in the designer-based mode of system development which can be progressively destabilizing of the forms of organization created by the process. The transference of the broad programme of research and development from operations research to the (re-)design first of commercial production processes, and subsequently of clerical work, has taken place in postwar contexts throughout this century, in each case changing conditions to such an extent that the same formula for success could not be repeated without adverse consequences both for the process and for the individuals in it.

We are now in a position to explain the occurrence of health hazards associated with newly designed production processes. Such problems are not uncommon in the history of industrialization, and can be explained with reference to the broader sense of uncertainty associated with the systemic production of error in the relation of particular kinds of models and approaches to modelling with different environments of use. Thus we will argue that certain--definable--structural presumptions or order at the organizational level prevent reconfiguration of social-technical arrangements necessary for the introduction of new technologies in ongoing systems, and over the long run for the support of innovation and strategic decision-making as ongoing organizational processes, amounting to continuous organizational learning. In this view it is possible to argue that a wide range of problems can stem from a narrow analytic approach to inquiry--embodied in design and implementation programmes which are at base non-adaptive, closed system methodologies amounting to what Boothroyd (1978) calls "precise puzzle problem-solving." In this constricted approach to design and implementation organizational control is opposed to individual control over experience--in spite of the fact that organization emerges out of the collective activity of organizing--in such a way as to result in hard to the individual and to undermine the conditions for organizational adaptation--or learning--as well.

Chapter IV

Uncertainty

In the preceding chapters we have established logical, historical, and sociological grounds for arguing that, as expressed in models, organizational forms arise out of human activities of understanding, choice, action, and interaction--activities which we associate with human rationality and social organization as fundamental to the adaptation of the human species in its widely-varying environments. Models are "economizing devices" (Buffa, 1978, p. 7) which are useful for reducing the variety, or number of possibilities in problem situations, as a guide to rationalizing and coordinating action. The development of models literally symbolizes the process of system definition as the basis for human adaptation, and the development of models at each level of resolution is what we recognize as the essentially social process of organizing. It is at this level that the organization in action and artefact, and the organization among our ideas are connected by instantiation--which is an abstractive process, having the characteristics both of inductive and deductive reasoning and of social action.

The foregoing provides the grounds for a generative theory of organization, which would also provide the basis for explaining the systematic occurrence of unpredicted and undesirable outcomes or side-effects accompanying the implementation of designed systems in ongoing environments--among them implementation failure and ergonomic problems in the use of computer-based technologies in offices. We will argue that these problems can be attributed to limitations in the contexts of inquiry and development, limitations which restrict the adaptiveness of humans in those environments, and hence the ability of such socio-technical systems to be self-regulating. In office automation, as in earlier phases of technologically-induced change, those restrictions arise partly out of competition for advantages in the redefinition of occupations and the restructuring of organizations which accompany technological innovation and implementation.

In this chapter we will argue that certain paradigms of research and development--particularly the kinds of commercial "how-to" formats for undertaking design and implementation in the engineering of office automation technologies--may often restrict the flow of information necessary to successful implementation, while at the same time limiting opportunities for individual mobility within organizations. On this basis it is possible to explain why it is not the "technology" per se but the methodologies involved in research and design and implementation of office automation technologies which have brought about the most significant transformations in the nature of office work and the structure of organizations. These changes are only recently being facilitated by the introduction of computers in offices. A review of methods engineering concepts and techniques developed throughout the past century of American military and industrial development reveals a consistent--and progressively narrowing--programme for rationalizing office work--and indeed for rationalizing all information-processing activities, including research and development itself--awaiting only the processing power to carry it out.

A Generative Theory of Organization

The view of organization as a process, and of organizations as provisionally reified constructs, implies a generative theory of organization as a social process, which is based on the notion of organization as represented in models, and organizational change as reflecting paradigmatic shifts in the articulation and instantiation of those models. We accept the rationale of the "process view" of organization, as outlined by Wiener, Waddington, and others, and agree with Burns that what we are studying in organization theory is not types of organizations, but types of models of organization, models which may be instantiated in different environments (i.e., under varying conditions or contexts of application, which will alter the outcomes of those models as they are implemented and operationalized.) Such a theory includes the following assumptions:

1) Organizations "exist" insofar as they are recognized by people. Both the existence of organizations and their characteristics are attributed to them as a function of people's observations and understandings, and those understandings are relative to the context--or experiences--of those observers in different places and times. As represented in models, organizations are constructs, provisionally reified through a sequence of definition and instantiation, a sequence which illustrates the social process by which reality is "constructed" and transformed, consistent with the arguments of the sociologists of knowledge and philosophical pragmatists. The key to this provisional reification of social reality--as expressed

in models--is the notion of Definition, a central issue in modern empiricism, connecting our experience of the external world around us with the understandings which we develop and act upon with reference to that world. "Definition" is thus the irreducible social element which underlies the organization we recognize both in the actions and in the artefacts that we attribute to human construction. The social process of definition also represents the fundamental reflexivity characteristic of human adaptation. The constructs built by people on the basis of such definitions and definition processes are embodied both in machines and in symbolic representations--expressions in a language--which have the power to order as well as to describe "reality", in the sense that representation orders reality.

2) The constructedness of organizations arises out of a process of instantiating people's understandings of systems in social and technical artefacts in particular contexts in certain periods of time. People not only recognize the existence of organizations, but they also create organizations by devising plans, building things, inventing and carrying out various procedures, and by dividing and coordinating their efforts according to their understandings of purpose and process. Definition then refers to a process by which meaning is generated or produced in the agreements which arise between persons in interaction as to the significance of the expressions of the individual experiences of each. In the process of defining--or articulating--people's experiences, the emergence of a

common language renders the private and isolated understandings of that experience public, and therefore is a vehicle for communicating and cooperating in problem-solving endeavors.

3) Once those ideas are instantiated, they take on an independent--i.w., material--existence as expressed and embodied in a variety of forms, from technical (and administrative) blueprints to speeches and other forms of discourse. These models, once embodied, represent an underlying organizational context (represented in various forms of understanding--i.e., methods and theories) into which new technologies are introduced, and by which development and change are organized and controlled. As the grounds for ordering processes of implementation, it is necessary to be able to characterize contextual differences--both in form and in process--as contingencies upon which outcomes of change will depend. Structure in an organized context is embodied in some constraint(s) on a set of possibilities which, taken together, define that context.

4) Models are represented in language(s), and models are created and transformed through the use of language(s). Language is the key to the identification of form, as well as the generation and transformation of organized systems. As expressions in a language, models--or systems of knowledge--can be independently characterized apart from any specific context or stream of action; conversely, context can be described in terms of these models as they are so articulated. Articulation of underlying models as a context for action enables us to analyze that context on the basis of subjective understandings, without implying the correctness (or falsity) of those understandings.

Models are definitions of order, making up the tacit knowledge understood by individual actors, and embodied in varying objective (i.e., instantial) forms via articulation and codification. These models or definitions emerge and are shared through a process of defining (modelling) and communicating these understandings among a group of people in some environment over some period of time. They are defined and bounded by a set of assumptions and presumptions which represent limitations on the valid scope of application--which we can think of as possible instantiations, or possible worlds in a logical sense. As such, models are neither descriptive nor prescriptive in themselves; all valid models can be interpreted and/or instantiated in some medium or another. The distinction betw-en assumptions and presumptions is based on the degree of awareness which people have of the conditions defining their sense of reality; i.e., on the degree to which those contingencies are explicitly articulated, and the extent to which knowledge of those contingencies is incorporated into processes of analysis, design, and implementation.

5) Change in organizations can be understood, then, in the same terms in which Kuhn conceives of paradigmatic change in scientific theories. Organization theorists such as Imershein, Benson, and Hall discuss organizational-structural change in terms of paradigm shifts; however, their discussions are based on straight analogical reasoning without inquiring into the process by which those understandings come to be embodied in concrete form. We will argue that organizational-structural (morphogenetic) change can be understood as a function of paradigmatic change in the literal sense, to reflect a process of adap-

tation through the construction of knowledge and social structure which is fundamental to human adaptation.

Thus we can define structural change in organizations as taking place through transformations in the constraints and contingencies by which organization structure and process are defined. Change in organization structure and process can then be analyzed as a function of various transformations in the paradigms or models by which we understand and order these organizational contexts. Transformation of organized contexts through a paradigmatic shift in our understanding of organization in these different contexts is what we define as the process of organizational learning, which is a social process of adaptation to the environment, taking place at the individual and collective level. Organizational learning, like individual learning, takes place through a problem-solving sequence having the internal logic of what Peirce called "abduction", which is a process of inquiry making us of deductive and inductive reasoning in iterative stages. (Buchler, 1955, p. 154) The perception of constraints or uncertainties is reflected in the definition of problems, which become the focus for investigation--investigation which is the basis for changes in our models and plans of action.

6) Organizational problems have traditionally been understood as problems of control and compliance, which are associated with measures of effectiveness and efficiency, as first suggested by Chester Barnard (1929). These concepts imply the reliability of organizational processes as defined, as well as their acceptability (implied in performance) to those carrying out the process. The factors of reliability and acceptability

are reflected in the outcomes of those organizational processes (operations) in terms of the cost-effectiveness of the processes or the productivity of the workforce--as factors influencing return on investment. Major problems in organized and designed systems include system failure, production of unanticipated (and undesired) side-effects, delays, and excessive costs of production and implementation--all of which undermine that productivity and cost-effectiveness. The objective of applying various scientific methodologies in organization design and management lies in overcoming these problems through planning and control strategies. Given a view of organizations as provisional constructs, it is evident that the perception of problems, by which we define situations as uncertain or complex, is a function of the underlying models which people recognize and instantiate in technological innovation and organizational development.

Uncertainty and error are a function of the parameters or limitations defining the models used to explain and order some domain of action, consistent with the constructivist views of organization. Models are plans of action, at one level implying direct action on the environment in solving problems, and at a higher level of abstraction implying action on the environment indirectly through observation and analysis consistent with some model of inquiry which specifies valid conditions and methods of observation. In this process, people perceive situations as problematic and act in ways which they believe will alter those situations. Their actions, and the outcomes of those actions as they see them, may change their view of the situation, as well as produce new outcomes. In addition, the recognition of problems may itself generate changes in their understandings and result in transformations of their models (thus altering the assumptions of constraint and purpose making up their view of reality.) These changes in the underlying models of the world may then be reflected in new acts and artefacts which are the outcomes of instantiating these new understandings. In both cases the context in which problems are identified and solved is transformed.

The transformation of exchange relations to hierarchical structures in the generation of organizations is associated outcomes which limit further organizational problem-solving activities: 1) A decrease in acceptability and an increase in social unrest may be associated with the implementation of change where contexts are transformed by institutionalization; this decrease in acceptability at the individual level and increases in social conflict at the group level both can lead to <u>operational errors</u> in performance. 2) The transformation of exchange is also associated with an increase in <u>strategic errors</u> at the organizational level, because codifying and institutionalizing a static and bounded model of a system in its environment increases the number of uncertainties or contingencies representing factors outside the paradigm, including especially those involved in processes of implementation. Problems in the reliability of systems in operation are thus associated with the scope--or narrowness--of models by which organizations adapt to their functional environment. Systematically increasing operational and strategic errors are the hallmark of degenerate systems development.

On the basis of such a generative theory of organization, what we have been calling "organizational style" can be identified with recognized forms or conventions in understanding, embodied in expressions, actions, and artefacts--forms which can be identified both in the structure of information in various systems of knowledge and in the structuring of information which constitutes organization in information and social structure. Context can be identified with particular styles embodied in organizing processes as well as in existing organizational structures; and organizational-structural change can be thought of as a transformation of context reflected in and brought about by paradigmatic shifts in these underlying systems of knowledge.

As the immediate context for the development and implementation of new technologies, corporate structure can be thought of as an underlying technology comprised of a set of technical (machine-based) and administrative algorithms or procedures which, taken together, represent the throughput--or mechanism--for producing corporate outcomes. This underlying technology is defined in systems analytic terms by a set of assumptions which constitute the parameters of a system, Z, defined by the specification of input functions, transfer functions, state transition functions, and criterion values for some time period $T_{1...n}$.

Viewed from the perspective of the models themselves, the process of implementation has the status of an instantiation which establishes a mutual relationship between some technological design and a system of social action, as the process through which representation comes to order reality. Thus in the process of organization, or adaptation, order or structure is a function of the nature of perception and recognition relative to the possibilities which can be identified and reflexive with respect to choices which can be actualized. The characterization of a system resulting from the transformation of an organization by the introduction of some technological innovation is thus a function of the specification for the new system which is stipulated in formal systems design, together with a specification of the old system of organization as a context into which the design is introduced, and a specification of the methods by which the designed system is to be introduced into that context.

The resultant system Z is therefore an outcome of the composition of features representing the technology and those representing the existing organization. Technology is represented by the resultant system produced as an outcome of some design process, such as Wymore's tricotyledon system design system; organization refers to the recognized structure in an existing system as the context for the implementation of change. The structure of this organizational context is then represented in a wide range of symbolic artefacts, including verbal and written expressions of organizational actors, forms and records and historical accounts and theories learned by organization members as part of their professional training, and a collection of all the routine programs and regular actions characterizing the operation of an ongoing system. The outcome of this process of organization at any given point in time is a socio-technical system.

One way of thinking of these assumptions as representing the underlying knowledge system of some organization taken as a context for development and implementation, is that they indicate the boundaries of a given system of knowledge, and thus of the understandings of that organized system and its outputs which are built upon the basis of that knowledge. Assumptions thus represent limitations on the possible instantiations of a model. What that means--consistent with Cole's substantial cause theory--is that the model (representing the designed system) holds, and therefore can be applied to, just those situations in which the assumptions stipulated in the model are met. Those assumptions thus represent constraints or limitations on the instantiation of some designed system.

Understanding this underlying context takes place (for actors and observers alike) at two levels of abstraction. We may speak of organization structure as 1) represented in a set of explicit assumptions, which are those definitions of the situation which are articulated or expressed by actors and stipulated in system design artefacts (to use Wymore's terminology); and 2) reflected in a set of presumptions, which represent the level of implicit knowledge, not generally expressed, but which is exhibited in habitual--i.e., patterned, but not necessarily self-conscious, behavior.

Organizational capability for successful implementation of change depends on the effectiveness with which formal and informal models of organization and technology (represented in these assumptions and presumptions) are able to predict outcomes of systems constructed on the basis of their specifications. This capability cannot be simply read off the specifications of the capacities of the technology, and implication which characterizes much of the popular literature on computerization. We have agreed with Sombart and others that the introduction or transfer of new technologies into ongoing organizations involves more than just the installation of hardware. The operation of the equipment implies a specific application, involving new methods of production, and new operating procedures and skills. At a more fundamental level, the process of implementation, as we have seen, implicates underlying theories of organization amounting to methodologies which organize the knowledge and working arrangements by which the system undergoing change is understood by its members. On the basis of this caveat, we can distinguish technological capacities from organizational capabilities as follows: 1) Measures of technological capacity describe a relation between a set of features and limitations characterizing the equipment and a set of categories and rules describing the application. (As we have seen, however, multi-purpose technologies, notably computers, can support a wide range of applications given the same features.) 2) Measures of organization capability, therefore, reflect a relationship between users and systems which is entailed in the implicit structure of information representing the organization in designed (i.e., technical) processes, and the social organization of communication in reporting relationships, technical interdependencies, and interpersonal interactions among persons performing a variety of tasks at a number of different locations. The lexical organization of the information-in-use reflects a "knowledge system" which characterizes a given organizational environment and is embodied in the language(s) used to specify structure and process in ongoing systems. Where that knowledge base is explicitly represented in a set of files and their ordered interrelationships, we may speak of that--implicit--knowledge base as a "data base".

The successful application of formal methods of analysis and design to the process of organzing, and with it the implicit process of negotiating some mutually constructed reality in some environment, requires that this underlying "knowledge base" or contextual framework can somehow be articulated as a common ground from which participants to the change situation can communicate in order to facilitate this bargaining process. This framework is variously expressed in conventional organizational paradigms which define organized systems in terms of a set of constraints--or constraining assumptions. These constraints consist of a set of technical and administrative arrangements which, from the perspective of the negotiation of any specific issue at some point in time, must be taken as given. In a very real sense, just because they are taken for granted, these supporting arrangements constrain the outcomes of the organizing process by determining, a priori as it were, the issues to be negotiated, the parties to the negotiation process, and the rules or restrictions which are to guide that process to some outcome, including not least the recognition that the negotiation process has "ended" and that some decision or event is therefore to be expected.

In order to assess organizational capabilities, and to articulate this underlying technology or context, we can understand the outcomes of implementation processes as a conjunction of two sets of mutually constraining assumptions, one stipulated in the "designed" system, and the other reflected in a set of (perhaps implicit and unstated) assumptions representing the form of the "existing" or entered system. These assumptions constitute built-in systems definitions, amounting to an implicit contextual framework both for the study of and management of organizations. Predicting the outcomes of technologically-induced change, therefore, depends on being able to account for both "existing" and "projected" definitions of some organized system, as well as for the influence of the process of negotiating the transformation (or re-definition) of that ongoing organization. The interdependencies between designed systems and the contexts in which they are implemented can then be predicted from an examination of the implicit negotiation of this set of mutual constraints characterizing the designed system--as a system for the organization--and the existing system--as the system of organization embedded in a wide range of instantiations in ongoing systems (including oral accounts, written records, methods and procedures accompanying hardware processes, and descriptions and instructions accompanying equipment and constituting training courses.)

We will argue that predictiveness of various models of organization in different problem-solving contexts depends upon a number of contingencies having to do with the logical and energetic requirements for the operation of self-regulating systems. Limitations on organizational capability--or the predictiveness of organizational paradigms-in-use--are thus reflected in the parameters of a given system design (which we can think of as relative contingencies with respect to some environment or context of application), and constraints in the definition process itself (which we can think of as reflexive contingencies having to do with the nature of inquiry and of action in negotiating the definition of some ongoing socio-technical system in some environment.

A Language Format for Implementation:

Characterizing the structure of ongoing organizations, and identifying those constraints upon organizational capabilities for successful implementation of change, can be facilitated by a language-format (as Wymore suggests) which can articulate those constraints, at two levels:

1) Material constraints represent technical requirements, features, and limitations. These aspects have long been studied in the disciplines of management science and human factors engineering. As problems in computer implementation suggest, however, a description of technical and economic considerations is perhaps necessary but certainly not sufficient to account for the ways in which knowledge systems (or cognitive maps) constrain and support organizational processes, including the implementation and use of computer-based systems.

2) Cognitive constraints reflect the organization of information-in-use, which represents the organization to itself in planning and ongoing operations. Cognitive constraints designate features of the model(s)-in-use, and are inseparable from performance at every level. The concept of model identifies the notion of organizational context with a set of definitions or assumptions about the nature of the organization and its environment(s). Since the perception of order represents an inference imputed to observation, there must be some connection between the organization of information represented in the methods which determine performance and those which represent managerial and engineering definitions of control, particularly in the definition of

tasks, and the organization of work, as they are (or can be) articulated in any given environment. The structure of organizations can be identified in terms of a set of cognitive constraints, or assumptions reflecting accepted or socially recognized definitions of order. Because recognition and articulation of the contextual as well as the technical determinants--or constraints--in systems engineering is necessary for successful implementation, there are practical reasons to attempt to characterize these underlying "theories" or systems of knowledge in order to assess an organization's capability for change along several dimenstions representative of the context of action, to include:

1) The scope of the definition of systematicity, the range of concepts and possible instantiations recognized, including the stipulation of conditional factors assumed to constrain the definition to certain parameters for which the model is valid;

2) The mechanism implied in the definition, the structure or internal logic or cohesiveness representing the interconnectedness--or contingency--which can be expressed between and among elements in a defined system; and

3) The strategies or prescriptions for inquiry and action which may follow from or be included in a given paradigm representing a type of action model--or programme. These models presume a set of value criteria and a set of methods for organizing the conditions of inquiry and action, which may in some cases be articulated and in some cases only implicitly understood and reflected un-selfconsciously in the actions of organization members, and in the outcomes of organizational processes.

The inevitable reflexivity of using systems analytic methodologies to confront systems analytic methodologies-in-use is of potential benefit in improving the power of explanation and predictiveness of various designed systems as applied in real-world contexts, and of overcoming the artificial and limiting separations between theory-building and practice. According to pragmatists like Dewey, Mead, and Whitehead, this reflexivity is the basis for the special effectiveness of modern scientific (experimental) methodology. The contribution of modern systems analysis is in making this self-conscious experimental method model-relevant, and indeed the role of the consultant or systems engineer in helping the client to define and solve problems (in Wymore's terms) has been defined by Boothroyd as articulate intervention--based upon what he calls "reflection-before-action". (Boothroyd, 1978, p. 47) To this end, that set of constraining assumptions representing organization structure as a context for development and implementation can be characterized at three levels of abstraction, or logical embeddedness, as follows:

1) Methodological paradigms--at the highest level of abstraction--designate methodological (or epistemological) models which stipulate the nature and limits of valid or accepted knowledge and processes of inquiry and inference, stipulating how information is to be acquired, identified, classified, and manipulated in order to reach conclusions. Assumptions defining these methodological paradigms may take the form of a) axiomatic, a priori stipulations of a set of definitions in a formal language or calculus; b) substantive assertions that order may take one or more identifiable forms; or c) restrictive definitions

which stipulate constraints on permissable organization or information in a given system.

Methodological paradigms are embodied in a set of research methods which define a procedure for the context of discovery, consisting in a set of decision rules for identifying and processing information. We can think of methodological paradigms as knowledge systems which cross conventional disciplinary boundaries and represent the basis for the organization of inquiry within various theoretical disciplines. Three major methodological paradigms have been identified (in Chapter I): Inductive empiricism (vulgar positivism), eneral system theory, and ystems analytic methodology.

2) Theoretical paradigms--corresponding to actors' tacit knowledge or models of organization--refer to definitions of systematicity in formal and social organizations, and are the basis for characterizing the structure of local organizations or firms. These theories or assumptions are acquired by organization members and decision-makers in the course of their education and training within established disciplines and professions, and are embodied in the organization of occupations. Three broad paradigmatic schools of thought can be (and have been) identified in the field of organization theory which can be seen as underlying these theoretical paradigms-in-use: Classical models of management science and administrative science, and theories of bureaucracy; Behavioral models of organization having their foundations in social-psychology and most closely identified with human relations; and Contingency models of organization, focusing on decisions and information, and examplified by socio-technical systems theory.

3) Strategic paradigms--at the most concrete level--imply action, and refer to methods of control embodied in a set of decision rules, instructions, procedures and/or strategies for controlling and directing events and solving problems. Such methods imply the process of organizaton as a rational undertaking which serves to coordinate and direct diverse activities and to maintain compatible definitions of that activity over time through various methods. These methods implicate the nature of the organization of information in any given organization, defined as an active system, in one of three conventional control strategies: The technology of Process control, which emphasizes the logical organization of a firm through the engineering of products as the basis for the definition of tasks and the coordination of communication and production relations; the art of Leadership, emphasizing social interaction and persuasion as the basis for generating and supporting requisite levels of motivation on the part of organizational actors; and control over the production of information in Research and design strategies which represent the dynamic structure of any organization as an inquiring, or learning, system--making use of a multiplicity of methods and equipment useful in organizing the acquisition and transmission of information for decision-making at all levels.

The content of these three paradigms taken together in context can be defined as making up the knowledge base of a given organization (as understood by some actor or observer). If this implicit knowledge is made explicit--if it is articulated--then it can be characterized as a knowledge system; if it is formalized it can be characterized as an abstract system (following Ashby). As a knowledge system, understanding is objectified in a symbolic language which can be analyzed independent of action by making use of a variety of methods, including in addition to automata theory and other variants of cybernetics, methods of discourse analysis, graph theory, and network analysis, as well as comparative exigesis of conventional models of organization represented in the literature taught to organization members.

We can say that theory and method are related in such knowledge systems through the progressive establishment of constraints on a set of identifiable possibilities. These constraints define the limits of organizing capability at each level--in practical terms, they identify that which must be added to any formal system of knowledge in order to make it operational. The way that a problem is conceived requires and precludes certain types of inquiry; conversely, the requirements for certain types of analysis--especially quantification and instrumentation--often preclude investigation in certain problem areas where the data is not amenable to quantification, or where instrumentation has not been devised. There is thus a mutual relationship between the methods used to study organizational phenomena--the categories and rules for making inferences--and the theories of organization which provide the concepts and values on those categories. As methods change, the conceptualization of the system of interest often changes, because new possibilities for inquiry are suggested or precluded. Conversely, the conceptualization of the system constrains the types of methods which are considered appropriate for inquiry and design in different problem areas. The determination of which methods and what concepts are legitimate, by themselves or in combination, is a major issue in education for the professions--with implications for issues involving who is qualified to carry out investigation, by what authority, and under what conditions.

If organizational contexts and structures are not invariant--or permanent--these issues of expertise and structure are never resolved once for all. As Gotlieb and Borodin point out, these issues cannot be "solved" but must be resolved over and again, as situations change. (Gotlieb and Borodin, 1973, p. 2) Not only have methods for investigation changed over the past century, but so also have organizations and organization environments. Our conception of organizations and our conceptions of science-in-industry have also changed, with implications for the design and management of large-scale complex organizations in uncertain environments. The literature of organization theory is the most accessible source of information for the assumptions making up these paradigms, and it is reasonable to expect similarities and differences in understanding to reflect systematic differences in education and experience. There is no reason to expect that the most current theories and methods are the most widely held, however, as actors in any given situation may hold to personal (or tacit) models of organization made up of bits of knowledge from a number of sources, many of them by no means current or even compatible with currently acceptable knowledge. These are likely to have sunk to a sub-articulate level, but unless they are replaced by other ideas and explanations, they are still the only basis for understanding in similar situations.

For this reason the horrified responses of modern social scientists to the persistence of older human relations theories based upon the now-discredited Hawthorne studies and popularized translations of Maslow's social psychology into "entrepreneurial social science" should not be allowed to obscure the fact that this theoretical position has a large following today, especially among consultants specializing in computer implementation strategies. Together with perennial versions of traditional management science, these explanations and strategies for planning and social control in organizations are the models which are important in predicting the outcomes of change.

In predicting the outcomes of implementation in different organizational contexts characterized by such paradigms, we have suggested that certain models block change and certain models support change, and that certain models are incompatible in combination. We can identify and contrast those models, first, on the basis of the degree to which order is presumed, and, second, on the basis of the degree to which order is preserved in planning and carrying out the process of implementation. We will argue that order-presumptive and structure-preserving models both block change and are incompatible with the requirements of systems engineering methodologies for developing and implementing designed systems.

Order-Presumptive and Adaptive Paradigms of Organization:

Organizational paradigms can be characterized on the basis of 1) the degree to which order is presumed in their accounts of organizational form and process, which generally involves the manner in which assumptions defining organized systems are articulated, including with expressions of systematicity, those expressions of the problems for which proposed systems are intended as solutions. Typical problems in the implementation of technological change include issues of process reliability, or organizational productivity, improving the responsiveness of systems to their clientele, improving cost-effectiveness of operations, and the like. 2) We can also characterize organizational paradigms on the basis of the processes or strategies by which organizational arrangements are constructed and transformed, as an explanation of the direction of the processes of change and control in ongoing organizations.

With respect to definition, these organizational paradigms may be characterized as either order-presumptive or order-constructive (adaptive). With respect to action, strategies for control may be characterized as structure-preserving or structure-changing (morphogenetic). Thus the two great organizational paradigms--one based upon formalization and the other based upon feedback in living (i.e., natural) systems--can be seen as augmented by two organizational strategies associated with the preferred responses to change in ongoing systems. By characterizing the models of observers and actors in organizations in this way, we can identify two developmental paths or styles of innovation and implementation--or two characteristic forms of organizational learning--emerging from the progressive composition of specific definitions or assumptions about organizational "realities" as represented in the above three cognitive domains. In office automation these two developmental paths can be identified in the design and implemenation of computer-based technology as process control systems or as learning or inquiry systems.

Most conventional organization theories are order-presumptive; in a logical sense, they account for the composition and coordination (or mechanisms of control) in organizations in only the very simplest sense built upon the notions of functional isomorphy among systems and the equivalence of decomposition and composition as productive of organization structure. This is in keeping with a tradition which accounts for organizational complexities on the basis of a rational reconstruction of abstract systems from data produced by analysis, speculation, and/or experience. The problem is the rational--i.e., abstract or purely formal--(re-)construction of systems, as we have argued in Chapter I. Implementing such order-presumptive models can lead to a systemic and mutually-reinforcing relationship between occurrences of uncertainty, error, and stress in developing new organizational structures in the course of technological innovation and implementation--a problem which we have called the Systematics of Error.

Order-Presumptive models of organization presume that some structure exists or precedes the design and implementation of a given formal system (design), either in a "natural" order discoverable by conventional inductive empiricist methods of scientific inquiry, or in a social order of shared conventions which constrain choices and render individuals' actions predictable. Where order is thus presumed, it is generally left unexamined.

The central problem from this perspective is one of control--control over the process, the personnel, and/or the outcomes of organizational processes in the face of disturbances to the conditions specified in the formal systems and programs designated for such systems. When the control problem is conceived as the optimization of return on investment--particularly through control over labor costs--the technical solution to issues of organization management has been to focus design efforts on providing computing power on a production basis, employing volume and speed of computer processing to achieve greater economies of scale in batch processes in ever-larger enterprises. The overriding objective of such design strategies is that of maintaining or improving the stability and productivity of an organzation as it grows larger and more complex.

Adaptive models of organization, in contrast, consider organization to be a consequence of the responses of organization members to exigencies presented by their environments, responses which taken in the aggregate, and over time, morphogenetically alter the ongoing structure of systems relative to the nature of environmental demands, and reflexively dependent upon available means for meeting them. In an order-constructive model of organization, structure is not presumed nor taken-for-granted, but must be explicitly defined--by a designer, by the members of a system, and/or by a consultant or team of experts specialized in some manner to undertake that task. It does make a difference who is involved in the design and transformation of organization structures--an issue often addressed to the question of who is the "client" or "sponsor" of some organizational change. It also makes a difference in what time period and in what environment this process takes place, as in adaptive models order is conditionally specified for particular purposes in definite environments over specified periods of time.

The central problem in implementing adaptive models of organization is that of providing a network of intercommunication sufficiently rich and flexible to support the relations of interdependency required to articulate complex problems and to identify, select, and integrate alternative courses of action. The view of problem-solving in the adaptive model emphasizes the inherently interactive and creative nature of system development and maintenance as an ongoing process of self-regulation in which organization structure changes over time in response to environmental contingencies. These contingencies include the internal organizational environment of employees, materiel, and operating programs, as well as the external environment associated with a network of clients, suppliers, and regulators of different types. The objective criterion of success is a pragmatic assessment of the adaptability--or goodness of fit--which a given configuration of social, administrative arrangements has for a given set of technical specifications, measured in terms of the effectiveness of the organization as a whole.

Just as each of these paradigms is oriented to a particular conception of the "problem of organization", each is associated with characteristic problems involving the implementation of these strategic assumptions. 1) In order-presumptive models, there is a tendency for organization structure to be unexamined, for it not to be planned, codified, formalized and internally rationalized (as is generally presumed in formal organization theories). Because order-presumptive models presume the universe is already organized, they provide no mechanism for defining order in particular settings, or for recognizing and adapting to changes in that underlying order or in the setting in which it is embedded. Rather, structure is presumed to be identical--or isomorphic--to the specifications in the design of the product, and planning and problem-solving take the form of reactive responses to error--classically, management-by-exception. The result is for the organization of the system to be opaque to its members and for problems in coordination to be resolved by ad hoc solutions, by maintaining slack in the utilization of resources, and by separatedly attending to conflicting issues and objectives in management.

Organizations ordered on an order-presumptive model tend to become increasingly maladaptive in complex environments. Increasing complexity in these environments may be associated with a number of factors, including 1) increasing size, beyond that of the current integrating capacities of the system as structured; 2) diversity in the outputs to which systems are directed; 3) uncertainties having to do with the information needed to regulate the system or to plan for future products and services, and with the actions of other companies and individuals in the environment; 4) dynamic and continuous changes entailed in the operation of the system, and changes taking place in the environment in which a system is operating, including changes both in the markets and in the production technologies around which the organization is structured. Strategic problems in organization management have conventionally been attributed to uncertainties in organizational environments and defined in terms of an ever-increasing information-overload on decision-makers, the results of which are manifest in a progressive unreliability of operations and costliness of control.

2) A second problem associated with the presumption of order in ongoing organizations is that planning and decision-making are assumed to take place on a top-down basis in which the decisions of omniscient and omnipotent executives premise the activities of their subordinates (to use Simon's terminology). The strategy is first to formally define the system--often in terms of the technical or quantitative languages of expert designers of machine systems. This designed system is then presented to its "users" as a complete system--a fait accompli--which they must both learn how to operate and how to adapt to their own tasks.

There is, furthermore, a tendency to isolate this technical core of expertise from the ongoing operations of the organization, and in the instance of computerization to centralize data-processing operations early on, which results in more precise specifications but less flexibility in system development. Systems definition can be undertaken in any number of ways--by representative occupational teams, by teams of management representatives, by designers or teams of external consultants, by data-processing and organization-and-methods teams working as liaisons between headquarters offices, equipment vendors, and users. The isolation of technical work avoids the uncertainty involved in specifying operating requirements under a wide range of ongoing conditions, but it increases the uncertainty in the user organizations, which must continually redefine formal systems to their needs, thus compromising the integrity and accountability in the relationship between normal and formal operations.

The consequences, or problems, resulting from implementing these strategies are manifest in information overloads on executives, who must resort to ever-more complex networks of liaison roles and/or more elaborate management information and control systems. Notwithstanding, information is still summarized and transformed at each level and department through which it passes on the way to decision-makers, resulting in what we might call "strategic errors" in decision-making. At the same time, problems for users result in a spate of input and operational errors, and numerous exceptions, which must be passed of for decision, thus further increasing the information-processing load on decision-makers. Ultimately in turbulent environments (characterized by competitive complexity and diversity of products and services, and by technologies undergoing continuous and often rapid development) hierarchical plans rapidly become outdated in the face of now non-routine operations.

In the instance of computerization in offices, the tendency for order-presumptive organizations to codify an implicit and not-necessarily-rationalized set of procedures, programs, rules of thumb, and ad hoc generalizations into a computerized information system has resulted in real problems involving the maintenance of reliable system performance. These problems are only exaccerbated by the corresponding tendency to add to the complexity and uncertainty by local engineering or programming solutions which extend this implicit definition of the organization in a system by the mere accretion of the results of trial and error experimenting, similar to the way in which tasks and roles emerge. The all-too-frequent result is that the new systems become prohibitively expensive and the period of development extends indefinitely, as parallel systems must be maintained to ensure reliable performance, and as frequent down-time leads to peaks and valleys in the work load, ultimately cutting into maintenance time and worsening the error rate in a vicious cycle of unreliability.

Finally, 3) order-presumptive models of organization are subject to serious problems in the acceptability of given systems of organization as well as changes in those systems, problems of acceptance that become explicit in the juxtaposition of existing and proposed systems in the process of implementing change. In order-presumptive models, objectives focus on the optimization of return, through increases in productivity, while assuming an unchanging structure of authority, expertise, and reward.

Structure-preserving strategies for organizational change are also associated with certain characteristic problems--having direct implications for the acceptability of designed systems--in the implementation of new technologies in ongoing systems. In the first place, change in technology means a change in work roles, and, therefore, in the expertise required in many positions. There is at least a training problem, and in many cases a severe recruitment problem, involved in managing the transition to a new system. Employees, particularly older employees, worry about their competence and find ways to protect their positions and their self-regard. In addition, when stratification of the reward system is extreme or is in conflict, the change situation will present both an opportunity and a threat to different people in the organization.

There is, often for the first time, intensified and overt bargaining for resources, for autonomy, and for input into decision-making.

Anyone in a discretionary position--which includes all those whose expertise and skill serves to resolve uncertainty for others--can define new work roles in such a way as to maintain or enhance their position and prerogatives in the organization. This phenomenon has been noted at two levels of organization in office automation:
1) Clerical employees, where possible--and especially where they are unionized--have been able to restrict employment displacement associated with computerization to attrition, thus deferring potentially negative effects of the technology on employment to a future time period.
2) Managerial employees have, for the most part, used their discretion to define applications of computer technology in terms which will have the least negative impact on their own roles, largely by defining computer-based systems as production-type data-processing operations. Any redundancy in employment is thus located in clerical positions, and by extension, in supervisory positions, retaining and segregating all the non-programmable creative tasks of planning and problem-solving, together with the rewards and prerogatives associated with those responsibilities.

Where the problems for which the computerization of office work is a solution have been identified as the high cost and unreliability of labor, and where the strategies for implementation have been to concentrate the adverse effects of change on clerical and supervisory positions, the responses of members of the system to the implementation of change have included high rates of turnover, absenteeism, and conflict. In some cases interdepartmental and interlevel conflicts have led to a rejection of the proposed system altogether, or to isolated and defensive strategies of implementation which increase the difficulty of coordination. Under these circumstances, if the original manual system has been replaced in the transformation, it may be extremely difficult to rationalize and reorganize the system; thus implementation failure can lead to breakdowns in the effectiveness of organizational processes and ultimately to the failure of the organization to meet its external objectives.

If, however, we follow the assumptions of the adaptive paradigm of organization, the process of implementation can be the catalyst for improving the reliability and acceptability of ongoing organizations. In organizational models based on the presumption of the constructedness of organizations, technological change may occasion a thoroughgoing study of current arrangements and programs in terms of their objectives, problems and information requirements. Where the implementation of a computer-based system is itself preceded by an articulation and redefinition of internal structures of work, and training and compensation, the introduction of new technology can represent a positive opportunity to analyze and reorganize current operations for greater operating efficiency and greater human acceptability--to include often-neglected physiological, motivational, and social contingencies in organization design. The feasibility study, which should be undertaken as a first step in formal systems implementation, has by itself frequently proved to be of significant benefit to the organization over the long run--whether computer technology is subsequently introduced or not. (The benefits are somewhat less significant, however, where that evaluation performed in the feasibility study is restricted to measures of cost and time and productivity without at the same time providing an operational definition of information and coordination requirements.)

In addition, the adaptive or order-constructive paradigm of organization makes it possible to conceive of the definition of an organization (made explicit in periods of implementation) as an iterative process of both top-down and bottom-up specification of the objectives and methods of accomplishing them at every level of the organization. Where the design and implementation processes are made explicit, it is possible to develop networks of communication which are compatible with and which support both the technical requirements of the operating process and the requirements of the process of research and development, which has as one of its objectives the transformation of existing operations. These communications networks depend on mutually compatible languages and shared data bases which are accessible from all parts of the organization, thus increasing the capacity of that organization to undertake adaptive structural change in response to changing environmental conditions.

Such an inquiring--or learning--paradigm is what is characterized in the broad programme of operations research, amounting to a movement of science in industry and war. In addition to the contrast between order-presumptive and order-constructive or adaptive theories, we can see in the examples of Eli Whitney's informal engineering model and the systems engineering paradigm which emerged from World War II operations research, the importance of the scope of the paradigms which underly research and development.

The Broad and the Narrow Model of Systems Engineering:

There are striking similarities, but even more important differences, in the systems engineering practiced by Eli Whitney and that exemplified by the operations research movement during World War II, differences which are significnat in terms of the uncertainties involved in the continuing development of computer technology. Operations research instantiated a model of social-technical organization, as did Eli Whitney's Interchangeable System. Although in each the logic of systems engineering is formally described in terms of "feedback", "control", and "optimization", both the basis for the organization of information and the scope of the models of the system of social organization implied as the context for development differ markedly. These differences have significant implications for the process of implementation, or instantiation of such formal systems in real-world contexts.

Differences in the conventional, informal approaches to engineering and the paradigm of systems engineering which emerged from World War II center on the referents and hence the scope of the formal systems produced, which may be distinguished in terms of a Narrow and a Broad view of systems engineering. In the narrow engineering model, the reference of the system is the hardware--defined as an abstract system--the concrete machine which we associate with the common-sense notion of "technology". The reference of "system" in the broader systems engineering model is a socio-technical system--seen as an abstract technology--within which alternative innovations are developed and implemented by the users of the technology. The process of system design is included in the broad version of systems engineering--and is often formalized as an abstract methodology, or format for analysis, design, and implementation--a format from which specific technologies can be deduced and evaluated. The processes of research and design are thus articulated in a way that permits rationalization of the process of inquiry, which is not characteristic of designer-based engineering models.

In contrast with Eli Whitney's (and Henry Ford's) informal engineering methodologies, the operations research movement produced as one of its outcomes not just the designs for particular technolgoies, but the self-conscious articulation of the process of design and development per se in the formalization of the mathematical theory of systems and the codification of a body of procedures for systems analysis and development in the disciplines of operations research and management science. A major difference in informal engineering and systems engineering methodologies lies in the degree to which this underlying organization for inquiry and development is articulated. In the broad systems engineering model the process of system development is self-conscious and subject to improvement and direction; in informal--or conventional order-presumptive--engineering the process of system development is obscure and the role of the designer is remote from that of the operator and user in the system. Whitney didn't publish a model of his system design system, nor is much known of his collaborators or the manner in which he worked; rather, he patented specific devices produced by his systems engineering work, and the American System came to be a recognized model through using these devices, and through ad hoc imitations in practice. In World War II operations research, the fact that many of the contributors to the movement were already practicing scientists in universities gave them a basis for unifying their work through a recognition of shared mathematical models and methods of analysis, reflected in the development of both general systems theory and cybernetics. The operations researchers also exhibited a keen self-consciousness of their networks of association and interdependency in the processes of design and implementation, and worked to institutionalize and otherwide maintain these flexible forms of organization in postwar contexts. It was not actually until the end of the war, however, that these activities suggested the emergence of a distinctive style of investigation, based on the notion of feedback entailed in the concepts of servomechanism and socio-technical system, and on quantification as the basis for analysis. We have seen that neither the concept of the servomechanism alone, nor the emphasis on quantification are novel elements in systems engineering, both having been central characteristics of informal systems engineering and industrialization for some time.

Operations Research vs. the Operations Research Movement: We have been referring to operations research groups working in a variety of endeavors in the course of World War II in more than one sense, and it is now possible to distinguish two different meanings which might be attributed to the lessons learned from this experience. We have noted that what first struck the participants in these endeavors as significant was the formalization of the cybernetic theory of feedback, which increased the power of mathematical modelling of complex and dynamic systems, and which facilitated a form of investigation which was especially efficient as a method for proceeding from basic research to development to production of new technologies, including both instruments and techniques. Equally impressive (in the accounts of the participants) were the networks of social and technical arrangements which supported and guided those research efforts.

In Chapter II a description of the activities of the operations researchers during World War II reveals a broad programme of science-in-war which reflects organizational learning through inquiry in service of practical objectives. This form of organizational learning is exemplary of the process of provisional reification which produces forms of organization in knowledge systems and social structures. The outcomes of this process include not merely a set of solutions to a number of concrete problems, but also the emergence of a set of procedures and arrangements--an abstract technology in Boothroyd's and Johnson's terms--for conducting scientific inquiry in the context of real-world problem-solving. We have seen that the methodology of the operations researchers was comprised of several elements, variously represented in different research groups in different periods of time: The formalization of the mathematical theory of systems and feedback in cybernetics; the systematization and codification of a set of methods and procedures for data-gathering, descriptive quantification and statistical inference gleaned from records of past operations for purposes of optimizing current systems, which is associated with "operations research" proper, as a set of methods; and an examplary, if informal, model of social interaction and teamwork, defined in terms of networks of social and technical interdependencies, each reflecting specifiable theoretical and organizational contingencies grounded in the methods and objectives of research.

From these changes a methodology of directed research came to be loosely articulated as a programme of systems engineering in general, a methodology which essentially was formalized in the course of military research during World War II. It was the theoretical and social formalization and codification of that set of methods and concepts which provided the basis for defining the constellation of activities of all of these individuals as a movement, and for recognizing in that movement an identifiable system design system (in Wymore's terms) for the organization of inquiry. We are arguing that in addition to a series of products, or solutions to problems of great complexity, the operations research movement was significant for producing a self-conscious or reflexive understanding of the organization of the process of inquiry and development involved in arriving at those solutions and in translating them into actual products, and methods of inquiry and production. Thus did the operations researchers reify (or objectify) a distinctive approach to conducting scientific inquiry in ongoing contexts.

What was learned during the course of World War II operations research was a diverse set of methods for defining and solving problems, and for organizing, budgeting, supporting, and directing the processes of inquiry--a set of methods which can be seen, broadly, as a methodology for the conduct of research in complex, real-world environments. In the examples of problems involved in developing radar and rockets, in accounting for human factors limitations and capabilities in operating new systems, and in the development of computing devices operations research was unique in devising formalized solutions to problems which were too complex to permit of solutions derived incrementally from conventional methods. Both the theoretical (or model-relevant) formalization exemplified in the mathematical theory of feedback (or cybernetics) and the procedural formalization of operations research methods of statistical analysis and optimization necessitated a social organization for inquiry which was characterized by the use of multi-disciplinary teams and coordination through networks of technical and administrative interdependencies. Thus the methodology of the operations researchers, broadly conceived, includes not just a set of methods and instruments for data collection, analysis and design in the technical development of machine systems; it includes as well administrative systems and methods of contracting and budgeting, reporting and coordinating the construction of complex, mission-oriented, and temporary or one-time projects.

Let us consider the whole range of methods for organizing information and activities of these researchers as comprising a "methodology", using the term in Kaplan's sense to refer not just to the various methods for acquiring data and compiling it in different forms, but also to the rationale or paradigm underlying these activities, a paradigm which is reflected in the definition of key concepts and relations comprising the hypothetical--or provisional--structure of these constructed systems. (Kaplan, 19 , p.) Technology, whether in the form of new products or new production processes, is an outcome of such a methodology or system of inquiry. In this view, operations research, as a set of methods, is an artefact of the operations research movement, which reflects the broader social and cultural context in which those methods are generated and applied.

Given this definition, we can consider methodology in two different sense, corresponding to the distinction between organization as a form and organization as a process:

1) A methodology can be a program representing the methods and assumptions characterizing the process of inquiry in a format or set of procedures and directives, which serves as a guide to action and as a framework for explanation and prediction. In this sense, methodologies include theories or sets of hypotheses and assumptions, along with a set of methods (or norms) defining the manner in which information is to be acquired, manipulated, and applied to practical objectives. Methodology in this sense amounts to a design for inquiry, a set of instructions for the design and conduct of research, which can be abstracted from any particular context, and transferred to another.

As codified in research reports and conference proceedings, and in contractual arrangements between various cooperating agencies, and as embedded in new technologies and in new methods and instruments of inquiry, the program or plan of research can be seen as an artefact (or product) of a broader programme for research. The operations research movement, exemplifying a broad programme for inquiry, produced two such programmatic artefacts, one theoretical and one procedural.

a) Theoretical formalization involved the application of known concepts and fundamental principles from mathematics and the basic sciences to practical design problems. In certain cases, especially in the physics of radar and rocketry problems and the physiology of problems operating new aircraft, the complexity and time constraints defining such problems defied solution by incremental approaches based on conventional methods and theories. This forced researchers (most educated in the sciences and mathematics) to return to fundamental principles in the construction of new explanations. The abstract concept of "system" provided a specifiable referent for the wide range of factors which must be taken into account in solving complex real-world problems, and the analytic paradigm of scientific inquiry thus came to be defined in terms of formal concepts of "system" and "feedback", which specified the context of real-world problem-solving in such a way as to render it amenable to formally rigorous methods of analysis. The theoretical object produced from this approach was the cybernetic theory of control and communication, which by defining any system as a mathematical entity--or abstract system--provided a template for modelling complex and dynamic phenomena based on the logic of analysis-by-design.

b) Procedural formalization produced an artefact which we now refer to as "operations research", which encompasses a set of methods for data-collection and statistical analysis in practical problem-solving situations, exemplified in training studies, and making use of straightforward inductive empiricist techniques of descriptive statistical analysis for purposes of optimizing certain criterion values and objectives. Methods of data-collection, recordskeeping, and statistical generalization from a record of ongoing operations reflect Simon's and Ashby's notion of a protocol (or algorithm). This protocol need not represent theoretical or model-based interconnectedness, however, but may simply begin with quantification of existing data in the manner of descriptive accounting for surface characteristics, such as that employed in business administration. And indeed, procedural formalization as the systematization of recordskeeping and statistical analysis is consistent with older theories of management science and automation, a point of similarity which facilitated the transfer of the methods of operations research into postwar applications.

The articulation of either of these methodologies as programs or guides for research constitutes an abstract--or formal--system, which can be instantiated as a recipe for inquiry in other contexts. Through the visible successes of the operations research movement during and after World War II, and later achievements in the aerospace programs and in the computer industry, a "logic of inquiry" based on methods of quantitative modelling became conventionally stabilized and extended into a number of environments in which such techniques had only received rudimentary application up to that time. The expression of the output of an abstract system as a linear function of its input describes a practical problem as a mathematical model, which can be solved for a variety of conditions thatmight be input to it, as a basis for decision-making. The discipline of management science has been defined in terms of just this sort of quantitative modelling, and it is on this basis that operations research--and especially systems analysis associated with the development and use of computers--came to revitalize management science in industry and education to such an extent that the two fields of study could be taken to be identical (isomorphic).

Prior to World War II, the magnitude of the problems encountered in engineering projects in business and military had already strained the information-processing capabilities of conventional research organizations to meet the strict requirements for data and the heavy computational demands of complex systems engineering, and these limitations were one reason for the relative decline of management science models before the introduction of computers. Hence the pressure to increase the capacity for ever-larger computations was a major factor in the development of computers, first in wartime, and spreading rapidly into postwar industries where they were first applied to managing the ever-larger computational load represented in financial recordskeeping in large and complex organizations.

We can identify aspects of theoretical formalization in the development of computers and procedural formalization in the development of computer applications, especially in this early period of technology transfer. However, according to the participants in the operations research movement, and echoed by management scientists such as Buffa, the mixed team approach was one of the most important features of operations research. More than the development of quantitative models and techniques, innovations in the organization for inquiry stimulated the growth of management science by contributing feedback control theory as well as 1) a broader understanding of the process of decision-making, 2) an experimental approach to business system analysis, and 3) the digital computer, which provided a kind of "management laboratory" or vehicle for simulating managerial problem-solving in the context of business problems. (Buffa, 1978, p. 15)

2) The concept of methodology can also refer to a broader set of ongoing activities and relationships which emerge out of the process of

inquiry, including but not limited to those codified programs which represent the conventional elements of scientific inquiry. Methodology in this sense includes the social context in which research is being undertaken, the assumptions and objectives on which the research is predicated, and the social arrangements and processes which take place as people go about the conduct of inquiry (including the development of methods for recruiting, training, and coordinating people who carry out the research and development process itself.) In this sense, the methodology of operations research, as exemplified by the activities of operations researchers in World War II, refers to the whole range of activities which were carried out by the operations researchers.

In addition to the obvious products of their endeavor--in new products and new technologies--new ideas about social organization for research and development emerged and were given recognition in the literature and curricula of business education and in the emerging discipline of computer science, where the role of technician as change agent was especially striking. As we have seen this type of organization is--like the emphasis on quantification--hardly new, at least in American history. Although American production technologies centered on the moving assembly line are more well-known, the form of flexible, project-oriented organization between industry, education, and government which characterized World War II operations research is common in American history, and particularly so in wartime. In reflecting on this form of organization, those engaged in operations research came to institutionalize their work in a) a set of standard practices and/or processes for inquiry, b) in the formation of corporate organizations established to carry out further work, especially in the commercial development of new markets and production processes to accompany the technological innovations produced through wartime research; and c) in a common body of knowledge embodied in new products, and expressed in the popular and professional literature, and in education for the professions.

In this broad sense, "operations research" as the popular name for this loosely articulated set of activities, can be considered the basis for a social movement, defined in terms of a group of people whose activities are unified by a common set of ideas, methods and objectives. Boothroyd (1979) and Johnson, Kast and Rosenzweig (1963) refer to this broader sense of methodology as an underlying technology--or <u>programme</u>--which includes all the elements of research and development, including the development and administration of the research endeavor itself, and the implementation of its products in ongoing environments. The methods of the operations researchers, narrowly conceived as a set of instructions for collecting and analyzing data, constitute a <u>program</u>, which is identified with the term "operations research" in engineering and management science texts. The methodology of operations research, broadly conceived, includes many such programs for inquiry and development of a variety of systems making use of a range of methods for producing things and for organizing the production process. The distinction between the narrow and the broad sense of methodology thus falls on the identification of what is left out of the account when such methods are codified and institutionalized in other contexts.

It is clear from the range of activity which goes into the application of such methods, that the design and implementation of systems involve complex and organized processes which are not represented in the design of technical systems emerging from the use of conventional methods of operations research and inductive empiricism. The codification of operations research which is reflected in the literature of systems engineering, in university management and engineering and curricula, and in published articles and conference proceedings reflects a formalization of a program of research which overlooks the non-routine elements of negotiation and decision-making and social interaction which accompany the development and application of technical systems. We have argued that operations research, broadly conceived as the methodology of an identifiable social movement, involves a social context and process of system definition, the knowledge of which is lost when the methods of inquiry are codified and institutionalized. In this sense, operations research is identical with management science only if we forget the problem-context and processes of social organization for inquiry and development which accompany the conduct of science-in-industry. The methods of analysis-by-design, and the presumptions of the designer-based mode of system development (including those involved in the development and application of computer technology) are thus embedded in a broader set of presumptions, in most cases unarticulated by those engaged in developing and carrying them out.

Abstract Systems vs. Abstract Technologies: What is left out of the account of systems engineering is the social organization for inquiry which is based on the articulation of problems in ongoing contexts. This social organization for inquiry represents both the context in which development and implementation take place, and the outcomes of processes of development taking place in different contexts. Reflecting on the two exemplary periods of systems engineering which we have illustrated in Eli Whitney's American System and in the broad form of the World War II operations research movement suggests fundamental differences in the context of development as well as in the logic of inquiry which defines a particular technological style.

We can make a distinction between the methods of operations research as exemplified in Whitney's narrow, informal approach to systems engineering; and the methodology of the operations researchers, representing a broader approach to systems engineering which takes into account the irreducible social elements in design and implementation of formal systems. The Narrow version of operations research is concerned with the development of formal models--or "abstract systems"--for purposes of constructing or installing some designed system. The Broad version of operations research, which includes the process as well as the form(s) of organization involved in the application of science-in-industry-and-war, can be associated with what has been termed an "abstract technology". Abstract technology refers not just to the formal model itself, but to underlying methodologies (or paradigms) which are involved in designing the process of inquiry as an integral component of systems design and development. The Narrow version of operations research corresponds to what we have been calling the "designer-based" mode or style of systems development; the Broad version corresponds to Wymore's system design system, conceived in terms of the activities and alternatives involved in producing formal systems and material artefacts as outcomes of design processes.

Noting that engineering in the 19th and 20th centuries has been firmly based upon systematic support for research within corporate contexts, Price identifies the concept of "abstract technology" with the invention of the method of invention itself, which Whitehead associated with the Industrial Revolution. An abstract technology--as a method of invention--is the means by which a research program can be carried out within a given industry with the deliberate purpose of inventing new products and techniques. In the post-World War II era, engineering was forced to recognize in this endeavor a further, "more ambitious business", which was that of learning the techniques of "the organized forcing of developmental change". Technicians had become change agents, responsible for defining economic and managerial arrangements which come to be associated with increases in the pace of social and technical change at the societal level. (Price, 1965, p. 33)

The common sense notion of technology, implied in the idea of an abstract system as represented in the design of constructed systems or artefacts, can thus be extended into the notion of an abstract technology, reflected in a description of the pattern(s) exhibited in processes of research and development in different times and different contexts. These patterns of inquiry and production represent the mechanism of organizational and social change. Boothroyd identifies "abstract technology" with a set of methods for analyzing abstract systems, which he defines as systems all of whose elements are mental constructs. (Boothroyd, 1978, pp. 4-5) Formally, an abstract technology defines a precise computable route from imagined choices and consequences to some imagine recommendation for action. Defined in these terms, abstract technology is the basis for "articulate intervention", which is the mission of operations research--and, we might add, the role of the expert as change agent. In this definition, the concept of "abstract technology" can be identified with a methodological model for factoring and composing a total process of design and control in the manufacture of some desired end product. The Operations Research Society defines this endeavor as follows:

> "Operational Research is the application of the methods of science to complex problems arising in the direction and management of large systems of men, machines, materials, and money in industry, business, government and defence. The distinctive approach is to develop a scientific model of the system, incorporating measurements of factors such as chance and risk, with which to predict and compare the outcomes of alternative decisions, strategies or controls. The purpose is to help management determine its policy and actions scientifically." (Boothroyd, 1978, p. 13)

According to Boothroyd, the problem of finding such computable routes was of sufficient complexity and potential value to have become a central concern of mathematicians working in the field. (Boothroyd, 1978, p. 95) Being able to find such a computable route depends on a precise description of a set of actions, and on the precise representation of the rules for inference on which one bases solutions to problems. Once the set of actions and choices is given and the principles of inference established, the problem of finding that computable route is simply a matter of logical elaboration from this precise problem statement. (Boothroyd, 1978, p. 95)

A description of the activities of the operations researchers during World War II reveals the nature of systems engineering in both the broad and the narrow sense, and illustrates the points of continuity as well as the fundamental differences in systems engineering and the type of informal engineering begun in Whitney's day and passed on through trial and error. Thus, according to Boothroyd, the operations research movement produced the foundations for two types of system development: 1) precise symbol problem-solving, in which finding solutions is akin to solving puzzles; and 2) articulate intervention, which he defines as a process of criticial investigation of the actions, theories, and proposals about problems of importance to some sponsor. Both definitions could be seen as functionally equivalent to a definition of management science, either as the application of quantitative models, or as the application of scientific methods of inquiry to the solution of practical business problems. Boothroyd argues that

> "...(U)nder the influence of RAND and other government-funded research organizations in the USA, this precise-symbol problem-solving became separated off from inquiries into larger action programs, and centered in the development of computational support."
> (Boothroyd, 1978, p. 110)

Both forms of organization for inquiry were formalized as outcomes of the wartime operations research effort, codified in the literature of systems engineering and applied mathematics and in the disciplines to which these techniques were first applied, and reproduced in the development of new technologies and production processes. Thus they came to be embedded in the development of computers and computer applications, which implicate such contradictory possibilities for liberation and organizational growth and for restriction and personal harm.

A more programmatic definition of operations research is given by Nagel and Neef as a method for determining the highest or lowest possible possible values for variables defining dynamic and complex real-world systems. Nagel and Neef formally define operations research as

> "...the study of the application of mathematical techniques to the choosing among various alternatives that decision or decisions that will maximize some quantitatively measured goal."
> (Nagel and Neef, 1978, p. 7)

We have argued that the (broad) methodology of operations research includes problem-solving by interdisciplinary teams, a focus on the central concept of "system in the conceptualization of problems, and the application of the methodology of linear programming, inventory analysis, and decision theory to solving those problems. The basis for using all of these methods lies in quantification, which then enables mathematical modelling and analysis of various problems. The central technique in the operations research method, according to Nagel and Neef, is that of linear programming, which is a method for defining a mathematical model representing the elements of a problem situation in terms of a set of variable factors, and using algebraic and geometric procedures to determine maximum and minimum values on those variables, given a set of constraints and objectives. (Nagel and Neef, 1978, p. 5)

The techniques of operations research are only recently being introduced into more traditional academic fields of research in the social sciences, but have been used for many years in industrial psychology, and overlap extensively with classical methods of management science and human factors engineering. The major applications of operations research techniques have to date been implemented in the fields of business administration, industrial engineering and industrial psychology, and economics. We have also seen the continuing development of a range of formal models and methods--loosely, applied mathematics--which are most notably associated with the design and implementation of computers.

As we have seen, the formalization reflected in these operations research methodologies can be based jpon some theoretical model or on some set of procedures or instructions. The procedural aspects of operations research (in the narrow sense) are most closely identified with the quantificational aspects of operations research methods; in this mode, quantification in operations research does overlap with the classical definition of management science as the application of scientific (i.e., statistical or mathematical) methods to optimizing decisions, broadly, and the application of quantitative models and methods to the definition and solution of standard business problems, locally.

Quantification:

Quantification--defined by Weber as rational calculation--and represented in techniques of data collection and statistical and mathematical analysis, was one of the essential features of bureaucratic administration long before the 20th century. As Babbage's work makes clear, data collection and statistical analysis were well-developed in the 17th century--well enough, in fact, to provide the impetus for developing computing devices. The Weinbergs note that accompanying the invention of the "millions" of new machines throughout the 19th century, came the invention of "thousands" of notational systems which inventors devised to understand the workings of their machines. Of these notational schemes, they argue, "...the computer program analogy is the greatest conceptual tool of our time." (Weinberg and Weinberg, 1979, pp. 98-99)

The concept of an underlying notational system--or language--in which systems can be expressed unambiguously is the basis for the notion of "productions" or "production systems"--by which we refer to a rigorous, formalized representation of some process of action designed to produce some end. This representation thus constitutes an algorithm or (set of) instructions for performing that action and producing that outcome. As an early example of abstract systems, production systems are part of a tradition which can be traced to Emil L. Post who in 1936 laid out the concept of a definite process in his Post Productions, which were based on the concept of a "symbol space", together with a set of instructions which direct operations within that symbol space from problem to answer. (Eames, 1973, pp. 125-126) These production systems--defined as formalized if not mathematical entities--underly the development of management science in the design and control of (organizational) production systems.

Management Science

The popularity of linear systems of modelling is due to their ability to project forward a set of results which can be determined precisely from given data. (Weinberg and Weinberg, 1979, p. 152) The model of a finite state automaton represents a set of state variables in a framework which makes it possible to compute the value of those variables at some future time by carrying out a calculating procedure which measures these changes of state at each moment in time. The state transition and output functions, taken together with the definition of inputs and outputs, constitute the formal systems analytic definition of a model of a system. The connection between management science and operations research is based upon their common use of this type of mathematical modelling as the basis for predicting outcomes in the future. In management science, the object of using these predictive models is optimization of the performance of the system by identifying "...a success trajectory through the space of all possible states of the world." (Albus, 1981, p. 281)

In management science, the organization or firm, taken as a whole is represented as a finite state system--or abstract machine. Managerial objectives are defined as "problems" which correspond to the choice of some function to be optimized (maximized or minimized) subject to the constraints in the system. These problems and constraints are represented in a mathematical model which is useful as it forces the problem-solver to identify the assumptions about the important elements of the problem and the "cause-effect relationships that exist" within the system, and which enable one to test those relationships for consistency and to explore the consequences of these assumptions. (Buffa and Dyer, 1978, pp. 4-10) Management problem-solving can, therefore, be defined as a process of feedback operating on a larger system.

In defining management problems, a process is defined as consisting of investments representing fixed costs, and inputs are defined as those factors which produce those variables costs. The process is composed of a set of operations, which are controlled (i.e., goal-oriented) transformations. Aggregations of these operations can be represented as graphs or networks of transformation nodes ($n_1, n_2 \ldots n_n$) and directed arcs that are ordered pairs (n_i, n_j) having a directed path between them. The arcs serve as nodal inputs andoutputs such that the design of the system determines the specific capacities c_{ij} between the nodes of the directed paths, and planning regulates the actual flows, f_{ij}, so that $c_{ij} - f_{ij} > 0$. (Starr, 1971, pp. 18-24)

Management exercises operating control by determining inputs, and thus setting variable costs, and by altering the production process--conceived as a control system--thereby altering the fixed costs of production. The task of management science is to apply scientific knowledge to the solution of a set of problems fitting one or more of the standard business functions. (Eldin, 1981, p. 11)

A production network is a set of facilities represented by a set of network nodes, each of which provides a specific service or transformation. The influence of various factors of marketing, finance, and technology input as variable factors produces highly differentiated network configurations. Categorizing control in terms of optimizing some relationship between fixed and variable costs can be expressed in terms of the conventional input-output model which can be solved mathematically, in order to determine which configuration of the production system will yield the maximum profit. A model is defined which contains all the relevant variations, the dependent variable 'O' representing outcomes and the independent variables 'S' and 'N' representing controllable and non-controllable variables, respectively (or contingencies and constraints, in James Thompson's (1967) terms). Given such a model, a strategy is defined as an operation that can be performed on the controllable and non-controllable variables in order to convert some abstract possibility into a "reality" according to the following relationship:

$$O_{ij} = f(S_i, N_j)$$

A given strategy in a given environment will produce a unique set of outcomes O_{ij} (where i represents the strategy and j the environment). Optimizing the system in terms of some variable of interest, then, defines for the manager a decision problem which involves selecting from two alternative strategies one proposed course of action intended to produce the desired outcomes. (Starr, 1971, pp. 52-55, 66)

Methods in Management Science: The process of doing management science involves the construction of problem-solving models which include a description of management problems and available methods for solving them, as represented in a set of "standard" forms of application, defined on the basis of these general models. Problems are classified by content and form, in a manner paralleling the major elements in the organization, and thus breaking the problem of analysis down into "manageable" segments, corresponding roughly to managerial functions of research and development, marketing, production, personnel, and finance. Within these functional areas, specific problems of inventory, allocation, forecasting, scheduling, routing, sequencing, replacement, competition, and search can be identified and solved by defining them in terms of one of three types of quantitative method: Deterministic mathematical models, deterministic network models (such as PERT and CPM) and stochastic models. (Eldin, 1981, p. 126)

1. Mathematical methods: Mathematical programming techniques are build upon three components in the definition of decision problems: a) choice variables, reflecting choices whose values will be manipulated in the search for a best solution, b) constraints, which are variables which limit the values which can be assigned to the choice variables, and c) an objective function, which is a mathematical expression defining the optimization process, the value of which can be computed once the values of all the variables are given. (Eldin, 1981, p. 141)

Most widely used among a group of mathematical models are a set of linear and dynamic programming models, appropriate to finding solutions for time-dependent problems. Linear programming models solve a set of equations which describe the relative constraints on the capacities of various departments in setting production schedules, in order to maintain maximum utilization of resources without violating the constraints in each sector. These constraints are derived from past records and/or from goal statements, and are described as quantities. The system of equations resulting from describing each of these variable quantities is then solved and values on the variables of interest are projected into the future. Alternatively, values are translated first into tabular form and then into decision or possibility matrices, which can be "solved" by manipulating them according to techniques of matrix algebra, also predicting outcomes by projecting transformations thus obtained forward in time. These matrices and equations can also be represented in graph theoretic notation, which is especially useful where the complexity in the problem situation defies drawing up and solving a mathematical model for specific outcomes. The manipu

The manipulation of decision matrices in linear programming models and graph theoretic representations produces a set of comparable systems according to rules which maintain the isomorphy--or internal structure--of the system through a number of iterations, focusing first on one factor or value and then on another, in order to demonstrate a set of alternatively possible (and equivalent) solutions to the abstract problem in which data is represented, from which to select a preferred configuration.

2. Network methods: Among the most straightforward and widely-used methods of analysis for solving management problems are a group of deterministic--but not necessarily mathematical or numerical--network models which can ultimately be traced to the introduction of the GANTT chart in the early 1900's which, together with other scheduling methods based on graphic representations of the complexities of work-flow relationships, are still in use today.

Network models are based on the logic of precedence--or temporal sequence--and for this reason the quantificational requirements on the data are not as rigid as they are in using mathematical and statistical methods. Measurement, and thus comparison, are conducted on the basis of an analysis of ordering relationships exhibited in the data, in a manner in keeping with the use of non-parametric statistical methods in social science research. These models reemerge in the form of flowcharting for developing computer applications and for scheduling and queuing and routing internal processes in computer systems per se. They also share a similar form with socio-metric methods of characterizing the nature of social relationships as a simple ordinal relationship.

Conventional network techniques continue to be updated. In these methods a project is defined as an ordered series of non-repeating activities, represented as arcs connecting two nodes which stand for events being initiated and completed. GANTT charts ultimately proved unable to specify a sequence of operations unambiguously in terms of a set of clear-cut precedence dependency relations, and were increasingly replaced by CPM (critical path methods) which were more determinant of that sequence. PERT (program evaluation review technique) augments CPM network analysis with statistical estimates of time values associated with each activity in the network (evaluated on the basis of 'optimistic', 'most likely', and 'pessimistic' estimates.) The activities in the project are arranged sequentially according to precedence constraints derived from technological specifications--which set task based interdependencies, in our terms. Then the concept of the critical (or shortest path containing all the required steps) is used to compute the sequence of activities and the expected time of completion for the total project. (Starr, 1971, pp. 184-189; Eldin, 1981, pp. 134-135)

3. Stochastic methods: Among methods of quantitative analysis and prediction used in systems analysis are a group of statistical models, which are appropriate where constraints on the quantifiability or normal distribution of the data prevent one from defining observations as numbers. Deterministic methods can be used with parametric data; i.e., data in which the number of unknown values is known, and therefore the number of equations is also known to yield a unique solution to the problem. Stochastic methods are used where the data is not fully quantifiable, and/or where the unknowns cannot be exhaustively specified.

Among these standard forecasting models--"...predictive models that drive other predictive models..." (Buffa and Dyer, 1981, pp. 116-128) are time series analysis, regression analysis, multi-variate analysis, factor analysis, analysis of variance, measures of standard deviation and mean, and a number of qualitative survey methods (including market research and Delphi survey techniques). These models are used to measure "causal" factors in the environment in order to determine their influence on the variables of interest (product demand, for example) by correlating two or more known time series. These models are, however, merely descriptive of localized populations; they give no clue to the reasons for regularity showing in data collected. As Eldin points out, "...analytic methods by themselves can only be used for the solution of determinate problems." (Eldin, 1981, p. 128) However, statistical methods of analysis can be useful in combination with other methods, and as a basis for identifying isomorphies among the data across a number of different representations.

Implementation Methods for Office Automation: Methodological problems associated with the narrowness of quantification are involved in the implementation of office automation at two levels of analysis: 1) In research on office automation and the impact of change, specifically health hazards investigations and interventions, and 2) in research in offices undergoing automation, taking place as a step in the process of implementing change. In this case, the investigations carried out by outside consultants or researchers are components of implementation research. Management science models used in implementing office automation research methods and results include: methods engineering and work measurement and scheduling, human factors engineering, and artificial intelligence approaches to hardware and software development.

As a basis for understanding the types of macro-organizational transformations likely to accompany the implementation of change in computer-based office systems, we can compare the assumptions and problems which characterize these research methodologies--i.e., abstract technologies off the shelf, in Wymore's terms--to suggest how limitations in the methods may influence the direction of inquiry, and hence the accommodations to change reflected in the structure of the organization. This classification is consistent with the scope of problems as defined currently within management science education, which roughly includes three schools of thought, each with its own problem-focus and methodology: Mathematical and technique oriented problems (described above), human behavior and organization theory oriented problems, and decision oriented problems, where we are no longer so interested in identifying appropriate problems for available techniques, but are now searching for techniques appropriate for non-routine problems, suggesting a focus on the process rather than the object of problem-solving. (Eldin, 1981, p. 274)

1. Work Scheduling: Analytic methods of measuring and ordering production operations fall into three main categories; process analysis, motion studies, and sampling theory. In process analysis current procedures are categorized in flow charts and organization charts. Design charts are used to develop changes and to plan project networks in human-machine interaction. Presentation charts summarize and clarify proposals. In general, a summary of available techniques includes a variety of now-standard methods of planning making use of flowcharts, as updated forms of scheduling tools such as GANTT charts, PERT, and CPM. These techniques are also frequently used in planning for the purchase and installation of computer-based equipment, in order to detail the stages from initial conception to purchase and final operation of new systems.

Motion studies are based on the principle of motion economy, originally attributed to studies done by F. W. Taylor and Frank and Lillian Gilbreth near the turn of the century. The object of motion study is to make the performance of the work easier, and therefore more productive, by efficiently distributing the work over different parts of the body through a sequence of movements involved in the performance of tasks. In office automation studies, this generally means measuring the effort which goes into production of documents in terms of keystrokes, and supporting the work in ergonomically designed man-machine systems to make performance easier and more efficient.

Data obtained from the first two types of measures is supplemented and tested by statistical sampling of work activities making up the production process. Sampling theory based upon conventional techniques of times series analysis is used to analyze work performance and machine use on the basis of direct observation of the process taken at random intervals and classified according to the state of the process at the instant it is observed. Work sampling is used to estimate delays and establish allowances "for investigating the utilization of high-investment assets", and for estimating how working time is (should be) distributed by workers among different activities. All of these methods can be used without interrupting the process. (Eldin, 1981, p. 197)

Studying office jobs with the objective of introducing automation technology makes use of questionnaires and fixed or random sample surveys of the range of tasks performed by secretaries. These feasibility studies are accompanied by analyses of the documents used and produced in these tasks, in order to identify the volume and problems associated with this production process. The outcome of these studies is generally a flow chart of the system, indicating current volume and distribution of work. This information is then the input to analyses aimed at eliminating duplications, reorganizing the work flow, and balancing work loads. (Cecil, 1980, pp. 243-247)

One application especially relevant in office automation results from the combination of work scheduling methods and the use of computers in recordskeeping and scheduling as developed in methods of "short-interval scheduling", in which a controller assigns a planned quantity of work and determines that the assigned quantity is completed within the time limit. This the principle behind the development and use of automatic scheduling and monitoring devices, such as automatic call directors used in telephone inquiry tasks, and automatic line counters and monitors built into word processing equipment to measure productivity. This type of application is a source of controversy, and an examination of the definitions and assumptions of work measurement in management science literature reveals that development of such uses--including applications to clerical work per se--has a long tradition.

According to Buffa and Dyer, the process of scheduling work by the use of deterministic network methods involves a number of interrelated steps characterized by a strong interdependency among the parts of any total job. The objective of analysis is to estimate the minimum time required to complete each step, which requires knowledge of the following parametric values: the precedence relations among the project steps, the minimum time to complete each step, and the minimum direct cost to complete each step. (Buffa and Dyer, 1978, p. 135)

The data necessary to the application of work scheduling methods is provided by methods of work measurement and work simplification to determine what constitutes the standard time to complete a specific task, and thus to determine what constitutes a standard--or fair day's work. (Heyel, 1979, p. 11) The objective is to design methods of work to result in high productivity on the part of the employee with the least possible fatigue. Preliminary analysis involves three steps or methods: Process analysis, represented in process charts and flow charts; Equipment analysis, represented on activity charts which plot the series of operations against a time scale; and Operation analysis, which is literally an analysis of the motions involved in performing a unit task, defined as an operation. (Heyel, 1979, pp. 11-13)

2. Work Measurement: "Work measurement and wage incentives are, along with methods analysis, among the oldest techniques used in industrial management." (Heyel, 1979, p. 13) Although work measurement methods have a long history in the U.S., Heyel notes that they were not always well-accepted and thus were not used to a very great extent before the 1950's. They are rapidly coming into use today, especially in the "paperwork businesses" of insurance and banking, which were early users of computer-based office systems.

The development of work measurement and incentives demonstrably does not derive from the use of computers; rather the influence appears to be in the opposite direction. Time study methods were originated by Frederick Taylor as a method of establishing time standards and piece rates for production work. Taylor's work, begun in 1881 at the Midvale Steel Company, was to establish the principle of standard element time data by a) dividing work into its elements, b) designing work by an impartial and trained observer, and c) attaching the measurement of work to the improvement of work methods and wage incentives in increasing the level of productivity of the individual worker.

For Taylor, the essence of scientific management turned on the question: "How do you induce a man to move 47 tons of pig iron a day when he has been moving 12½?" Taylor's method contained four components: 1) Select the right man, dealing at all times with individuals only. The right man is not identified in terms of his skills, but in terms of his willingness to increase his productivity in response to the inducement of higher wages--the "high priced man". 2) Provide him with a bonus for increasing his productivity by following the techniques designed by management. Taylor is arguing for wage incentives as opposed to seniority or craft as criteria for compensation, which criteria are essentially "fixed" and thus not subject to managerial control. 3) Separate manual from mental aspects of labor down to the unit task. A planning department prepares detailed written instructions on the best way of doing each piece of work, which instructions are based on the science of time-and-motion study. 4) Provide a team of foremen who will act as teachers, "helping and directing" the workmen to perform these operatins.

Time and motion study under Taylor involved first selecting ten or fifteen men who were especially skilled in doing the work, studying the exact series of elementary motions they performed and timing them, and selecting for the fastest movements and eliminating all false and slow movements. The task is then defined in terms of the quickest and the "best" movements and implements. This method implicitly presumes the employment of unskilled workers; by design, the skill of the designer replaces that of the worker. Thus as Taylor expresses it,

> "The science of handling pig iron is so great and amounts to so much that it is impossible for the man who is best suited to this type of work to understand the principles of this science, or even to work in accordance with these principles without the aid of a man better educated than he is."
> (Taylor, 1947, p. 41)

Motion study research is attributed to Frank and Lillian Gilbreth, who developed in 1885 their version of the "bricklaying system" to accompany Taylor's "science of handling pig iron". The Gilbreths purpose in motion study was to eliminate waste motion and to establish "the one best way" to perform the job, and in addition, to reduce fatigue. They spelled out 22 prescriptive principles of motion economy, based on the unit of the "therblig" (Gilbreth, spelled backwards) which was defined as a subdivision common to all manual work. Also on the basis of Taylor's work standards analysis method, the GANTT chart method was developed in the early 1920's to assist in computing times for use in job standards. Gantt contributed to Taylor's work standards movement by devising incentive plans to reward operators and supervisors who performed as set by the plans. Gantt justified measurement thus:

> "...(T)he man's record...is the most complete analysis we can make of the workings of a plant, and the one that will help us most quickly to bring into their proper channels things that have gone haphazard. Such analysis is...far more important than an improved tool steel or a new set of piece rates, for it enables those in authority to see each day how their orders are being carried out."
> (H. L. Gantt, 1913, quoted in Heyel, 1979, p. 13)

There is considerable continuity in this approach, especially in the U. S., and industrial engineering methodologies continue to emphasize 1) work methods design, making use of methods of motion study and methods improvement, and 2) work measurement and "labor cost control" in the interest of greater productivity.

> "From the president of the company to the mail clerk, controlling costs or helping somebody control costs justifies his industrial existence."
> (Birn, 1961, p. 1)

We control costs by measuring the factors that affect them, in an activity defined by Taylor as "management based on measurement plus control". Birn argues that if we define labor as part of the total cost of every raw material input to the firm, one can see that ninety-five percent of the cost of production is labor; therefore, the heart of the cost control program is "control of the cost of human labor". (Birn, 1961, p. 4)

Birn has developed a unit of measurement used in analysis of performance standards and individual wage incentives which he calls the "standard minute". Wage incentives in earlier days referred to piecework rates. With a standard minute of measurement it became possible to separate compensation (which can be negotiated separately) from the issue of what constitutes a fair day's work. Using a stopwatch, time study men establish

> "...the standard amount of time which should be taken to produce a given piece of work, so the work can be expressed in standard units, which then are considered the amount of work which the employee is expected to do, as a minimum, in one minute by the clock."
> (Birn, 1961, p. 101)

When paid on the incentive basis, the employee receives one minute's pay for every standard minute's worth of work he produces.

Birn justifies the application of work measurement and work simplification and time motion study to clerical work on the basis of the steadily increasing ration of costs in indirect or administrative work--overhead--in proportion to direct production costs--as does nearly every book and article written on office automation today. According to Birn, office work measurement programs are undertaken for the following reasons, or objectives:

1) Providing management with methods of evaluating performance, especially in difficult jobs where there is no standard against which to evaluate progress and performance;

2) Scheduling of work, in which planning is more effective with data to identify normal or fluctuating work which creates backlogs;

3) Effective budgeting and cost control, in which work measurement permits the development of cost projections and cost control, which can be evaluated on a priority basis;

4) Reducing work and increasing effectiveness, eliminating duplication and cumbersome and needless opertions. "A reduction in the actual amount of work required almost inevitably results from measurement."

5) Providing motivational incentive to employees, on the observation that "employees produce their best work when they are given a definite task to be performed in a definite way, and are informed of the expected time standard for its completion." (Heyel, 1979, pp. 51-53)

Current methods for measuring office work include: 1) Historical measurements using records of past work completed and times of performance, with time data analysed together with production records for the same period; 2) employee reporting of current input and output, using standard forms to tally percentage of time spent on activities; 3) stop watch time standards are the most well-known methods, which break operational cycles into elements which are groups of basic motion elements with a duration long enough to be timed; 4) and predetermined standard measurements of time, which are based on previously obtained time values for identical units of work. (Heyel, 1979, p. 55)

Time study measurement characteristically involves an impartial observer using a measuring device to see how long it takes to perform an operation. Work measurement was originally defined as determining the amount of time that would be required by a "qualified and well-trained person working at a normal pace to do a specific job." The real weakness in this method comes in identifying the unit of measure and the standard or productiveness of a "normally qualified" employee.

Taylor's original ideas were updated in the 1920s by Lowry, Maynard and Stegemerten (at Westinghouse) who added the concept of rating to measurement in their system, Methods Time Measurement (MTM) which remains the most widely used work measurements system today. Rating adjusts observed times to the observer's concept of what would constitute a "normal" pace, as a basis for developing predetermined times for different operations.

Work measurement rests on direct observation, using a stop watch to time the elements in a job cycle, and adjustments are added through rating to allow for recovery from fatigue (rest), unavoidable delays and provision for body functions. Rating also adjusts the standard to allow for faster and slower workers, and maintain competitiveness with one's own rate of pay. After these adjustments have been made, the value obtained is "the time to perform one cycle of the job and hence to produce one unit of product, expressed in standard minutes per unit for the operation. From this analysis, labor standards are established as part of the data in standard cost accounting. (Heyel, 1979, pp. 47-49)

Birn considers the MTM method to be a significant improvement over methods of control relying on data from past performances, on the argument that what the records show is that which one did do and not what one should have done. In contrast, the standard data defines the rate of work or output for an individual in terms of "...the basic causes that effect his speed of motions"; these are skill (ability) and effort (will to work). Thus MTM is a motion study, not a time study. In discussing the early history of work measurement, Birn points out that clerical work was not subjected to time study for thirty years because of the difficulties in measuring office work. This situation changed with the introduction of predetermined motion times, which made it possible to measure activities economically that previously had been unmeasured or measured poorly.

> "Predetermined motion times freed the industrial engineer from dependence on the stop watch. No longer did someone have to be doing the job in order for him to measure it. Properly trained, he could readily visualize what should be done, and by recording motion by motion this visualization of method (and applying the appropriate predetermined motion times, of course) he could arrive at an exact description of the method and a consistent time for following it."
> (Birn, 1961, p. vi)

Thus MTM makes it possible for an industrial engineer to build Standard Data, by which to measure quickly and easily the formerly unmeasurable. Standard Data is, in a sense, a category representing "prefabricated" work elements, which can be assembled quickly in series to measure almost any human activity economically, and it is much less costly than micro-motion study through films. Birn's own system, Master Clerical Data (MCD) is a variant of MTM, developed by noticing that some 95% of the work could be measured accurately with only 40% of the total data in MTM studies, eliminating many of the literally thousands of possible combinations of the three basic motions in different environments. The author notes that as any tool becomes overly complex with increasing detail, measuring devices must continually be developed which are appropriate to the economics of the problem. Thus the engineer uses the "building block" technique to group larger sets of elements. Since clerical work requires tailoring the measures and methods developed for factor work into larger groups of elements tailored for office work, MCD (master clerical data) formatting will make clerical work accessible to commonly used methods of setting predetermined times, micromotion analysis in motion picture records, and in work sampling, which involves random sampling methods to determine information concerning workers' activities and the utilization of the machines. (Heyel, 1979, p. 50)

3. Work Simplification: There is a close relationship between measurement and simplification. Work simplification involves breaking a problem into simple elements in an "organized commonsense attack upon the way in which the work is done now, with a view of doing it better." (Birn, 1961, p. 71)

The fundamental premise of work simplification is to eliminate any work that fails to add value to the material or to provide essential information. Simplification can be effected by eliminating tasks, combining tasks, changing the sequence and/or simplifying tasks. The key to work simplification is the role of the foreman or supervisor, who encourages employees and elicits motivation to produce a better product at a lower price, assuming that once the employee really sees how a job is doen and comes to a desire to improve the method, "possibilities for improvement will inevitably occur to him". Thus the supervisors "bring the religion" to their own subordinates." (Birn, 1961, pp. 71-73)

To whom can these measurement techniques be applied? The answer is not to the treasurer, purchasing agent, chief accountant, sales manager, or office manager, all of whose jobs involve decision-making, creative thinking, and insufficient standardization to justify applying measurement. On the basis of this argument, secretaries also are exempted from measurement, because

> "...(T)he chief function of the executive secretary is to be on hand. Her value in many cases lies not in what she can do, but what she can remember."
> (Birn, 1961, p. 29)

Another characteristic which is particularly striking in the management science/engineering model of organizational control is the emphasis on compliance as the underlying problem to which design techniques are addressed. As Birn expresses it, the problem being addressed by these methods of control is not the rate of speed at which the employee works, but whether the employee works at all, a problem definition which exhibits remarkable stability over the past century. Thus F. W. Taylor could lament in 1917

> "Underworking, that is, deliberately working slowly so as to avoid doing a full day's work, "soldiering" as it is called in this country, "hanging it out" as it is called in England...constitutes the greatest evil with which the working people of both England and America are now afflicted." (Taylor, 1947, p. 13)

The underlying objective in developing methods and instruments of production control, as exemplified in methods engineering, work measurement and machine pacing of work is to substitute "process" controls for personal controls in inducing a certain level of productivity. A common theme in advertising for computerization which belies this problem-focus on the employee who will not work is expressed in invidious comparisons between secretaries and computers--which do not sit and chat, take coffee breaks, leave their desks, or call in sick.

In technical terms, idleness is defined as complete inactivity of either person or machine. The greatest room for methods improvement occurs in the idle time experienced by the operator while the machine is running, or in the idle time occurring while the machine is stopped waiting for the operator. Rearranging the time element makes it possible to reduce idleness by redefining the sequence of the elements of the task. (Heyel, 1979, p. 36) Like Taylor, Birn argues

> "Office productivity is not increased by means of speeding up of motions. Actually, most office workers work at a fair day's work pace or above when they work."
> (Birn, 1961, p. 35)

Birn's argument is that the speed with which an operator works is basically up to him (or her), affected principally by 1) his ability and 2) his desire to "make these motions". Accordingly, to Birn the conditions under which the motions are made have negligible effect on the relative speeds of motion. The reason that working conditions are unimportant in this system is because it is defined in terms of speed of motions, and not in terms of output, and therefore what is important is not evaluating jobs in terms of times, but "normal conditions".

Speed of motion is a personal measure (in this view) of "mental attitude", holding states of health and physical capabilities and limitations constant. The distinction between ability and desire is manifest in the definition of effort--which is "the will to work". According to Birn, the reason that varying levels of performance are encountered, given the worker's environment, is that "workers have more or less motivation to produce" and production is lower because they do not work as often, not that work more slowly. The updated version of the classical argument for what Weber called "imperative control" in the paperwork factories is clearly articulated by Birn in support of his methods:

> "...(U)nder day work environment, where workers are paid a wage or salary instead of piecework, operators work at an average or better-than-average pace when they work, but they only work about half the time...Now, if by one means or another we carefully measure what our employees <u>should</u> produce in terms of a fair day's work, if we inform the supervisor what is expected of his employees, and if we use these measurements to reward good performances and discipline poor performances, we can expect a tremendous increase in overall performance."
>
> (Birn, 1961, p. 22)

The objective of office work measurement is to increase productivity, not by speeding up production, but by "making it possible for management to obtain a normal productivity 8 hours a day instead of 4." (Birn, 1961, p. 44)

Given this orientation it is somewhat surprising that Birn reports resistance to office work measurement occuring among supervisory employees and not from their subordinates, observing that those who are presumably the most likely to be affected seldom resist the change and often actively endorse it. (Birn, 1961, p. 35) The much more serious problem is that this application of work measurements methods does not necessarily imply increases in productivity--if productivity is a measure of the output of some production process as a function of the total costs of that process. Such methods of cost control as proposed by Birn and others in the management science tradition of methods engineering emphasize measures of performance referring to individuals and not measures of output as a function of the structure of production processes and the design of work as well as level of performance--given such design and structuring of tasks. Thus the focus of attention is directed to controlling labor costs rather than designing more efficient production processes, which is ultimately required for anything but a simple incremental improvement in productivity--defined as output per a given unit of labor.

In the 1980's modelling for word-processing applications still involves standard cost-accounting techniques based on discrete "business entities"--materials, labor overhead, space, utilities, equipment depreciation, and leases. Stultz defines word processing in this context as referring to the use of machines to "store, manipulate, and give out information". (Stultz, 1982, p. 37) He argues that

> "Information can be managed more efficiently through automation. Automation lets fewer people do more, faster, in the same way that automation in a manufacturing operation increases productivity and reduces costs. In fact, office automation should be approached in exactly the same way as manufacturing automation."(Stultz, 1982, p. 216)

These organizational or business entities correspond to sections of the organization, functionally defined. In offices the primary functions are 1) administrative support, 2) customer service, 3) data collection, 4) correspondence, 5) reproduction services, and 6) communication services. Each function designates an "organizational area"--i.e., an environment defined by the task. Those functional entities associated with document production include the following steps, or task stages, which could be automated: originating the document, typing drafts, reviewing documents, incorporating revisions, final review of documents, reproduction, distribution, and storage of document masters. These functional "entities" or tasks can then be represented in flow charts, which permit companies to identify alternative steps and even processes--on this basis "challenging every step", in the tradition of F. W. Taylor and the Gilbreths, making improvements which increase output per hour, for example, or quality of output. (Stultz, 1982, pp. 171-173)

Within these "discrete entities", specific process costs are defined in terms of the following measures of productivity:

1. "net units of output/direct labor hour,
2. keystrokes per hour,
3. minutes per office transaction,
4. supply cost per net unit produced,
5. supply volume consumed per process,
6. scrap value as a percentage of total material cost,
7. equipment depreciation or lease as a percentage of product value,
8. equipment downtime, and
9. number of items processed per square foot of space."

(Stultz, 1982, pp. 166-167)

According to Stultz--as representative of the conventional wisdom in word processing implementations today--these measures can be devised for word processing installations based strictly on the types, costs, and capacities of the equipment, and on performance standards tested on employees or provided with equipment, and therefore referring to that equipment.

Given these measurements, the costs of document preparation are modelled--analyzed--according to a set of indices representing industry standards, which he identifies in the following equivalencies, reproduced here to show the level of specificity in which definitions of office work are being conceived for purposes of justifying purchase and installation of computer equipment:

1.	# hours/written page (including research)	=	3.7 hours
2.	# of minutes/typed draft page	=	12 min
3.	# of minutes/edited draft page	=	8 min
4.	# of minutes/author reviewed page	=	9 min
5.	# of minutes/final page unit composition	=	18.2 min
6.	# of minutes/page unit makeup	=	3.8 min
7.	# of minutes/proofread page	=	5.8 min
8.	# of minutes/final page check and document assembly	=	3.0 min

These indices are then converted to standard units defining equivalent page units for handwriting, 10-12 pitch typewriters, and for single-double spaced text. The indices are then used to measure performance of the organization and persons in it, and to plan work for the future, largely carried out by allocating resources in terms of the requisite transactions for a given period of time. Data is collected on worksheets to provide quantitative evidence of performance for setting compensation, and as a basis for future costing. (Stultz, 1982, pp. 167-169)

These measures analytically separate the generation of a document from its production (re-production would be more correct), which means that the analysis generally overlooks the high (and increasing) costs associated with the "editing" function. Customary measures of productivity, such as "number of sales dollars generated per person", or "payroll as a percentage of sales dollars", or "direct labor hours per page unit" are not readily translatable to the performance of employees other than production workers, Stultz argues, in keeping with Birn. It is customary that cost-accounting for the value of products produced in a manufacturing firm as a whole measures productivity only for production line employees and does not typically include administrative, professional, or even clerical employees. When this type of cost-accounting is extended to clerical employees, it still does not imply a similar accounting for administrative and professional work as a percentage of costs in producing a given level of sales, and thus not only do costs per person and other measures of organizational productivity tend to be misleading--labor as 95% of the cost of each operation, for example!--but the costing of clerical work which builds in but does not measure a potentially endless iterative editing cycle in the generation and production of documents is also likely to be systematically misleading.

Newer methods of analysis and design base office automation strategies on formalized production systems methodologies, such as Zisman's augmented Petri net representation of event-driven office procedures, which is designed not only to monitor but to replace typical secretarial functions with machine processes. Zisman's modelling strategy defines office procedures as collections of asynchronous, concurrent, and event-driven processes making up a production system. Production systems consist in a set of rules, a data base, and a "rule interpreter". These rules are "productions" consisting of a condition and an associated action, a definition which lends itself to Petri net formalisms, as a form of deterministic graph theoretic notation following in the tradition of GANTT and PERT charts, and consistent with state transition network representations in automata theory--such as Wymore's. According to Zisman, state machines and partial order graphs, such as PERT charts, are both restricted forms of Petri nets. (Zisman, 1977, pp. x, 47, 28)

Petri net formalisms have an advantage over other types of modelling technique in that they are able to indicate which productions--or rules--are enabled or in process at any given time in any given task-determined context (recognizing that not all procedures are operative at any given time). Petri net representation makes it possible to identify in the production system when particular rules in a production are relevant; augmentation of Petri net formalisms with production rules at each node tells us when an "enabled" transition should be enacted. (Zisman, 1977, p. 58)

This production system formalism enables the analyst to provide representations of complex office processes for purposes of automating them. Rather than focus on measuring and monitoring the productivity of clerical workers, Zisman develops a specification language called SCOOP which is intended to automate--and thereby replace--the secretarial functions of a journal editor, determining when actions can and should be taken, and "causing" them to be enacted, according to predetermined rules. In contrast to Stultz's more conventional approach to measurement, Zisman's specification language has not, in fact, been implemented and is, therefore, untested, although he argues that it is easy to add production rules and transitions to existing networks without having to modify them. (Zisman, 1977, pp. 182-184)

The achilles heel of these formal approaches to office automation is the implementation of a predetermined formalism. In addition to the narrow empiricism of Stultz's (and others') measures of clerical productivity as a function of the number of keystrokes or minutes per document page--regardless of the nature of the working environment, the content of the document (for example, is it filled with technical symbols or terms, or proper names in a foreign language?) or the length of the editing cycle (which is in principle unspecifiable)--the weakness of this approach in actually rationalizing procedures (much less improving productivity!) reflects the limitations of management science approaches based upon procedural formalization of operations research via simple quantification and cost-accounting. The narrowness, and weakness, of such approaches has significant implications for the reliability of these designed processes to produce their intended outputs in any environment, but especially in working environments which are conflicted because of the application of just this type of work measurement and methods engineering.

Stultz's problem orientation to the human-machine interface in performance measurement reflects the same "attack" on underworking decried by Birn, and before him by Taylor and his disciples, through the "challenge" to every task, thought of as a "target" for the application of these methods of inquiry.

> "...(A) printer that runs at 40 characters/second may be limited by an operator who performs keyboard work at 70 words per minute...The printer will be idle most of the time. In addition, the operator speed of 70 wpm is also greatly overstated. Actual keyboard speed may only be 35 wpm...Operators take breaks, talk on the phone, sometimes return late from lunch, talk with other nearby operators, are absent...take vacations."
> (Stultz, 1982, p. 175)

Zisman completely excludes clerical personnel from consideration in the development of his office production system--which he acknowledges is developed to assume the role of the secretary--in this sense conforming to the stricter definition of "automation" as the replacement of people by machines, as opposed to supporting them with machines. He argues explicitly that his Office Procedure Specification Language is "clearly not a tool intended for use by office clerical personnel." Rather, office systems technologies will become the province of a new category of office analyst, whose duties will include analysis and specification of office procedures and management of automated office systems, in a role analagous to that of the database administrator. (Zisman, 1977, pp. 10, 110)

Limitations of Quantitative Approaches to Design and Implementation:

We have referred to Eli Whitney's unreflective and informal approach to systems engineering as a "designer-based" mode of system development, noting that neither quantification nor systems engineering is a modern invention, and that procedural formalization--as exemplified in the programmatic emphasis of operations research on methods engineering--has a long tradition based upon the identification of machine requirements and costs of operation in equipment design and installation. The designer-based style of systems development is a mode of thought which underlies the foregoing models for defining office systems, and is characterized by the following assumptions regarding the division of labor between humans and machines:

1) The object of automation is to replace human labor, and the justification for purchase and installation of new equipment lies in increasing productivity--in expected savings in labor costs, and in increasing the reliability of the production process. A further benefit to be expected from automation is in freeing humans from drudgery

and from unsafe or hazardous working conditions by specializing certain operations to machines.

2) The human component in such systems is assumed to be an unskilled worker, laboring individually at a single dedicated task, either as an operator or monitor in some machine process. These tasks can then be defined and measured and scheduled according to the characteristics of the equipment and the machine processes, but without having to take into account differences in working conditions or skills in the workforce.

3) The design of equipment incorporates as much of the complexity and requirements for precision as possible in the task, and as much of the interdependence among tasks in machines and machine processes. Ideally, these requirements and interdependencies are transparent to the operators and users of those machines, to whom they are black boxes--the epitome of a "user-friendly" system. The task of management, then, is to solve problems arising from elements not covered by the machine or routine operations.

As we have indicated, this designer-based mode or style of system development is plagued by two characteristic problems or limitations on its ability to improve organizational capabilities by the introduction of scientific methologies and instruments: 1) its reliability, and 2) its acceptability in ongoing, especially uncertain contexts. We will argue that it is just this restrictiveness and exclusiveness--remoteness--of design and implementation of formalized office systems that brings about both the unreliability in and resistance to new technologies, problems which can undermine the success of implementation, creating uncertainties for the organization and stress for individuals in it.

Limitations on the Use of Quantification: Problems in the reliability with which designed processes are capable of producing their intended outcomes are in part a function of the ability of available models to account for the full range of factors making up the problem-environment. In this light, there are a number of constraints operating on the proper use of quantitative models of inquiry which are of special relevance to health hazards and ergonomic research in office automation. The limitations imposed on the research by quantitative models derive from the fact that they are restricted to reasoning about aggregates, which defines strict requirements for the "quantifiability" of the data, and for the size and distribution of the data sample and the length of the time series. Parametric methods, such as those used in management science and operations research programmes, describe aggregate phenomena in terms of a set of parameters, defined as some constant factor or element of the situation whose values characterize one of the variables of interest in the model. (Eldin, 1981, p. 128) Since computational methods involve adding, dividing, and multiplying data representing sample scores, the use of these inferential methods without distorting that data presumes that it is: 1) properly quantified, meaning that the data possess a mean and a variance, and may enter into numerical equations; 2) normally or symmetrically distributed; and 3) separated by standard or equal intervals of time or distance between the variates. (Miller, 1977, p. 195)

Limitations on the applicability of quantificational models then are associated with at least three qualifying assumptions, having to do with the appropriateness of quantification, the quantity of the data, and the computational capabilities and resources necessary to calculate the outcomes. We must first assume that the population represented in the data is made up of individuals who are essentially undifferentiated and interchangeable on all but the variable(s) of interest, and that there are no significant interactions among these individuals, save in the aggregate (which, after all, is a function of the conditions of observation). If they are related on any other dimension, then the inference does not hold, which is why so much effort is expended to achieve statistical control over the data in order to exclude "spurious" interactions.

Furthermore, reasoning from aggregates in the use of quantitative models--particularly time series forecasting and linear programming models--requires large amounts of data, partly so that undifferentiated units can be assumed, and partly because the equations do not apply in the same fashion to small groups as they do to large aggregates. Ultimately, quantitative analysis is restricted to reasoning about aggregates, and specific predictions cannot be made to individual members of the set describing that aggregate. Buffa and Dyer point out that a considerable quantity of historical data is required for valid causal regression analysis, on the order of a 5-year record for each independent variable. Still, they argue, there is no assurance that a match with historical data is any guarantee that the model is a good forecast for the future, or even to present environments to the extent that they do not resemble the past. (Starr, 1971, pp. 66-68) Buffa and Dyer, 1981, pp. 125-129)

According to Thom, a major problem in applied mathematics is the fact that "...every quantitative model first requires a qualitative isolation from reality". (Thom, 1975, p. 322) As the Weinbergs point out, the first important decision in conducting the research is choosing or defining the aggregate categories used to refer to the organized system. This amounts to establishing the boundaries of the system of inquiry on the basis of 1) statistical controls; 2) an orientation to particular problems or goals for the research, as in analysis-by-design; and/or 3) explicit social definition and control through custom and convention. This usually also means, as Starr comments, that the model is limited to known situations, so that there is a known pattern available to describe the relationships among events in the model, and so values can be given for all the terms in the problem. (Starr, 1971, pp. 66-68) Statistical controls applied to experimental and survey research are achieved by matching samples on all of the relevant factors (or variables) of interest, and then ensuring the randomness of the data sources on all other--unknown--variables. In this type of research--which Miller (1977) calls "pseudoexperimental design"--although the observer can control known alternative hypotheses by controlling the conditions of observation, unknown alternatives--i.e., factors not included in a model--cannot be controlled because these variable factors cannot be isolated in real-world problem-solving contexts as they (perhaps) could be in a laboratory. Therefore, the "universe" from which samples are obtained, and to which prediction refers, cannot be defined or bounded, except indirectly, by the use of statistical tests.

A further problem in applied mathematics concerns the proper use of linear differential equations. If the trajectory recorded for the data in a protocol does not fit the assumptions of fixed or standard intervals of difference--which means that we assume that each fixed interval of time represents a fixed interval of change or difference on some variable of interest--then the model may underrepresent fluctuations or differences in the data, which seriously undermines prediction. The use of differential equations assumes, furthermore, that the rate of change in one variable is a function of that of another variable over time, reflected in the composition of the system under study. Linearity means that the concentrations on the variables of interest are proportional to each other--i.e., that the rate of change in one variable depends on the size of the other. If the initial characterizing assumptions are more complicated than this, then the system of equations is no longer linear, and thus exceeds the capabilities of the model. (Weinberg and Weinberg, 1979, p. 38)

There are numerous problems with the application of quantitative models in complex, problem-solving contexts. In organization research, even where the data is properly quantifiable, when collected for a single organization for purposes of planning and control, valid statistical controls usually cannot be achieved, because the data is insufficiently large in volume, it is not totally undifferentiated, and it is--by definition--structured or organized, rather than atomized and intependent. Thus, as we argued in Chapter I, to isolate that data for purposes of analysis is to destroy the very structure which we would illuminate. Buffa and Dyer point out that for long-term forecasting, where many important managerial decisions are made concerning the commitment of resources, locations of markets and facilities, and investments in physical facilities--exactly the sort of decisions encountered in office automation--there tends to be no statistical record of the history of the firm.

> "Rather, what people think, samplings of how they react to market tests, knowledge of consumer behavior, and analogy with similar situations may be the best we can do."
> (Buffa and Dyer, 1978, p. 132)

As we argued in Chapter III, this anecdotal and personalistic "evidence" may, indeed, reflect the structure of organizations as it is understood and responded to by their members; however meaningful this information is, however, it is not quantifiable and will not support the use of rigorous quantificational methods.

Apparently not all interesting phenomena can be modelled within the limits of quantitative models. According to Thom,

> "...(O)n reflection, very few phenomena depend on mathematically simply expressed ("fundamental") laws...(and) even when a system is controlled by explicit laws of evolution, it often happens that its qualitative behavior is still not computable and predictable; as soon as the number of parameters of the system increases, the possibility of a close calculation decreases. (This is) the curse of dimensionality." (Thom, 1975, p. 322)

Bayliss also notes that although under ordinary operating conditions systems may behave as if they were (very nearly) linear, "all practical systems...both living and non-living, are non-linear when driven to their limits." (Bayliss, 1966, p. 8-9) Finally, the Weinbergs, like Wymore, argue that ordinary differential equations are quite limited in their ability to account for the complexity of the empirical world, and that for non-linear, discontinuous, differential equations--that which would be required for even the most trivially complex systems--"...there exists no general analytical method of obtaining even an approximate solution." (Weinberg and Weinberg, 1979, p. 80) This caveat suggests a serious limitation of quantitative modelling associated with the narrowness of models-in-use as a function of these computational limitations which limit the models to a few aggregative variables. It is here that the requirements of quantification could almost be said to actually undermine the systems approach, by requiring a simplification of the analysis which may be suitable to the use of available methods, but which is still insufficient to explain or to resolve the process of interest. An even worse alternative when faced with such problems is not to do analysis at all, relying on incremental problem-solving, or "management-by-exception".

Since the major contributions of applied mathematics to corporate decision-making have focused on the application of linear programming techniques to cost-benefit analysis, simplification and satisficing (in Simon's terms) are common strategies for using such techniques where the data are insufficient. Nagel and Neef allude on the one hand to computational difficulties associated with the use of linear programming models, and suggest on the other hand that these quantitative models can be extended usefully into other problem-contexts, even less amenable to quantitative modelling than cost-benefit analysis. They argue that in order to apply operations research methods in some environments (such as those studied in social science research) it may be necessary to resort to non-precise forms of measurement in which case the conclusions will probably not be valid in the mathematical sense. However, they argue, the insights gained from the exercise still will be of practical value in decision-making. (Nagel and Neef, 1978, p. 68)

These insights may not, however, be of much practical value, and may, in fact, be quite misleading in some contexts. The Weinbergs point out that a major limitation in these formalized production systems is a fundamental parochialism in typological or formal thinking in general, which leads to a characteristic failure to consider alternative explanations or to take into account diversity in or interactions among the objects of observation in what we otherwise take to be homogeneous aggregates. While this problem arises inevitably in the more empirically-oriented disciplines which respond to nature "as it comes", they argue that a danger in the more formalized disciplines which "purify" and separate inquiry from the complexities of ongoing situations and practical objectives is that specialization may restrict one's thinking to the construction of "easily solved mathematical models"--precise puzzle problems. These models not only become simplified, they note, but the simplifying steps are usually not made explicit, and thus the modeller is not aware of the simplicity--read artificiality--of his model. (Weinberg and Weinberg, 1979, p. 80)

Precise puzzle problem-solving exhibits the logic of what has been called in other contexts reductionism or operationism, in which the conduct of investigation progressively consumes the variety in describing systems, and subsumes knowledge of ever broader domains within increasingly narrow models and methods of analysis created by formalization.

Those pieces of information which can be quantified represent the precise problems which can be solved given methods already at hand, without questioning the process or context in which the methods are developed and used. The result of restricting the use of information in the conduct of inquiry to that which is already known is that uncertainties are progressively "imported" into the system in the course of development, and this is the ground for recurring management information crises, as managers attempt to resolve a growing number of problems falling beyond the parameters of the available models.

Systems development in the mode of management science as a type of informal engineering--procedurally formalized through methods engineering, work measurement, and work simplification techniques--exhibits what we have called a transformation of relations of exchange to hierarchies, a transformation which comes about because of the narrowness of quantitative models and which is inimical to organizational learning. Optimizing methods applied in ongoing problem-situations create and rationalize hierarchies by "driving out the slack" in operations, as Cyert (1963) would put it. In Koontz and O'Donnell's terms (1974) we are even more specifically driving labor costs from the "bias"--the solution space representing the objectives of the firm.

Eliminating the slack in systems in an effort to minimize or eliminate certain costs of production also serves to reduce the variety in that system, largely by defining input functions in an increasingly narrow range. This takes place, in part, because not all of the relevant factors in a decision-situation can be expressed within the methodology used to study the situation--which often means that this information is not amenable to quantification. As Johnson, Kast, and Rosenzweig point out, while production control lends itself to a variety of measures--quality, inventory, durability and the like, which are associated with the processing of physical materials--in administrative control, only cost-control is operational for defining bureaucratic services. This is especially the case for uncertain contexts--such as the implementation of computer technologies in offices--in which the objectives and/or methods are either unclear or inconsistent in some way; under such circumstances, only costs can serve as a universally applicable measure to represent other less tangible items. Even if it is not possible to define the service rendered, it is usually clear what the cost of rendering it was, and in this way cost-benefit analysis comes to serve as a universal language, accounting for the structure of operations in a literal sense. (Johnson, Kast, and Rosenzweig, 1973, pp. 64-65)

By focusing on current measures and categories in cost-benefit analysis of systems, optimization strategies presume and reinforce static conceptions of both system and environment; cost-benefit accounting reflects a kind of operationism in producing a record of the system which includes only those elements which can be narrowly defined in terms of costs. As a definition of the system, however, this record is insufficient to support redefinition of the system, or of its measures of performance, in the light of changing environments. Where environmental conditions are changing, it can happen that narrowly restrictive input functions prevent information regarding those changes from being communicated in the system because it does not fit the stipulations of the model, and thus is eliminated from consideration and is reflected (only) in increasing rates of error which cannot be explained from within the perspective of the designed system.

We may attribute this phenomenon to the fundamental distinction between living and artificial or designed systems, which is the locus of control and the self-consciousness with which control or determination of form is articulated and exercised. In designed systems, control can be built-in as a component of the system which is specialized to function as a governor--or comparator--on the other components; or it can be added as a managerial or engineering function to be performed by some person based upon a similar principle of negative feedback which compares actual and expected outcomes, adjusting the process according to pre-established objectives and processes. In designed systems, then, including socio-technical systems, control can be expressed in the form of negative feedback loops, whether executed by machines or people. However, this control mechanism cannot account for the generation of form or transformations in structure beyond the maintenance of stability.

Moreover, even the maintenance of stability through the application of negative feedbacks--the error-controlled regulator--is limited by at least three factors: 1) delays in recognition and reaction to errors; 2) interference from other mechanisms; and 3) loss of information on its own performance. Limitations due to the loss of information on its own performance reflect a weakness which is unique to error-control systems, and is associated with the circumstance in which systems become so efficient in controlling disturbances from the environment that fluctuations no longer register as significant. Since these fluctuations--errors, or problems--are the stimuli which induce action in the controller, a completely efficient control system will ultimately eliminate all information coming in from the environment, and with it all activity of the controller as well. (Weinberg and Weinberg, 1979, p. 225) This situation fits the classic definition of entropy, and there are those who might argue that this point has already been reached in modern management, and less broadly, that this dynamic may be a significant factor retarding the successful implementation of computer-based technologies.

Problems in implementation stem from abrogating the ongoing interaction with the environment characteristic of adaptation in living systems, including socio-technical systems, in favor of an imposition of designed production processes oriented to minimizing production costs and maximizing return on investment. Breaking this adaptive relation takes place because development and implementation are separated in the designer-based approach, with "social implications" becoming of residual importance to the main problems of designing and installing computer equipment. As Champine explains it, problems in current computer-based systems follow from the manner in which the technology of computing developed in use, rather than being fully worked out on in the laboratory on the basis of fundamental principles which are subsequently translated into applications. The results of this bootstrapping have been unanticipated--and often adverse--consequences for the reliability and acceptability of the systems introduced, generally experienced as errors which are not resolved by cntinuing development, because the separation of design and use means that design processes are deprived of the feedback which would confront them with problems in use. In this circumstance,

> "...(The) more complex systems are more prone to failure, and because of the increased dependence of organizations on computers, the failures that do occur will have more serious consequences. Also, when failures occur, they will be more difficult to diagnose and correct."
> (Champine, 1978, p. 5)

Systems degenerate because the models upon which order is predicated are too narrow to account for the variety in their environments, and thus the number of "residual" issues increases without a corresponding increase in the capability of the system to account for them. Degenerate systems reflect a process of organization which consumes increasing proportions of operating energies in responding to and preventing the occurrence of errors without reexamining and reorganizing their operating assumptions in the light of changing conditions. They lack the capability to be self-regulating. Current cost-justified implementation strategies have the potential to transform their host organizations into just such degenerating systems, as Allen suggested, destroying the careers of their designers and managers in the process. (Allen, 1982, p. 77)

Implementation Problems: In designer-based approaches to the introduction of technology, implementation is residual to the design of the equipment. Development in this mode proceeds in three stages: 1) design of the equipment; 2) development of applications (which often takes place through marketing and advertising); and 3) installation of the new system, which involves adapting the ongoing system to the new technology rather than the reverse. In practice this means that people and environments are adapted to machine technologies through recruitment, training, supervision, and through machine monitoring and pacing of operations. This usually also means that inadequacies in system design are identified through the experience of problems--often befalling production-line workers.

The adaptation of individuals to jobs follows from programmes in industrial psychology initially developed out of applied research which produced tests and training aids designed to improve employment interviews for salesmen. With the advent of World War I, these techniques, first introduced by Walter Dill Scott in 1916, were used to develop a recruitment and training program which used intelligence testing to eliminate those mentally unfit for duty and to recommend those of superior ability for promotion to officer status. Thus military psychology begun in 1917 produced two tests, called "a" and "b" in World War I and "alpha" and "beta" in World War II, which came to be the standard for industrial psychology--both on the side of management and that of unions. (Sgro, 1981, pp. 4-7)

Current approaches to implementation--particularly in the U.S., where union influence is negligible--focus on selection and training of workers for newly designed jobs, such as word processing operator and supervisor, and on procedures for managing the purchase and installation of the equipment. In Cecil's word processing programme, implementation begins with selection of the word processing supervisor, who--like the data processing supervisor before her, is responsible for subsequent steps in implementation. The steps taken in the implementation of word processing technology, according to Cecil, are the following:

1. "Selecting the supervisor.
2. Selecting the word processing secretaries.
3. Writing operating procedures.
4. Developing all reporting and measurement forms.

5. Developing work request forms.
6. Selecting and ordering equipment.
7. Selecting and ordering furnishings and supplies.
8. Selecting and preparing the site...
9. Working with vendors to plan methods of operating.
10. Analyzing documents and organizing ways they will be produced.
11. Recording stored information prior to beginning operations...
12. Coordinating with other service departments...
13. Scheduling for beginning operations and phasing in departments and users." (Cecil, 1980, pp. 251-252)

The division of labor in planning and implementation is laid out in the following schedule, which is typical of installation programmes:

Sample Orientation/Implementation Schedule

Date	Activity	Responsibility of
April 20	Announcement of new system	Word processing manager
May 2	Announcement of job openings	WP manager
May 9-13	Job interviews	WP manager and personnel
May 20	Selection of WP supervisor	WP manager and personnel
May 24	Begin training WP supervisor	Organizations which train personnel
June 5	Begin writing procedures manuals	WP supervisor
June 10	Announcement of WP/AS personnel selections and data of orientation	WP supervisor
June 20	First drafts procedures manual due	WP Supervisor
June 30	Completion of procedures	WP manager
July 10	Orientation program	WP manager/supervisor
July 27	Followup training session	Manufacturers/WP supervisor
August 2	Second implementation training session	WP manager/supervisor
August 3	Startup of WP system	All WP personnel
August 4	Third implementation training session	WP manager/supervisor
October 1	WP evaluation session	WP manager/supervisor

(Cecil, 1080, p. 253)

For Sanders, implementation is a matter of developing a plan, consisting of a set of documents leading to a budget and a schedule for purchase and installation of new equipment. These documents (similar to Wymore's artefacts produced by system design teams) support system design by detailing the sequence of steps leading to a running system, including: 1) specification, which is supported by an outline of corporate requirements, a systems analysis, and a systems design; 2) implementation, which is associated with programming, documentation, and training; and 3) assimilation, which is accompanied by testing, acceptance, and commitment. Systems design proceeds in a top-down fashion, while programming works up from the level of greatest detail to that of the complete--and running--system. Sanders suggests that it is necessary for the data processing manager (newly promoted to that position) to "grind" out of "his people" the requisite documentation, and like Cecil, he suggests a division of labor in producing that data, in the following documents, amounting to an implementation programme:

> 1. "Brief managerial description. (Written by you...)
> 2. A technical description of the program. (Systems designers and programmers.)
> 3. Instructions for the computer operations. (Programmers.)
> 4. Testing procedures. (Technical assistant and users.)
> 5. Corporate using procedures. (You, the programmers, and the users.)
> 6. Training manuals. (Systems designers and programmers.)
> 7. Preliminary economic analysis. (you, the systems analysts, and users.)"
>
> (Sanders, 1973, pp. 29-31)

Acceptance amounts to a complete replacement (and thus elimination) of the old system, and a billing of costs to user departments, reflected in the budget for the computer installation.

This "top-down" approach to computer implementation has the potential for introducing considerable uncertainty and unrest into ongoing organizations. Even Sanders notes that

> "In carrying out the detailed work...you will stumble upon the most unbelievable horror stories. This will be the first time in the long history of the organization that anyone has ever tried to find out what is going on. You will find rich pay-dirt, and the Top Man will probably have a heart attack. Thus, we see a very important and not universally recognized value of the computer. It acts as a corporate catalyst in that it forces the organization to examine itself, usually for the first time." (Sanders, 1973, p. 82)

Even Stultz, who defines implementation as falling under the step of "installing changes", which follows design, equipment costing, identification of applications, and purchase of equipment, notes that the success of installed systems depends on the manner of installation. He admonishes managers early on not to limit their thinking to equipment and related considerations, arguing that to overlook the people involved in word processing systems is a "classic and costly mistake". Autocratic management, according to Stultz, commonly overlooks the "informal organization" in favor of adhering to the "organization chart". Because they do, organizational change is resisted because it does not allow them to continue "doing nothing", and because newness per se is associated with fear of the unknown and disruption of comfortable, familiar patterns. The anxiety which may be created by change can only be alleviated if the manager can "sell" the change to his employees, and the best way to do that, Stultz argues, is to involve them early on in the decision-making process. (Stultz, 1982, pp. 21-26)

Appending human relations strategies of selling technological change in an essentially management science mode through participative management is a common strategy for computer implementation advocated by industry consultants, largely through their experience as change agents with problems created by autocratic installation of wide-reaching organizational changes associated with computer technology. The apparently paradoxical finding noted by Birn and later by Stultz that it is not lower-level employees, but managers who resist technological change is understandable in the context of the above approaches to implementation, and it is indeed the case that implementation has proceeded much more slowly in the U.S. than computer industry observers had predicted.

A major factor in the outcomes for implementation over the past two decades is that of the predictions made in the course of researching the introduction of computers. In 1958 Leavitt and Whisler predicted major structural changes to follow from the advent of "information technology" in organizations. These changes were to include: 1) moving the level of planning upwards and making planning and design increasingly remote from performance; 2) increasing the structured-ness of middle-management jobs, increasingly limited to instrumental reasoning and severely bounded discretion; and 3) a recentralization of large organizations through which creative functions of innovation and planning will become the exclusive province of top managers, and permanent downgrading of certain classes of middle-management jobs.

> "One major effect of information technology is likely to be intensive programming of many jobs now held by middle managers and the concomitant "deprogramming" of others."
> (Leavitt and Whisler, 1958, pp. 41-44)

Specifically, it was expected that programmers will displace the judgment and experience of those who previously had scheduled production, and that programmers themselves should move up in the organization, appearing in staff roles close to top management. (Leavitt and Whisler, 1958, p. 45)

However, by the 1970's, when the outlines of structural change brought about by the introduction of information processing technology were clearer, Whisler found that the jobs in the middle had considerably more responsibility after the implementation of computers than before, and rather than being displaced, they found themselves increasingly performing complex, creative, innovative and problem-solving work associated with the implementation per se. In this role, Whisler discovered that these middle managers--aware of the predictions of their impending decline and demise--were in a position to control the scope of organizational change by directing the way in which new technologies were to be implemented. (Whisler, 1970, p. 7)

Lucas found that most unsuccessful computer installations had failed because they overlooked users and organizational issues in favor of a focus on technical aspects of systems and equipment. Arguing that design is a creative task involving an understanding of user needs, Lucas identified three fundamental problems in systems design and implementation: 1) technical issues involved in designing the system, writing programs, testing, and converting old records and procedures; 2) organizational issues associated with changes in job content, work relationships and organization structure; and 3) problems in the management of change, including the coordination of users, computer staff, and consultants in developing and managing complex and changing systems. Organizational changes frequently elicit resistance--especially on the part of line managers--as a result of modifications in the distribution of power implied in the change situation, which creates uncertainties for some individuals and departments, and creates opportunities for other departments to resolve those uncertainties and thus to enhance their influence in the organization. For these reasons, Lucas concluded, management has focused the implementation of computer technology on clerical applications, while the impact of the technology on management has been "rather small". (Lucas, 1974, pp. 2-4, 10)

Clearly, the direction in which implementation has proceeded in this top-down designer-based approach has been in the mode of structure-preserving change, directed to the mechanization and automation of routine operations, routinizing many formerly diverse activities making up clerical work, but overlooking other, potentially more productive, analytical uses of the technology as a "decision-support-system" by managers. Part of the reason for this relative underdevelopment can be attributed to problems experienced even in the simpler transactions-processing applications which have been introduced in clerical work, including: 1) cost overruns and delays in installation; 2) suboptimal use and resistance (in some cases even sabotage) to the technology; 3) unreliability and excessive error rates and expense in operations; 4) fears of job displacement and unrest in the work force, manifest in high rates of turnover and absenteeism and in a wide range of "ergonomic" complaints by operators, including vision problems, muscular-skeletal strains, and job-related stress exhibiting a range of physiological and psychological symptoms.

Wessel laments the fact that even after more than ten years' development and implementation of computer information systems, systems developers and administrators still do not have a consistent body of accepted understandings emerging from this experience. What is striking, he argues, is the continuing stream of errors and problems associated with systems implementation and operation.

> "The world of complex information systems seems always new--the next case unrelated to the last! As such the errors are repeated and even intensify over a period of time. The results are high costs, unsatisfied expectations, low morale, poor performance, organizational chaos, administrative burdens."
> (Wessel, 1979, p. 3)

Part of our inability to account for these errors, Wessel argues, and for the complexity and uncertainty of implementation is that the kinds of adaptive interactions between information, people, and computers are not well-explained by the conventional models of science exemplified by Newtonian physics. Because "information, information systems, people, and their organizations are social products" it is not enough in organization analysis to perpetuate a conventional (order-presumptive and structure-preserving) approach to studying information systems based upon a one-sided focus on hardware issues, which, by extrapolation, are extended to the design, development, and implementation of complex systems involving people and organizations.

The underlying problem in these approaches, and "Probably the greatest single cause of computer system 'disasters'", according to the Weinbergs, can be attributed to the failure of designers to take into account the broader systems requirements for structural stability in their single-minded focus on feasibility which emphasizes a set of calculations which show "how every last drop of 'efficiency' will be squeezed out". Among those elements which may be squeezed out, they argue, is structural stability.

> "Things run so efficiently that the designers win awards. Then comes the first 'structural' failure, usually blamed on the operators, rather than the designers...But it is the designers who...have forgotten to ask, 'What regulates the structure matrix...who regulates the regulator?'"
> (Weinberg and Weinberg, 1979, pp. 160-161)

Similar problems occur with the management as well as the design of computer systems. We have in the introduction of data processing and word processing technologies an ongoing example of the way in which problems in implementation affect the course of organizational learning. Nolan's six stages of growth in data processing represent an example of organizational learning which is particularly relevant to the course of office automation. Movement through these stages is directed and influenced by an external body of knowledge representing the data processing profession (itself changing in response to developments in information technology) in conjunction with an internal body of knowledge representing the largely experiential knowledge of organization managers, specialists, and operators. (Nolan, 1979, p. 116)

According to Nolan, there is an appopriate balance between control and slack in management approaches at each stage in this learning process. In the early stages of initiation and contagion, high levels of control and low slack can obstruct the implementation and use of information technology; where slack is high and control is low in later stages of control, integration and data administration, data processing systems can become inefficient and inordinately costly. Nolan observes from his studies of over 35 corporations during the 1970s that there comes a point of data processing implementation, usually in the control stage 3, where data processing shifts its orientation from management of the computer to more functional management of the resources of the company. At just this point data management undertakes to rebuild and professionalize the data processing activity, while at the same time holding users responsible for the costs of data processing--while they have not yet, in their terms, managed to get from data processing the information they had wanted. At this point they "give up on data processing". Just as users give up, however, the data process

Just as users give up, the data processing department moves from less efficient and less responsive batch processing applications to provide data communications and data base technologies which facilitate interactive inquiry, and users begin to perceive real value in their applications. Nolan comments that in at least one representative case, in a company with over 1500 applications, "users ranked their data base and interative applications as far and away more effective than users of conventional or batch technology ranked their applications." (Nolan 1979, p. 120)

Companies customarily move through these stages with the impetus of a series of crises; in Stage 1--initiation--low level operational systems such as accounting are automated. At Stage 2--contagion--because of a continuation of the low control and high slack necessary to introduce new technology, at some point loosely expanded operational systems become unable to support higher-level applications, and the maintenance of the computer system begins to consume the majority of programmers' and analysts' working efforts. In Stage 3--control--a series of reorganizations takes place which bring about a shift from managing the output to managing the data resources of the company. At this point the applications portfolio is restructured to integrate applications, and alternative ways are found to bring about user accountability. Data administration is introduced in Stage 5, which facilitates the use of data base and data communications technologies, and Stage 6 represents the completion of the applications portfolio, which now "mirrors" the structure of information flows in the organization.

Rather than arguing that there is one best form of management or organization for companies incorporating data processing technology, Nolan argues each stage has a characteristic form of organization which is most appropriate in terms of the slack or control required to accomplish the objectives--for learning or for consolidation--at that stage. A similar cycle exists in the introduction of word processing. Based on her study of 200 organizations using word processing technology, Johnson cites a number of factors associated with adoption and use: 1) sponsorship of top management, 2) experimentation, 3) visibility of use by key people throughout the organization, 4) communication among operators in the organization, and 5) adoption of the technology in evolutionary stages. She found that although most installations had the backing of some executive, there has to date been little planning and almost no systems thinking associated with development.

What she refers to as the IBM strategy to set up word processing centers "in the basement" staffed by low-level people, without consultation with principals results in resistance on the part of both operators and authors--who are reluctant to lose control over their work and status in the organization. Supervision and training are not necessarily included in the early stages of implementation in this plan, and instead people are merely assigned to these duties. Where high-status secretaries are transformed into word processing operators, problems arise because of their perfectionism, lack of supervisory skill, and lack of "system sense". (Johnson, 1983, pp. 21-22)

The result is a crisis at some point, at which the failure of management to plan effectively is seen as a technical failure of the system, and there is pressure to abandon word processing centers. Just as in data processing, word processing centers become established just at the point at which frustration is highest among users. Where word processing centers initially must overcome resistance to using the technology among the users, once established these centers encounter a second crisis as their procedures and organizational structure come into conflict with demands from users for more functions and for distributing the systems. Rather than resisting such demands--which entail breaking down the organization which centers have established--Johnson argues that word processing specialists should then introduce new functions, languages, and activities to extend the capabilities (applications portfolio) of the system.

We can argue on the basis of these experiences, that operations research-type methodologies in the dominant designer-based or process-control mode which treat total production processes--social organizations, in other words--as if they were automata run aground when it comes to implementation. In translating formal systems designs into outcomes for action, the ability of those abstract designs to explain and to order action and outcomes in different contexts is limited by the complexity of those designs and of the environments into which they are being introduced, and by the uncertainty arising from elements in the environment not included in the design--including the process of translating that design into action, which is implementation.

There are even indications of implementation problems in the literature describing the successes of World War II operations research. Wymore referred to "personality clashes within interdisciplinary teams" (Wymore, 1976, p. 382) and the necessity for field assistance to translate newly designed systems into use was accompanied by organizational problems. If field assistance was not provided, field officers. experimented with the technology, imposing self-designed tests based on frequent misunderstandings of the nature of the equipment, with the result that erroneous conclusions were reached as to the potential benefits and limitations associated with use. In some cases--with obvious parallels to computerization--users concluded that the equipment would not work, or would not work for their particular needs; and in some cases it was indeed true that certain problems (for example, mine detection, or underwater demolition) were intractable using the forced-draft, or designer-based mode of research. Finally, Baxter notes (again, with obvious parallels to computer industry consultants) that

> "Some travelling scientists were too zealous, and ran foul of the accepted doctrine of the War and Navy departments that weapons under development are not to be sold to commanding officers in the field...Some missions left a trail of misunderstanding and antagonism; others were highly successful." (Baxter, 1946, pp. 104, 126, 407)

It appears that change--including the change necessitated by and entailed in the process of implementation--undermines the predictiveness of initial designs as a model or blueprint for ordering events and environments of use. The whole designer-based perspective is to this extent fundamentally limited because a description of the designed production process does not take into account the processes of design, or implementation, or the kinds of decisions required to modify and replace such designs.

In general we can notice that the social technology of operations research (broadly conceived as the range of methods which arose out of the operations research movement during World War II) developed out of and is best applied to conditions of uncertainty and change. Institutionalizing that methodology has had a number of benefits for organizational development: 1) Increases in the elegance and simplicity of the knowledge base serve to make the flow of information more efficient and less ambiguous; 2) Formalized methods make it possible to create algorithms--precise maps of the processes of interest--which is a necessary first step in creating effective procedures which can be reliably mechanized; and 3) Such an approach makes system structure explicit, and illuminates the degree to which clear-cut procedures at once rationalize the production process and also (as Weber, Perrow, Meyers, and other of the bureaucratic school have argued) serves to protect the autonomy and personal interests of individuals working in positions within organzations which are bound to a certain scope of authority.

However, the translation of that programme of inquiry into a set of rules or procedures has rendered this process an objectified design in itself, to be applied or imposed on the investigation and design of future systems, which must develop within this framework. As a description of the process of system development, the routinization of inquiry itself represents a degenerate system design system, which allows in its methodology less variety and information input in its later stages than in its first informal iterations, which were not guided by such a methodological programme. This it does in two ways: by radically simplifying the information accepted into the system, and by overlooking new information altogether.

Information is simplified through a variety of order-presumptive strategies for decision-making as reported in the organizational literature, especially by Simon (1958), Cyert and March (1963), James Thompson (1967), Argyris (1978), and Koontz and O'Donnell (1978). These strategies include: 1) premising the activities of subordinates in formal system design, which generally implies implementation without prior consultation; 2) satisficing, which accepts a suboptimal solution to different problems rather than expending the effort to determine a good design; 3) short range planning and control; 4) isolating and buffering the technological core, cutting off users and designers or data managers, modularizing applications for maximum efficiency of communication; 5) uncertainty avoidance, whereby information is simply eliminated from consideration which is not meaningful in terms of a given model; 6) uncertainty absorption, where information obtained at the boundaries of an organization is summarized and abstracted at each level through which it passes on its way to a decision-maker; 6) quasi-resolution of conflict or analytical approaches to conflict resolution; and 7) we could add the form of operationism which extrapolates design of jobs from instructions for the equipment and/or from simple reproduction of existing procedures and categories of information.

An alternative possibility is that analytical models and methods will not be used at all, with implications both for the success of computer implementations and for the extent of use of information technologies. In spite of the increasing power of quantitative and systems analytic methods, and the considerable advantages associated with the increasingly widespread use of computers to manage the computations associated with such complex models, management science models are not widely used even yet, and are even held in disfavor in some circles. Cochran notes that although theoretical models of organization and design as developed in business schools and in the organization literature have traditionally held little currency with line managers, who prefer informal sources of information, the operations research movement has already involved more data collection, more attention to factors in the environment, and more use of computers than ever before. However,

> "A major difficulty in this kind of approach is the cost in time. A complete simulation model may take three years of staff time to prepare and in the end not prove particularly useful. As a result the use of systems and mathematical approaches declined at some large companies in the hard times of the late 1960s and early 1970s."
> (Cochran, 1977, p. 207

Koontz and O'Donnell also note that in spite of the potentialities of operations research methodology, it has found application in only a small number of managerial problems. Two major difficulties with applying such methods have to do with the "sheer magnitude" of computation, which reaches the limits of available computational techniques with even relatively simple problems, and with the realization that many of the factors associated with managerial decision-making involve qualitative attributes not amenable to quantification. As an example of such limitations, PERT techniques require an unambiguous statement of the problem or program for analysis, which is difficult when that definition is uncertain or in conflict, and impossible where it is undiscussable. A recent study conducted by Koontz and O'Donnell disclosed that although many industries were still keeping network plans and information, they were not using PERT techniques in their actual control of operations. The reason they give is the same reason for the dis-use of management information systems (M.I.S.) and for other quantitative and formalized techniques for managerial analysis and control: 1) "...specialists in the field promise too much and users become disillusioned"; and 2) the problems to which the techniques might be applied are too complex to permit the valid applicatin of such techniques. The response is, therefore, typically not to use these methods. (Koontz and O'Donnell, 1978, pp. 119, 504)

These findings are consistent with the discovery by Steinbrunner, Mintzberg, Simon, Cyert and others that decision-makers do not necessarily seek out information on the model of analytic planning and problem-solving. According to Cyert,

> "Organizations learn from their environments in only a limited sense. Decisions are contingent on feedback, but decision rules are not."
> (Cyert and March, 1963, p. 99)

Mintzberg argued on the basis of his research that the relative failure of the concept of management information systems can be attributed to misconceptions about the activities which comprise the manager's role. His study showed that "...managers are not reflective, regulated workers, informed by their massive M.I.S. systems...". Rather, managers spend the bulk of their time in communication in "interpersonal, informational, and decisional roles". Specifically, instead of engaging in systematic planning, managers spend the majority of their working hours in activities which are characterized by "brevity, variety, and discontinuity". Although managers do perform a number of regular duties, contrary to the prevailing myth, he argued that they customarily dislike reflective activities and this is the underlying reason for the underwhelming use of management information systems. "Managers are simply not using them", Mintzberg argues, noting that they prefer using verbal media--especially telephone calls and meetings--which are associated with "soft" information. Although there is evidence that management is overburdened by the demands for information and communication involved in their role, there is little to indicate that the increasing pressure of work leads to increased use of formalized models and methods, and it appears that the computer has little influence on their work activities. Instead,

> "The strategic data bank of the organization is not in the memory of its computers but in the minds of its managers." (Mintzberg, 1975, pp. 32-34)

As do Cyert and Mintzberg, Steinbrunner implies that managers do not seek out information on the model of decision-making associated with the analytic paradigm because the reauirements for information-processing associated with calculating alternative outcomes and updating information in light of pertinent data. Because the demands of the analytic paradigm are unrealistic in all but the simplest applications, the actual result, he argues, is a rejection of these central assumptions in favor of what he calls a "cybernetic" decision mechanism of uncertainty control, which focuses the decision process on only a few incoming variables and overlooks outcome calculations altogether, in favor of a few set responses. This considerably reduces the variety in the system, and sets up a tension between adaptive capacity and internal simplicity which Steinbruner argues is a fundamental issue in cybernetic analysis. (Steinbruner, 1975, pp. 65-68)

A further reason why information is not actively sought and analytic models applied to planning and implementation of computer applications is that the activity of modelling can also serve to heighten the awareness of social divisions and conflicts within the organization as it makes these assumptions explicit for purposes of planning and changeover. Thus it is that the conduct of feasibility studies preparatory to the introduction of new equipment can not only improve the efficiency of operations but also can add to a sense of conflict and uncertainty in firms, especially those in which the underlying assumptions upon which procedures and methods are based reflect conflicts between different sectors in the organization.

The recurrence of these unanticipated errors in implementation--complaints of working conditions, and failure to achieve expected increases in productivity--may call forth three types of effort, at least two of which are especially order-presumptive and structure-preserving: 1) One is to extent the classical model of organization, adapting it to contemporary environments by incorporating models of human factors engineering and human relations to address problems of environments and implementation--the conventional human factors approach most characteristic of American contingency theories. 2) A second alternative is to attempt to increase the adaptive capability of the total organization by developing appropriate styles of organization management. This approach is in a sense an expansion and outgrowth of the earlier human factors strategies for adapting the workplace to new technologies, however, it focuses on the structure of the organization rather than the adaptation of the individual to that structure.

Wessel observes that the view of information as a "human product in a social context" emerged in 1975 in reaction to the errors and conflicts experienced in early computer implementations, and partly in reaction to the abstract nature of information research which was dominant in the 1950s and 1960s. The trend moved away from treating information merely as data, and toward an attempt to understand the relationship between users and information and systems within their organizational and social contexts. This trend began in the U.S., Wessel argues, and spread to Great Britain and Sweden (Wessel, 1979, p. 6) from whence it is being reintroduced into American organizations through socio-technical systems approaches, such as Mumford and Henshall's (1979) ETHICS approach, which emphasizes worker-participation in bottom-up planning for implementation as the key to more humane and successful implementations and more reliable systems.

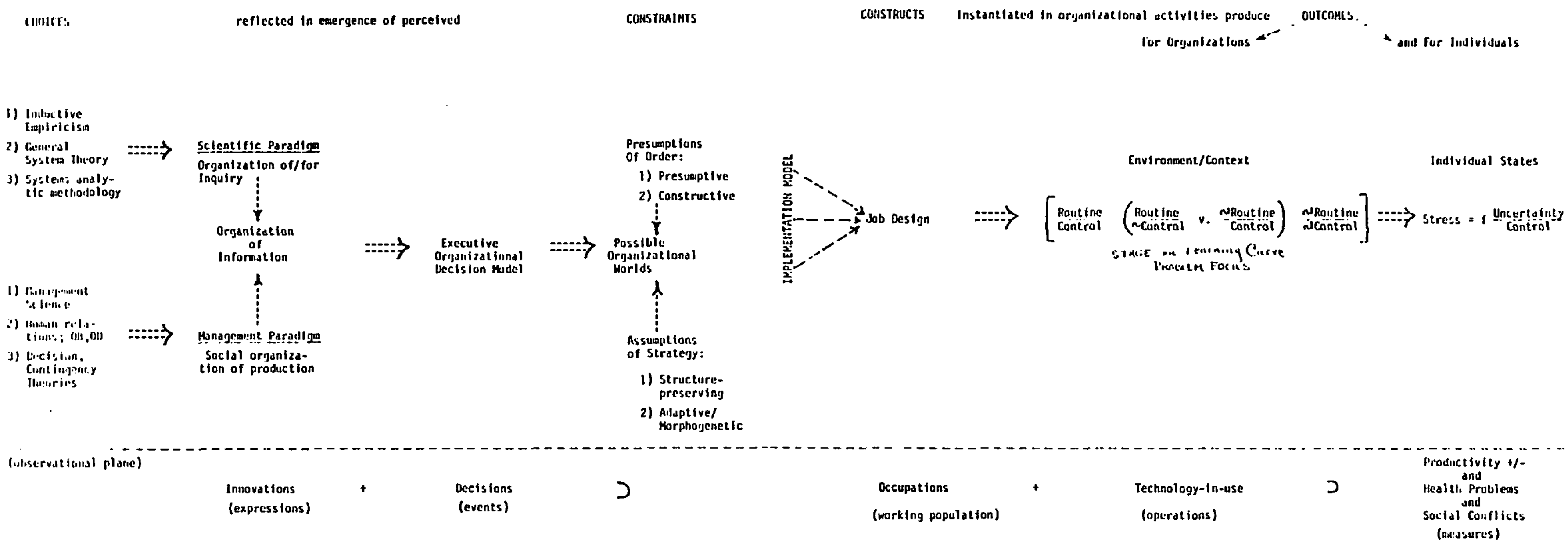
Although this approach is receiving considerable attention in current implementation research and on the consulting circuit, and especially in union circles where it is associated with the quality of work-life movement, Berg et al argue that not only are work-reform, interventionist organizational strategies of dubious effectiveness, serving often only to completely unsettle complex social arrangements without producing their expected results, but these "new" innovations in work reform are neither new nor did they originate in Europe. Rather, such innovations as work-redesign, work-enlargement and experiments in word-restructuring and participative management recently being undertaken in Scandinavia and Great Britain and elsewhere in Europe "...tend to overlook the fact that these ventures are rooted in American and, later, English human relations work" at the Tavistock Institute prior to 1950, and much earlier in the industrial-relations experiments reported in American shops as early as 1904. (Berg et al, 1978, pp. 223-224)

The third alternative suggests, however, that America may be a pioneer not only in developing progressive forms of management for innovation, but in giving up such work reforms. It is also the case that problems in implementation and use provide the impetus for continuing design efforts to further mechanize the process, building in the complexity of these environments into greater flexibility and power in the technology, and solving human interface problems by attention to processing requirements which can be incorporated into machine design. This is the current designer-based thrust in the direction of developing expert systems applications, which will enable the replacement of skill and motivation at all levels. This approach makes it possible to further lower training and skill requirements, rendering the use of the machine more accessible, permitting the location of employees in such a way that distracting interactions are minimized and task-related interdependencies are built into the workflow, and finally, using the occasion of change--already fraught with conflict and uncertainty--to transform the social organization of the office by physically closing or moving operations, displacing employees by attrition, and reassigning them to different positions within the organization--all strategies which were employed in differing degrees throughout the stages of computerization in offices.

What we see in these conflicting developments is a fundamental discontinuity in the methodologies of operations research which emerged from the World War II era. The broad theoretical formalization of the concepts and methods of operations research (either through the cybernetic calculi or through model-relevant positivism) does not reconcile easily with the programmatic emphasis of operations research as a method or set of methods associated with the predominantly inductive empiricist orientation of these practical measurement approaches. Extensively--objectively--they would appear to share the same techniques arrived at by different paths, in each case directed to the rationalization of the enterprise, with the process of organization carried out through the application of methods of scientific inquiry to practical objectives. What differs is not the propriety of practical application, but the conception of scientific method and of organization structure which is entailed in this application, and which can be investigated by separating our view of the system proposed <u>for</u> an organization in some formal design and design methodology, and the system <u>of</u> organization representing the ongoing context into which that application is to be introduced. The total process of combining these theoretical and methodological approaches into some strategy for guiding the course of implementation can be outlined in the following diagram.

Social Definition of Reality in Technological Innovation and Implementation of Automation

Operates according to a mechanism of progressively constrained alternatives and opportunities.



We have attempted to show that the designer-based mode of systems engineering, in spite of a technical language which purports to be isomorphic with that of systems analytic methodology, such as those developed by Ashby, Wiener and Wymore, actually contradicts several of the critical premises of the analytic model: 1) the requirement for systematicity, 2) the requirement for the free-flow of information and resources, and 3) the requirement that new information be incorporated into existing models, transforming them as a context for future development on the basis of past experience, which is the hallmark of an adaptive system. However, we have argued that the dominant designer-based style of development has actually emerged as an order-presumptive and structure-preserving variant of systems engineering methodologies--largely by becoming institutionalized in data processing departments and professions--and in this way actually blocks the very development which it would foster by a number of characteristic approaches to inquiry which are inimical to further innovation and which elicit conflicts of interest and social unrest in social systems undergoing change. This is the context for the experience of ergonomic problems associated with computerization.

Chapter V

The Ergonomics of Office Automation

In the foregoing chapters we have argued that the social process of systems definition creates "structures"--defined as systems of knowledge, technological artefacts, formal organizational arrangements, and designs for machines--all of which on becoming objectified represent constraints on future action, defined with respect to the limitations in the models guiding development. These constraints make up the context for development, a context which is defined and bounded by a set of assumptions and presumptions making up observers' and actors' view of the world. Problem-solving is an inherently subjective activity which not only alters the context(s) in which it takes place, but in which the influence of context is significant both in the identification of problems and in the forms of organization established to solve them. Differences in context alter the path or style of development in at least two ways: 1) by differences in the importance of practical objectives, and 2) by constraints and opportunities present in that context--in other words, the capability for change.

The current and continuing phenomenon of office automation is itself an instance in the provisional organization of socio-technical systems based upon the introduction of new technology--computers and word processors. Two paths of development are emerging in the course of introducing office automation--one in which the processes of change are inherently stressful, and in which the outcomes can be both confining and hazardous to the health and well-being of office workers; and one in which change is essentially liberating, leading to organizational and individual learning. In the former, the computer is an "inquiring system" (as perhaps first described by Churchman (1971) and now the model for artificial intelligence approaches to computerization); in the latter the computer is a tool in an inquiring system, which in this case is the total organization--a distinction which has implications for learning at both the individual and organizational level. The outcomes of the latter proliferate structural and social change in organizations, and hence this model is relatively submerged by dominant order-presumptive and structure-preserving paradigms of organization. The outcomes of the former, while based upon the objectives of increasing productivity, actually appear to be producing both error and conflict in ongoing organizational processes, and a spate of ergonomic complaints focused on the new equipment being introduced.

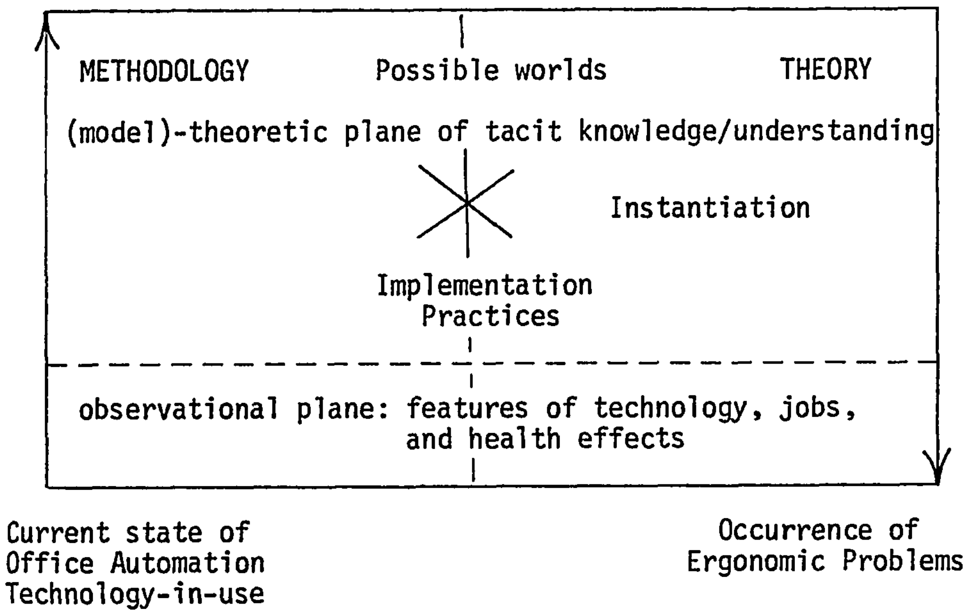
In this chapter we will argue that there is a relationship of mutual causality between the incidence of error and stress in ongoing implementations, which is mediated by the nature of the organizational context and the processes of implementation by which it is transformed. A major problem in office automation lies in presuming that the production mode is appropriate for office work. This presumption does not match the nature of office work, which is characteristically variable and non-routine. It overlooks the fact that even if clerical work were routine under normal circumstances, the implementation of technologically-induced change in new tools and methods of work is itself a non-routine process fraught with uncertainty.

This presumption of order is, in fact, an artefact of the models of organization and of implementation of new technology, and is a major factor producing error and stress. Not only is the designer-based paradigm of organizations too narrow to account fully for the full range of production processes in the context of ongoing organizations in their broader socio-cultural environments, but it is also too narrow to account for ergonomic problems arising out of this mechanistic style of development, and thus insufficient to remedy them.

Building on the characterization of order-presumptive theories and methodologies of office automation begun in Chapter IV, we can argue that presumptions of order have at least two adverse consequences for organizations and individuals: 1) They overlook factors beyond the boundaries of the conventional models-in-use, and where those models are insufficient to explain events, there is no effective method for searching out information and applying it to problems. In fact, we have noted information strategies which protect knowledge bases from new information; consequently organizational change and development can only be driven by the experience and unreflective solution of a continuing series of problems.
2) Moreover, presumptions of order both limit and constrain the possibilities for individual growth and mobility. In particular, both design approaches and opposing union approaches proceed from the common premises of office jobs as a) machine-driven and b) essentially "women's work", in effect reifying those presumptions in the process of implementation, and progressively narrowing the range of possibilities open to development by introducing a positive feedback cycle into the process of development which threatens to undermine the success of implementation, the strategic

capabilities of organizations, and the aspirations of office workers to improve their status through the introduction of new technology. The relationship between the methodology of office automation and the ergonomic problems experienced as a consequence of introducing new equipment in ongoing contexts is diagrammed below:

DEVELOPMENTAL PATH



A major deficiency with the order-presumptive paradigm for research and development is that it tends to focus all its attention on formalization of what is often an implicit or presumed system of knowledge, and that it characteristically overlooks the processes of inquiry and implementation in instantiating the formalisms produced from this process. By neglecting the context and process of design and implementation, order-presumptive paradigms are unable to account for the manner in which "pre-existing" and "emergent" constraints combine with the specifications and assumptions in designed systems to create error-ful outcomes--or what we define as "problems".

Structural presumptions of order at the organizational level prevent reconfiguration of socio-technical arrangements in organizations which is necessary for the successful introduction of new technologies, and over the long run for the support of innovation and strategic problem-solving and decision-making as ongoing organizational processes amounting to continuous organizational learning. At the individual level, presumptions of order--such as those characterizing the designer-based mode of system development--prevent renegotiation of the distribution of rewards and requirements in the definition of new occupations, and in the development of new technological applications of office technology, with implications both for the reliability and acceptability of office technologies implemented in ongoing environments.

One of the dirtiest "rhetorical tricks" in office automation is that of dangling occupational mobility before women in situations of technologically-induced change, representing machine-operators' jobs as entry into secretarial occupations, and secretarial jobs as "stepping stones" to higher level--managerial and professional--occupations. This is a common theme accompanying technological change in American society, and is indicative of the values which we associate with the development of technology; however, the articulation of this ideology often obscures outcomes of the job definition process which are quite opposed to the mobility they promise.

An examination of the history of office work in the U.S. suggests that the continuing process of machine-based job design has more often than not served to downgrade jobs, and indeed entire occupations, by tying them to specific types of equipment, and by eliminating intra-organizational "ladders" of occupational mobility in favor of permanent specialization in discrete and unconnected categories of office work. This same process can also serve to confine the definitions of jobs to levels of specificity and routinization which are insufficient to resolve the full range of tasks encompassed in the traditional functions and roles of office work, with negative consequences both for the successful accomplishment of the work and for the well-being and opportunities of those performing it.

The clearest indication of the constraints comprising the designer-based technological style of office automation is perhaps to be found in the language in which office automation is operationalized--in the definitions of titles and roles, of tasks and relationships, and especially in the expressions of opportunity and mobility which are associated with the design and marketing of office equipment and the types of application intended to be performed by office employees. In this view, certain expressions--or linguistic strings--constituting conventional definitions of computer-based office applications and occupations thus have the effect of constraining or precluding the instantiation of certain possibilities from among a set of known possible alternatives for the organization of office work. Thus we are restricted to the continuing development of transactions-processing approaches to computerization when numerous analytical or learning approaches already exist, which in a broader sense limits the strategic potential of organizations undergoing change.

Furthermore, limitations on alternative outcomes of organizational design, and limitations on the process of choice itself (as entailed in design and implementation strategies) may adversely influence the adaptiveness of systems in their environments, through reducing the repertoire of knowledge and process and through reducing the flow of information necessary to knowledge production and problem-solving. These limitations have negative consequences for organizations both at the corporate and at the individual level, and are a major factor in the experience of ergonomic problems--especially stress--in the context of implementing office automation.

Language Analysis as a Tool in the Study of Organizational Change:

The role of language is central to the study of organizational change, and particularly to the understanding of relative organizational capabilities for change in the context of different underlying methods and strategies for control and communication. 1) Language is reflective of socio-cultural forms of thought--which are expressed in various linguistic forms. Language as it is used is itself a product--or artefact--of the process of articulation, or the social definition of reality through expressions and constructions of various forms of thought. Linguistic methods of research--and particularly discourse analysis--thus represent a potentially powerful tool for organization analysis, a tool which is more adequate to explicating the full range of factors impinging upon social-technical processes of development and implementation than more conventional quantificational methods of systems analysis and operations research. 2) Language use is also productive of social structures, thus giving us a basis not only for accounting for structure in existing systems, but for explaining the generation and transformation of structure as it reflects paradigm shifts at the underlying level of understanding.

Buckley defines language use as an "epistemological process" consisting of a mapping of coded--or ordered--information from the environment through a series of transformations which re-code or process that information through human "...sensory, linguistic, and other mental or neurophysiological mechanisms". These information-processing transformations translate various codings (or representations) of the environment into decisions and actions which embody transactions with the environment. These transactions close the loop in information processing by modifying the attributes of the environment as recognized, thus changing later inputs to the system. (Buckley, 1972, p. 189)

Because information represents a mapping between some observer and the environment, it is inherently relational and thus it is meaningless, Buckley argues, to ask what the "real world" is like apart from the knower. Rather, the medium of "reality" as a socio-cultural product is language itself, which has "long been considered an important contributor to the structure, and perhaps the content, of knowledge and thought processes." (Buckley, 1972, p. 195)

People experience their worlds as "real" by externalizing their perceptions in self-expression--or language use, in Narasimhan's terms. The most straightforward way of externalizing and thereby reifying our experiences is through naming (and, by extension, codifying) them, which makes it possible to remeber and to refer to them. This reference--or remembering--is the key to the organization of our knowledge. Once we can identify and refer to our experiences, we can communicate them to others, and thus can translate ever more complex ideas into "reality" through cooperative endeavor, facilitated by language use in this most rudimentary sense. The source of people's beliefs about causality--and thus the ground for our complex designed systems--is not logical necessity, nor ideology, but a presumptive ontology grounded in habit and custom. Order or structure is attributed to the environment by people, and their understanding of the connectedness or causality among their observations is developed in the language of expression in which they articulate their experiences. The meanings expressed in those languages represent agreements among people as to the nature of their experiences, and as reflected in patterns of language use, these meanings thus constrain--and order--the range of possible observations people in a given language community can express, and thus the types of actions they can meaningfully undertake. Language-in-use, then, is not only indicative of patterns of organization, but of patterns in the process of organizing as well, through the reflexive aspects of language, which add to our adaptive capabilities vis-a-vis the environment in processes of continuous learning and interaction. In this way the structure of organizations has a hypothetical--or provisional--character, changes in structure reflecting changes in the methods and models on which organization structure is predicated.

Thus as Holzner has suggested, various linguistic forms of expression are likely to indicate sources of constraint (and structure) in the broader organizational environment, as they reflect and are constrained by the types of expressions which people typically can construct in interpreting their experiences. (Holzner, 1979, pp. 99-100)

The social agreement as to the meaning or significance of people's observations as expressed in some language is what we mean by convention (usage) or custom as the basis for all knowledge and social action. Style, then, can be defined as a recognized body of conventional understandings, embodied in expression, action, and artefact; and context can be identified with particular styles, defined in terms of some such set of conventional understandings. This suggests that the analysis of context can then be undertaken by a linguistic analysis of the expressions in which that context is represented.

We have argued that organization structures (as the context in which development takes place, as well as the context into which new technologies are implemented) are represented in models or paradigms of organization making up the knowledge base--or organizational memory--of given systems. Models of organization represent patterns and flows of information in a number of different instantiations, including: 1) the technical processes embedded in machine designs and operating procedures, 2) in administrative functions and procedures centered on the recording and reporting of information, 3) in codified documents, and 4) in the flows of interpersonal communication and social interaction in each of these domains, which when taken together, constitute the structure of a given organization at some point in time. Therefore, if there is an "objective reality" to organizational models, it is as expressions in languages, or as language systems themselves, which can be analyzed for their coherence, scope, and commensurability with other systems.

Systems definition as carried out through modelling of various kinds constitutes a simulation of some assumed conceptual system; in designed production systems, technical interdependencies stipulated in the design, once put into operation generate the structure of ongoing organizations in networks of communication and reporting relationships, flows of records and other information through various procedures, and in designed--machine-based--processes. Hempel has noted that in defining hypotheses, the conjunction of two or more hypothetical statements asserts a "reality" which is entailed in a set of theoretical connections--which we would call interdependencies. This reality is a construct--an artefact of the conjunction of two or more definitions reflecting the provisional reification of systems through the assertion of some theoretical relevance--systematicity or connectedness--among a number of empirical statements. This connectedness is what Halliday refers to as "coherence" or "texture" in discourse, suggesting that one way in which to study organizations is through analysis of the languages in which those organizations are represented, which gives us a basis for comparison, evaluation, and concrete reference (i.e., documentation in the form of blueprints and verbal statements) for our organizational constructs...and at least one--cautious--way of characterizing the organizational environment as experienced by some actor.

A number of discourse analytic methods of inquiry have been developed which can be used to characterize and compare conventional definitions of technology, task, and organization, as these emerge through the development of office automation, and to analyze the understandings which clerical workers express about their work. Discourse analysis is the analysis of any language string longer than a sentence, including text as well as dialog, and suggesting possibilities for analyzing anecdotal and interview data which is not meaningfully quantifiable, by focusing on those elements which hold a text together, making it appear as a "coherent" whole. According to M.A.K. Halliday, the cohesion or patterns of meaning in a discourse reflect the semantic relationships between a sentence and the environment as understood or recognized by some speaker/ actor who thus attributes those semantic relations to the environment. (Halliday, 1976, pp. 26-27)

That set of expressions which is indicative of all those things which an actor recognizes as part of the "real world" Halliday calls a "register", which reflects both the scope and the orientation (or mode) with which that actor perceives the world. This register is similar to Van Dijk's notion of "frame", which is a "subsystem of knowledge about some phenomenon" reflected in a set of meaning postulates and propositions characterizing a system of knowledge, formed by a set of semantic relations. (Van Dijk, 1977, p. 137)

Grammatical cohesiveness or structure is indicative of the choices which speakers make in expressing their ideas. According to Halliday, cohesiveness refers to a range of possibilities, or semantic resources, for determining meaning--i.e., for linking sentences with what has gone before. Texture in discourse comes through the interpretation of one element of language with reference to another; this is the requirement which enables a passage of discourse to function as a text. Halliday identifies cohesion in a set of distinct categories--reference, substitution, ellipsis, conjunction, and lexical cohesion--which determine different types of cohesive relation and which are associated with particular features of text. (Halliday and Hasan, 1976, pp. 11-13)

Among the choices which speakers make in expressing their ideas are the following: 1) The choice of the active vs. the passive voice, which is often indicative of the locus of agency, as in Grimes' (19 distinction between actors as agents and as props in the context of some action; 2) the choice of declarative, interrogative, or procedural/ imperative modes of expression, perhaps best developed by Longacre (19 is of relevance to the development and implementation of various office procedures and methods of work. 3) The choice of lexicon--Halliday's register--refers to a set of names and descriptions which determine the dimensions or categories of meaning, and hence the parameters of knowledge in any given context. 4) The choice of verbal structure--the grammatical cohesiveness in a text--is entailed in the connective relations which form the basis for inference. Interconnections are embodied in precedence relations which are expressive of temporality, logical embeddedness, and causality. These interconnections are also reflected in referential relationships which link expressions to objects in the environment, to other speakers-and-hearers of information, and recursively to the use of language itself--thus providing the reflexivity in the relationship of systems to their environments which is central to individual and organizational learning and adaptation.

These different characteristics of language-in-use, together with methods of linguistic analysis of discourse as developed by Grimes, Halliday, and others, can be used to investigate the underlying assumptions about organizational structure embedded in the forms of expression which people use to talk about their working environments. As an example of the type of organization analysis which can be undertaken using these tools of discourse analysis, the following pieces of text demonstrate the structure of assumptions defining the office as it is expressed in popular literature, illuminating the context and constraints which determine the success or failure of office automation programmes.

In an older vein, simple content analysis which identifies key words or repeating terms on which a discourse turns can indicate major themes or ideas central to the experience of office work. The protagonist in Joseph Heller's Something Happened describes the office in which he works through several key concepts--suicide, insanity, illnes, and death--with the most frequent reference (13 in one chapter) to death. (Heller, 1974, pp. 14-67) A more involved example of the cognitive constraints reflected in office work is represented in Barbara Garson's All the Livelong Day, which is a report of a field investigation, where participant observation necessarily produced verbal and anecdotal "data". Her interview with clerical workers produced the following representative discourse, which can be analyzed using several techniques to indicate the underlying sense of "order" which office workers experience in their jobs.

Barbara Garson, All the Livelong Day, p. 171

1. "The other day when I was proofreading endorsements I noticed some guy had insured his store for $165,000 against vandalism and $5,000 against fire.
2. Now that's bound to be a mistake.
3. They probably got it backwards.
4. I was just about to show it to Gloria (* the supervisor) when
5. I figured, "Wait a minute".
6. I'm not supposed to read these forms.
7. I'm just supposed to check one column against another.
8. And they do check.
9. So it couldn't be counted as my error.
10. Then I thought about this poor guy when his store burns down and they tell him he's only covered for $5,000.
11. But I figured, the hell with it.
12. It'll get straightened out one way or another.
13. (Interviewer: I must have looked disapproving.)
14. "Listen," (she apologized slightly,)
15. "for all I know he took out the insurance just to burn down the store himself."
16. (Then growing angry,)
17. Goddamn it.
18. They don't explain this stuff to me.
19. I'm not supposed to understand it.
20. I'm just supposed to check one column against the other.
21. If they're gonna give me a robot's job to do, I'm gonna do it like a robot!
22. Anyway it just lowers my production record to get up and point out someone else's error."

(Events)		Instantiation					Environment	Scope = Knowledge Domain (Model)		
	CONTEXT	Participants								
Event		I	They	Some Guy	Obs.	E^r	Setting	Background	Collateral	PLP
1.		x					The other day when I was proofreading some endorsements,			
1a. I noticed some guy had ensured his store for $165K against vandalism and $5K against fire.		x		x						
2.								(Now) that's bound to be a mistake.		Now
3.			x					They probably got it backwards.		
4.		x					I was just about to show it to Gloria (the supervisor) when			
5. I figured "wait a minute".		x								Wait
6.		x							I'm not supposed to read these forms.	
7.		x						I'm just supposed to check one column against another.		
8.			x					(And) they do check.		And
9.									(So) it couldn't be counted as my error.	So
10. (Then) I thought about this poor guy		x								Then
10a.			x					when his store burns down and they tell him he's only covered for $5K.		
11. (But) I figured, the hell with it.		x								But
12.								It'll get straightened out one way or another.		

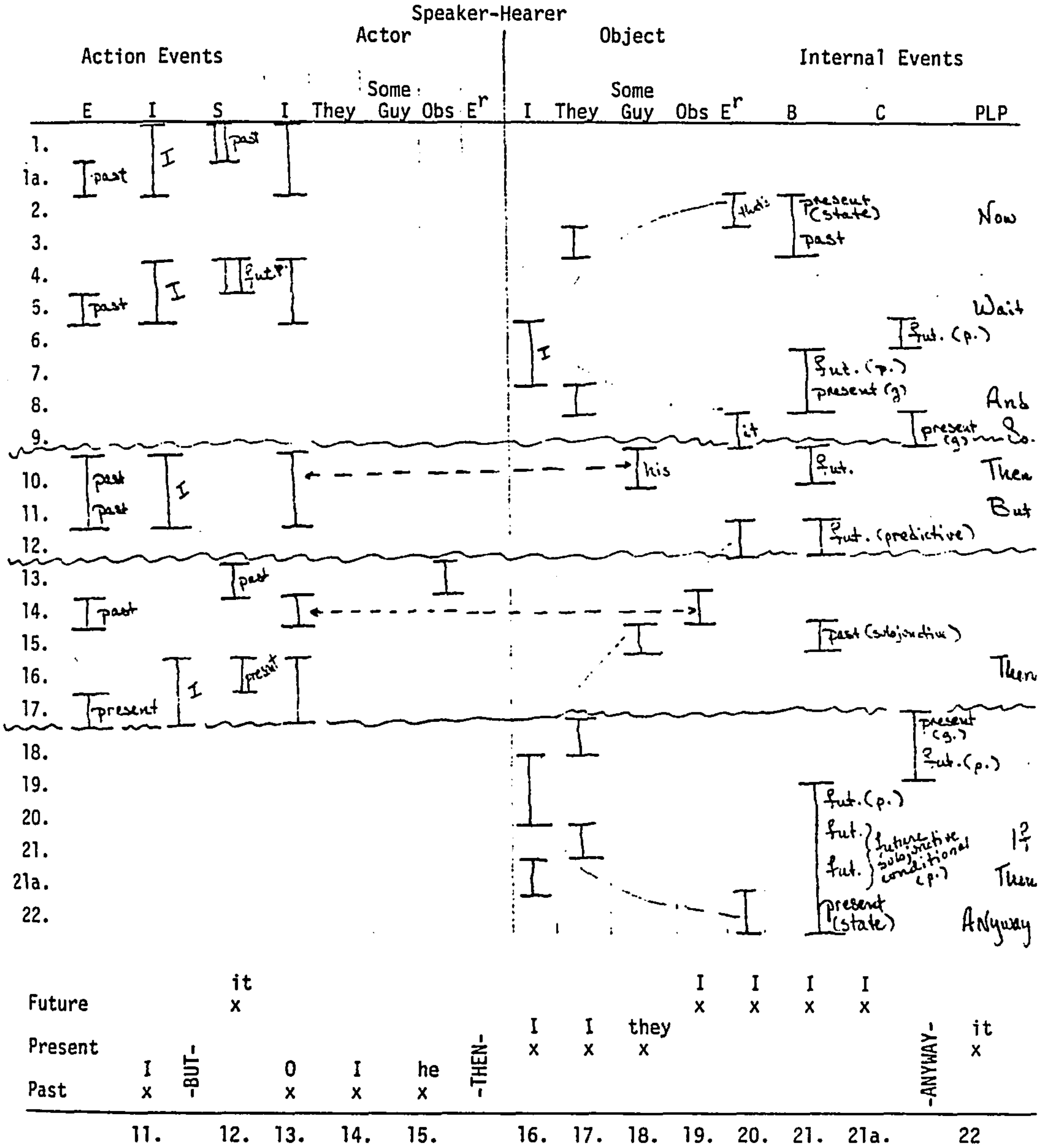
(Events)		Instantiation					Environment	Scope= Knowledge Domain (Model)		
	CONTEXT	Participants								
Event		I	They	Some Guy	Obs.	E^r	Setting	Background	Collateral	PLP
13.					x		[I must have looked disapproving.]			
14. "Listen," she apologized slightly		x								
15.		x		x				"for all I know he took out the insurance just to burn down the store himself."		
16.		(x)					(Then) growing angry			Then
17. "Goddamn it".		x								
18.			x						They don't explain this stuff to me.	
19.		x							I'm not supposed to understand it.	
20.		x						I'm just supposed to check one column against the other.		
21.			x					If they're gonna give me a robot's job to do,		
21a.		x						I'm gonna do it like a robot!		
22.								(Anyway) it just lowers my production record to get up and point out someone else's error.		Anyway

Span Analysis

				PLP			
1.	(s)	I was	past				
1a.	(e)	I noticed	past				
2.				Now	(b)	that's bound	future
3.					(b)	they probably got it	past ?
4.	(s)	I was..to show	future				
5.	(e)	I figured	past	Wait			
6.					(c)	I'm not...to read	future (stip)
7.					(b)	I'm...to check	future (stip)
8.				And	(b)	they do check	present
9.				So	(c)	it couldn't be my...	future ?
10.	(e)	I thought	past	Then			
10a.					(b)	when his store burns	future
11.	(e)	I figured	past	But			
12.					(b)	it'll get straightened	future
13.	[I (observer) must have looked (past)-(s)]						
14.	"Listen," she apologized		past				
15.					(b)	he took...to burn	past (subj.)
16.	(s)	( ) growing angry	present	Then			
17.	(e)	"Goddamn it."	present				
18.					(c)	they don't explain	present (gen)
19.					(c)	I'm not to understand	future (stip)
20.					(b)	I'm ... to check	future (stip)
21.					(b)	If they're gonna...	future (stip)
21a.					(b)	I'm gonna	future (stip)
22.				Anyway	(b)	it lowers my ... record to get...to point	future (stip)

	1.	1a.		2.	3.	4.	5.		6.	7.		8.		9.		10.	10a.
Future				that x		I x			I x	I x							his x
Present												they x		it x			
Past	I x	I x	-NOW-		they x		I x	-WAIT-			-AND-		-SO-		-THEN-	I x	

Sentence Number	No. of Ties	Cohesive Item	Type	Distance	Presupposed Item
2.	2	now	C 5		
	1	that's	R 22.6	0	insured his store for...
3.		they	R 14	∞	(ambiguous)
		it	R 13	0	
4.	1	it	R 13	0	it (...that's...)
5.					
6.	1	these	R 21	M(1)	it (insured...)
7.	(I'm just supposed to check one column against another.)				
8.	3	and	C 11.1	0	(#7)
		they	R 14	∞	(ambiguous)
		( )	E 1 (?)	0	(ambiguous--one column against...)
9.	2	so	C 31.1	0	(#8)
		it	R 13.6	R(4)	(it, #4)
10.	1	they	R 14	∞	(ambiguous)
11.	1	it	R 13	M(1)	(#9)
12.	1	it'll	R 13.6		
13.					
14.					
15.	1	he	R 11.6	N(4)	(poor guy)
16.					
17.	1	it	R 13	M(1)	(#15)
18.	1	this	R 21.6	0	(it)
19.	1	it	R 13	0	(this stuff)
20.					
21.	2	they're	R 14	∞	(ambiguous)
		it	R 13	½	(robot's job)
			or	M(1)	(this stuff)
22.	2	anyway	C 5		
		it	R 13	cataph.	(to get up...)



Speaker-Hearer
Actor
Object
Action Events
Internal Events
E I S I They Some Guy Obs E^r
I They Some Guy Obs E^r
B C PLP
1. 1a. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12. 13. 14. 15. 16. 17. 18. 19. 20. 21. 21a. 22.
Now
Wait
And
So.
Then
But
Then
Then
Anyway
Future
Present
Past
it
I I I I
I I they it
I 0 I he
-BUT-
-THEN-
-ANYWAY-
11. 12. 13. 14. 15. 16. 17. 18. 19. 20. 21. 21a. 22

Cohesion Analysis

Domain is "endorsements": Object is "it"
bound to time and task: "the other day", "proofreading", and supervisor: "Gloria"

		I	Some Guy	It	They	Observer
	1.	I was		endorsements		
	1a.	I noticed	guy insured his store			
NOW	2.			that is		
	3.				[they] got it	
	4.	I was...to show		it		
WAIT	5.	I figured				
	6.	I'm not...to read		these forms		
	7.	I'm...to check	remote & ambiguous	one column against another		
AND	8.				[they] do check (__)	
SO	9.			it couldn't be my error		
THEN	10.	I thought	guy--his store	mediated		
AND	10a.		remote		[they] tell him he	
BUT	11.	I figured		it		
	12.			it'll get		
	13.	strong identity	remote			I must have
	14.	(she) apologized		remote		
FOR ALL	15.	I know	he took..to burn//the insurance..the store			
	16.	(__) growing				
	17.	Goddamn[it.]				
	18.				[they] don't explain this stuff to me	
	19.	I'm not...to understand		it		
	20.	I'm...to check		one column against another		
	21.				[they're] gonna give me a robot's job	
	21a.	I'm gonna do		it		
ANYWAY	22.			it lowers my record to point out someone else's error.		

I refers to actions; [they] refers to procedures

Discourse analysis enables us to identify assumptions, as they are expressed in language, as constraints on the outcomes of development, and indeed on the course of the development process itself. In this view, presumptions of order--especially those shared but unarticulated understandings about role requirements and prerogatives--act to narrow alternatives in the course of development and implementation, in some cases generating or exaccerbating symptoms of stress and conflict in socio-technical systems undergoing change. In a more existential sense having to do with the use of language-behavior in processes of adaptation and organization, language-based approaches to organizational research are capable of reflecting the alienation often experienced in the working population as inhibitions or constraints on self-expression--and hence self-actualization and action.

What is expressed in these examples is the experience of office work as constraining--and alienating. The issue is whether one has a "voice"--or agency--in the events affecting his or her life. In this view, alienation and stress are fundamentally associated with an individual's sense of control over the contingencies--or demands--which are experienced in his or her immediate environment, which suggests that ergonomic problems in office automation are at once a source of and a consequence of implementation problems in otherwise valid system designs.

There is evidence, in fact, that under continuing approaches to office automation over the years, office work is indeed becoming progressively constrained, with potentially adverse effects both for organizations and for individuals working in them. It is a fundamental argument of this thesis that systemic and unresolved problems in implementation--including ergonomic problems experienced by the workforce--may be explained with reference to the narrowness of scope in these traditional models of research and development underlying the design of jobs and organization of work.

The Progressively Constraining Definition of Office Work:

Presumptions of order not only work to the disadvantage of workers' efforts at self-improvement through the vehicle of technological change--both at the managerial and clerical level--but, as we have seen in the work of Whisler and others reporting on the early experience of computerization, they also constitute the basis for negative feedbacks during periods of development in the form of excessive organizational controls which prevent transformations in the definition and organization of work, transformations which are necessary to successful implementation.

The constraining assumptions upon which work-related issues involving the implementation of computer-based office systems are enjoined are, to a great extent, set by designers through the manner in which they conceive and build equipment, and in the ways in which they translate its use to potential users through the definition of various markets and marketing strategies. Beyond the effects of alienation and competition on individuals working in organizations, language-based analysis of organizational models underlying development and implementation suggests that the level of articulation, the scope--breadth or narrowness--of models of organization and problem-solving is associated with the predictiveness of those models, and, by extension, the adaptiveness of the organizations implementing them in their wider environments. Although operations work is typically denigrated in the conventional designer-based style of technological development, and often overlooked in formal systems design (and, indeed in the literature of management, which is biased against machine-based definitions in favor of functional definitions of organizational roles) it is clear that the mundane work associated with "production" and "problem-solving" is also the most direct access to the strategic data representing the organization to itself--the knowledge base on which organizational learning and problem-solving depend.

In task-related terms, this means that at the same time that the ongoing redefinition of office work is limiting the opportunities for mobility among office workers, the formalization of office work through the application of methods engineering and work measurement programmes may be constraining the way these jobs are defined to a level below that which is meaningful in terms of tasks. At the level of the specific definition of office jobs, the identification of office jobs through conventional methods engineering approaches to implementation--upon which recruitment, training, and compensation of office workers is based--is narrower than the role which office workers actually must carry out. This is especially the case when we take into account the additional work-load and problem-solving responsibility which is involved in the implementation process. Even without including the tasks associated with the implementation of change, office work becomes excessively constrained by the routinization of structured communications tasks in a way which is typically limited to those recurring, high-volume activities which are amenable to quantification and optimization. As Simon (1958) and others have noted, unstructured, non-routine tasks are commonly routinized by omission, or by appending them to routine measures--notably cost and time indices.

Formal techniques such as methods engineering and work measurement introduced with the implementation of new technologies constrain the definition of office roles to sets of tasks specified in conjunction with new equipment and methods of use. Such definitions typically only include routine tasks, and further routinize what is in many cases non-routine work, and in so doing constrain the official definition of work to a level below that needed to accomplish the objectives. Thus Diebold agrees that

> "In today's business, most of the clerical force is involved with unstructured information used in decision-making and transmitted by correspondence."
> (Diebold, 1979, p. 52)

At the same time that the definition of office jobs is being progressively narrowed by the application of such design methodologies, the levels of education in the workforce have continued to rise, and in what should be a match between job definitions and workers' qualifications, increasing education in the office workforce is typically being met with increasing limitations on the definition of office work--its scope and variety and discretion. The constriction of the definitions of office work is also associated with categorical limitations on the occupational mobility possible to office workers--both in their physical restriction to a narrow place of work, and in their chances for promotion. In the ongoing development of office technology and definition of office work, this progressive constriction of the definition of office jobs, together with increases in skill and education on the part of the office workforce constitute a volatile context for problems associated with cognitive aspects of work performance and with task-related interdependencies and interactions among office workers, problems directly implicated in the incidence of stress among office workers engaged in processes of implementing change.

In the case of office production systems, the narrowness of formalization is a source of problems having to do with the cognitive aspects of task-related performance and interdependence. These problems arise because the definition of the job of "clerk", and later "word processor", overlooks three important elements of the (secretarial) role: 1) the generation or production of information (by principals) which is a critical element in the measurement of information-processing workload and productivity (of operators); 2) the manual setting up activities associated with otherwise "automated" processes; in computerization this aspect has serious implications for the design of both software and hardware interfaces in man-machine systems; and 3) task-related interdependencies between different jobs in those processes, which involve all those presumptions associated with the access and dissemination of information, presumptions which implicate the structure of interdependencies making up the organized system per se.

We have argued that implementation problems in office automation are fundamentally a function of context, and that element of context which is most important is the definition and organization of work in the organization undergoing technologically-induced change. In introducing new equipment organizations frequently encounter ongoing problems in task accomplishment which are a consequence of dis-isomorphies in the various implicit and explicit expectations as to the component activities making up a given process, and the manner in which they are assigned to persons in that process. In office work the dis-isomorphy of most impact on the success of office automation is that between the implicit definition of the secretarial role and the explicit definition of clerical jobs--definitions which may refer to the same person.

The Changing Definition of Office Jobs: As we have seen in the outline of the work design and measurement methodologies presented in Chapter IV, the "designer-based" style of systems development is a long-running cultural theme underlying the pervasiveness of management science and classical human factors engineering in the development and implementation of computer technology and office automation. We have characterized this style on the basis of its orientation to a "total" rationalization of organizations as production systems, with the criterion value of technical success the ultimate elimination of human labor from that process. Assumptions built into the designer-based engineering of clerical jobs become objectives for purchasing and implementing computer equipment. Among these assumptions are the following:

1) Recruiting and training call for a minimal level of skill on the part of operators and for the least possible training time, an assumption central to the current notion of "user-friendly" systems.

2) An emphasis on cost-effective utilization of equipment underlies the separation of typing and non-typing tasks, indicating an underlying dichotomy between machine-based jobs and those based on the

production and communication of information. Typing jobs are further transformed into production work subject to machine scheduling and physical centralization and networking in machine-dominated environments--word processing and data processing shops--where the traditional amenities of "white collar work" are replaced by office environments increasingly characterized by factory-style working conditions--including durt and noise.

3) The configuration of hardware and the flow of work are mutually determined by the requirements of the equipment used and by traditional assumptions about "who should be where". This means that tasks and work flows are determined by the features and configuration of the hardware viewed in the context of existing data structures and reporting relationships characterizing the organization before the changeover. There is much room for uncertainty in implementation, uncertainty which typically must be resolved by the operator, which makes implementation and changeover highly stressful, particularly as those changes affect work roles and relationships--i.e., implicate organizational interdependencies.

Edwards has argued that the degree of indeterminacy which remains after technological constraints, market discipline, and other factors have been taken into account in determining the labor process constitute a space for working out conflicts in the workplace. (Edwards, 1979, p. 15) We would say that this area of indeterminacy represents a possibilities space for identifying alternative systems, a matrix of alternatives characterized by a shifting and ultimately indeterminate set of possibilities based upon the state-of-the-art in technological design and available information on the part of the organization, and constrained by established structures--methods and objectives--characterizing the organization as an ongoing institution. In this context (in spite of Cyert's (1963) claim that the division of labor is irrelevant) the essential question is "how shall the work be organized"--i.e., what division of labor is to accompany the technological requirements of production. It is Edwards' argument that in the process of working out indeterminacies in the division of labor

> "The labor process becomes an arena of class conflict, and the workplace becomes a contested terrain. Faced with chronic resistance to their effort to compel production, employers over the years have attempted to resolve the matter by reorganizing, indeed revolutionizing the labor process itself...Work has been organized, then, to contain conflict." (Edwards, 1979, p. 16)

The manner in which work is defined and organized in the designer-based style of systems development appears to support Edwards' claim, exhibiting a relationship in which errors and conflicts at the working level--and particularly resistance to the implementation of new methods and equipment--are met by increasing mechanization and automation of the tasks in question, and by extension the downgrading of skills and opportunities for workers in those occupational classifications.

We have seen in Chapter III that the transformation of the contexts for development and implementation brought about by institutionalization and professionalization contradict in a fundamental sense the expectations for personal and organizational learning upon which are based opportunities for occupational mobility. Just as Cullen defined professionalism in terms of an accepted knowledge-based and a corporate organization constituting a recognized occupational group, Watson argues that work is structured in patterns according to two basic principles, which he refers to as 1) bureaucratic or administrative organization based upon the firm as an employer and 2) occupational organization based upon the tasks performed by employees in those firms. A bureaucratic orientation focuses on the design and assignment of tasks by those who control recruitment, compensation, and supervision of others. An occupational orientation emphasizes emergent patterns in the way that work is performed. Occupational categories can be defined on the basis of universal organizational functions, as they often are in conventional organization theories; they are also recognized and codified in considerably greater detail in the employment categories recognized by employers and employees, categories which to a great extent derive from the current state-of-the-art in production technologies and their associated tasks. This recognition is embodied in the emergence of associations and combinations among people performing similar work.

Watson defines occupations in terms of the range of tasks identified under a particular heading or title. The jobs or sets of tasks on which those occupations are identified are defined by a combination of the bureaucratic designation of who a person works for and the occupatonal designation of what the person does. On a broader scale, occupational patterns reflect the division of labor in society. These patterns can be defined horizontally on the basis of employment in the primary, secondary, and tertiary sectors of the conomy; they can be identified with the sites or locations of work in certain industries, such as manufacturing, finance and insurance, or communications; and patterns can be identified, as they conventionally are in organization theory, in terms of the hierarchical structuring of status-based categories of work linking occupations with social class. (Watson, 1980, p. 150)

The definition of jobs and occupations in a designer-based style of systems development tends to emerge unreflectively as a function of conventional practice taken together with the constraints and possibilities presented by different technologies. This process takes place in several stages:

1) The conventional way to define new jobs is to wait until practices emerge and then to perform a research study which describes the activities or workers, using the most proficient employees as exemplars, in the manner of F. W. Taylor and the Gilbreths.

2) The naming of new jobs also follows a period of initial practice, after which the names in common usage are legitimized as they are recognized and published in employment definitions.

3) Once the definitions of work become established in practice, the processes of institutionalization and professionalization proceed to restrict entry through qualifications for membership in given occupational categories, which are specialized in carrying out the further rationalization of tasks and processes emerging with the new technologies.

There is evidence from job descriptions and occupational designations in office automation that the definition of office work is being progressively constricted, while office roles defined in terms of a wide range of tasks necessary to carrying out traditional office functions with new technology may be broadening. At the same time, the percentage of office work in the total employment of the workforce, and the concentration of women in office work are both increasing. This general tendency can be illustrated in a series of developments in office work, from the burgeoning importance of office work following World War I to the transformation of office work via the introduction of computers in the years following World War II.

As innovation and development begin to "settle down" after a generation of intense activity, products are becoming more standardized, conventional procedures and applications are becoming established, and the definitions of tasks and roles in office automation are carving out discernible lines of stratification among office employees. At the same time, expectations for upward mobility in organizations continue to be associated with office work in general, and with the acquisition of technical skills associated with new equipment in particular.

Expectations of mobility during times of technological change in organizations and occupations are based upon the observation that technological breakthroughs provide access to new occupations for people who do not have certificates providing entry to established occupations. Success is largely based on having useful or valuable skills in the early stages of technological development. In the case of computerization, people came into data processing and systems analysis either through training in mathematics or engineering, or by being located at the right place and time, which was the case for many early programmers.

Women were among the beneficiaries of the initial stages of computerization for precisely this reason. They were located in offices, with operational knowledge of the work of their employers--business managers, engineers, and other professionals who were among the first developers and users of computer equipment. Their clerical skills and organizational expertise in the areas of office procedure and records-keeping made them natural candidates for early on-the-job training for programming jobs, and there is still a higher percentage of women in programming than in most other technical professions, suggesting interesting parallels with accounting.

Eames notes that women found a place in this work from the beginning at least in part because such newly created occupations had not yet developed precedents for employing either sex. At the U.S. Army testing ground at Aberdeen, Maryland, 200 women were employed for doing the calculations for different aspects of ballistics problems, and women were prominent among the first programmers on the new programmable calculators and computers--notably Adele Goldstine, wife of Captain Goldstine, who worked on the ENIAC for Aberdeen Proving Grounds, and Grace Hopper, who worked for the Naval Ordnance Computation Project at Harvard as a programmer for the Mark 1, where she developed the original operating programs and pioneered in the development of computer languages. Hopper later worked for the Eckert-Mauchly Computer Corp., where she contributed the first 'compiler' program and assisted in the development of COBOL. (Eames, 1973, p. 135)

In this historical context an interesting and significant social factor in office automation is the durability of the "Horatio Alger" myth of the "former secretary", a theme most recently fueled by word processing breakthroughs on the technical front, made possible by the development of microcomputers. Central to the model of the "former secretary" is the expectation that occupational mobility will come about through technical specialization. A common belief among clerical workers--dating back to the early employment of women in offices and continuing to the present--is that machine-based skills are an entry into clerical--or office--work, and that clerical work is a potential entry into management and administrative positions.

The expectation of occupational mobility through office work was, however, a myth contrary-to-fact as early as the 1920's when the doors to occupational mobility through office work closed as 1) business occupations became "professionalized", increasingly requiring a college degree for entry into managerial and staff positions; 2) as clerical occupations became increasingly mechanized, especially for those based directly on machine-operators' skills; and 3) as women increasingly came to replace men in clerical occupations, which overlapped with increasing concentrations of women in clerical occupations. Thus in striking contrast to the prevalence of the myth of occupational mobility as it has emerged alongside technological innovation and development, real mobility continues to be constrained by traditional office roles, into which innovations in equipment and work design are merely adapted. Moreover, as Watson and others have suggested, what we might view as a structure-preserving adaptation of new technologies to traditional office roles overlaps with and tends to reinforce constraints to occupational mobility associated with traditional roles of women and of machine-operators in the stratification of work in the broader socio-cultural context.

Consistent with the prevailing designer-based style of technological development, stresses currently manifest in office automation reflect a general tendency in the direction of progressive constriction in the definition of office jobs, together with rising levels of education and aspiration among the office labor force, which is increasingly made up of women. While management jobs have, in general, been defined presumptively in terms of function--organizing and coordinating for purchasing, marketing, research and development, and production--these definitions have remained remarkably stable over the years, in spite of changes in technology. Since the introduction of the typewriter toward the end of the 19th century clerical jobs--like production jobs--have been defined more explicitly in terms of the processing of paperwork, and thus their definition has derived more closely from machine technologies used in these processes. In this sense, the collective activities of office machine operators embody the definition of information-processing work as a production process. This is the first step in developing a process-control technology--the essential characteristic of automation.

These transformations can be demonstrated in the definitions of office jobs associated with changing office technologies at several points in the course of the 20th century, and in the changing demographics of office employment during that period of time. Technological breakthroughs and controversies in the definition and organization of office work have occurred at two others points in history--first following World War I in the 1920s and subsequently following World War II in the 1950s--in each case pointing up the changing context for research and development represented in the transference of technologies developed in wartime into peacetime commercial enterprise.

Clerical work can be and has been defined both in terms of the functions of the office, and in terms of the equipment used in performing the tasks associated with those functions. The specific articulation of office work as a production system--defined as a set of tasks rather than a simple function--was largely brought about through the introduction of office equipment--beginning with the typewriter and the telephone--a transformation which coincided with the initial employment of women in offices. In this sense, the first change in office work was brought about by the simultaneous introduction of office equipment, and women office employees. According to Morison,

> "Prior to 1880 very few women were employed in American stores or offices. Salesladies then began to replace salesmen behind the counter, and the lady stenographer with her typewriter, which came into general use around 1895, replaced the Dickensian male clerk with his high stool, calf-bound ledger, steel pen, and tobacco quid...."
> (Morison, 1972, p. 97)

The traditional functional role of the secretary was based on literacy and defined as an adjunct to a decision-maker. The functional role associated with secretarial jobs is still defined primarily as an "assistant-to" a decision-maker. This role entails an ongoing responsibility for performing a range of information-processing activities associated with decision-making and the coordination of production, and for maintaining the office as a central location for producing and communicating that information. Defined in this broad, functional manner, the expectations for secretaries are quite encompassing, for example,

> "Tomorrow's secretary, like the secretary of today, must be punctual in the morning, willing in emergencies to stay until the job is done, and calm under tension...There will be a premium on flexibility as automation brings changes in procedures and personnel, and shifts in policy. Tomorrow's secretary must be ready to embrace change all along the line. The ability to plan work is another skill that will be even more important in the automated office."
> (U.S. Dept. of Labor, 1964, p. 12)

In this Employment Service publication, secretarial jobs entailed several possible routes to occupational mobility. Mobility can take place through specialization to stenographic work--either by shorthand or by machine--with routes to better jobs following either the line from stenographer to secretary to executive secretary, or a line of specialization as a stenographer in a given technical area, such as court reporter, medical or technical or legal stenographer. The executive secretary or administrative assistant position is defined as a management-level position, and on the basis of the full range of tasks subsumed in this broad definition of the secretarial role, secretarial careers were advocated for college graduates who could expect to enjoy higher starting salaries and rapid promotional opportunities, a route by which "...a few college-trained secretaries are able to move into professional occupations." Typists, however, were already finding an increasing proportion of what had been their exclusive work domain subsumed by copying equipment and automatic typewriters. (U.S. Dept. of Labor, 1964, pp. 15-16, 20)

Secretarial jobs have also been defined in a much more instrumental, machine-based manner stemming from the introduction of new office equipment, beginning with the advent of typewriters. Thus Kleinschrod can argue that just as the introduction of typewriters into offices led to the position of the typist--or "typewriter"--today the introduction of word processing technology has led to the introduction of the "word processor" or word processing operator. As is generally the case with machine operators' jobs, the title of the position is named for the machine which is geing operated; thus the person who operates that machine is a "typewriter", the term referring both to the person and to the equipment used by that person.

The derivation of job definitions from the equipment used, both in identifying job categories and in developing training requirements and educational programs, serves to eliminate the potential for mobility which might have been associated with the wide range of responsibilities prior to the rationalization of the job into a bounded set of tasks. That rationalization typically comes about through the identification and codification of new categories of employment based on the emergence of standard practice as a function of the introduction of new equipment.

A New Conception of Office Practice reports on a study conducted in 1923 and 1924 by the Harvard University Graduate School of Education in conjunction with the National Association of Office Managers. The objective of this study was to identify the training needs for clerical work--taking into account the new equipment then being introduced into offices, and the emphasis on quantification and measurement which emerged during this period. This investigtaion, carried out with the cooperation of a number of businesses, provided definitions for emerging clerical occupations, focusing especially on machine-operators' jobs with a view toward determining the skills which should be acquired by clerical workers through "commercial education" at the secondary school level.

The authors of this study note that their survey of the activities performed by clerks in these businesses demonstrates that clerical work is by nature diversified. This is desirable, they argue, because performing a number of diverse activities avoids monotony, which is associated with reduced efficiency. For this reason, they argue, clerical jobs can be thought of as "commercial trades" analagous to industrial trades only in the most restricted sense, and they recognize that job names largely reflect not unique tasks but convenient payroll categories.

This study arrives at three conclusions and recommendations which are highly significant for the mobility of office workers. First, they argue that because so few employees actually advance from clerical to stenographic to secretarial occupations, and because office work is so diverse, training in bookkeeping and stenographic skills is superfluous for most office employees, who should instead be specialized to formerly neglected clerical occupations, namely dictating machine operator, calculating machine operator, duplicating machine operator, file clerk, and cashier. (Nichols, 1927, pp. 13-14)

The second area of interest concerned the potential for occupational mobility through office work. The authors of the study addressed this question to employers and workers, asking both groups the following question indicative of the transformations in business organization taking place during this era:

> "When businesses were smaller, it was possible for a clerical apprentice to learn much about business management, finance, transportation, and office procedure of all kinds; in short, "to learn the business". Is there the same opportunity for such all-round training in your office today?" (Nichols, 1927, p. 92)

Opinions as to potential mobility were divided among office workers; those working in certain occupations such as correspondents, dictating machine operators, bookkeeping machine operators, multigraph operators, statistical clerks (indeed, all manner of clerks specialized to different functions) all believed their present jobs held opportunities for advancement. Those engaged as typists, file clerks, adding machine and listing machine operators, addressograph operators and hollerith machine operators indicated no opportunities for advancement from their present jobs. (Nichols, 1927, p. 92)

The opinions of office managers agreed with that of office workers who believed that the opportunity to gain all-round knowledge of the operations of the business was "greatly reduced". The reduction in opportunity to learn the business on-the-job derived from the departmentalization and specialization taking place in organizations during this period, which required supplementary training of a "non-skill" nature in order to advance beyond the clerical level. Thus the conclusion of this study was that

> "Not all clerical workers need be potential executives. Clerks who will be satisfied to continue as clerks are needed." (Nichols, 1927, p. 30)

The mobility which was believed to lead from clerical work through stenographic work to secretarial work and ultimately into managerial positions was, by 1927, acknowledged to be merely an illusion centered on the notion of clerical work as a stepping stone as held by clerical workers themselves. The reality even then was that advancement beyond secretarial work required further non-skill training, and conversely, that for those not having these higher qualifications and not having bookkeeping or stenographic skills, clerical work had become permanent employment without possibility for promotion. At this point it became necessary to identify those employees suited for such jobs, and to investigate the degree to which those jobs could be seen to provide an adult living wage. (Nichols, 1927, p. 30)

Finally, this study reported and to a certain extent reified the observation that clerical work was by 1927 coming to be essentially a women's domain, with implications for distinguishing the training of boys and girls to men's and women's trades. The authors observe that as the employment of females in clerical work outnumbers that of males, "there is little, if any, clerical work for which boys are preferred... (suggesting that) boys in the near future may find it necessary to enter business through some other channel. (Nichols, 1927, p. 78) On the other hand, office boys stand on the "ladder to management", which in this era begins by "teaching office boys their particular job" under supervision which acquaints him with the office and with other office boys through sorting and filing of papers and performing odd jobs. Then

> "As he grows he works naturally into the job of the junior clerk, or if he is more mechanically turned, he will be directed into some line which is more along his natural bent." (Western Efficiency Society, 1917, pp. 66-67)

Thus boys were separated out of clerical work early on, largely reflecting the increasing proportions of girls employed in office work rather than any concrete differences in aptitudes or skills.

With the continuing automation of office work brought about in the post-World War II period by the introduction of computers into organizations, the emphasis on procedure analysis increases, with similar objectives of determining the requirements necessary for office employees working with new equipment. According to Becker and Murphy, writing in 1957 on The Office in Transition, office work has been transformed by the advent of computers; where employees once were selected on the basis of a set of job requirements comprising a job description, the introduction of computers involves two kinds of job description, one describing the tasks of people doing the manual elements of the work, and a second job description--or program--which is written up for the machine. (Becker and Murphy, 1957, p. 97)

The justification for the introduction of new machines is that companies expect to save money by cutting down on personnel. The implication, Becker and Murphy argue, is that procedure analysis is essential to successful implementation, and that significant resistance to the introduction of equipment is to be expected. Procedure analysis is directed to identifying "paper work problems" with the objective of locating those areas where mechanization may be most profitably installed. Those tasks most amenable to mechanization included clerical work involving repetitious copying of information, lengthy calculations, multiple sortings, and large volumes of recurring work. Having identified such areas of potential "pay dirt" the strategy is to apply a "standardization formula" to each step of each operation of each transaction. (Becker and Murphy, 1957, pp. 57-60)

Implementation takes place through the explicit definition of new office occupations and the drawing up of organization charts showing definitions for job duties and responsibilities, prerequisites for employment in various jobs, indicating the relative positions of these jobs in the promotional line, and specification of required skills and training. (Becker and Murphy, 1957, p. 50) The study of office work and the definition of new office occupations is directed by the need to utilize expensive, high-speed operating machines to their fullest extent, an objective which requires standardization of the work. For this reason, Becker and Murphy argue, more than half of the job of automation involves procedure analysis. And in this endeavor, analysis must be conscious of human relations in recognizing and overcoming resistance to change on the part of operating employees who "...cannot be expected to welcome the systems man, the methods man, or in some cases, the outside consultant, with open arms." (Becker and Murphy, 1957, pp. 143, 145-147)

In this study, just as in the Nichols report of the Harvard study of office occupations in the post-World War I era, the activitiy of investigating the nature of office jobs and their interconnections serves to reify and codify those definitions in recommendations and qualifications for employment in newly emerging occupations. Becker and Murphy represent one of the earlier accounts of office automation which associates enlarging the activities associated with a given job or occupational category with upgrading of employment, in spite of the fact that skills and training do not change. Thus they argue that office automation favors the employment and hence occupational mobility of older women, whose maturity is a preferred quality in mechanization, citing the increasing employment of older women in banks as evidence in support of this claim. This upgrading of employees takes place by lowering the skills and educational levels required for performing the work, which is brought about largely by implementing equipment which subsumes those skills. Thus the introduction of electronic calculators has made it possible to assign work to clerks which had previously been performed by accountants; in similar fashion, the "care and feeding" of electronic computers will lead to a new profession made up of analysts, programmers, operators and maintenance engineers. (Becker and Murphy, 1947, p. 100)

This cost-driven, machine-based orientation to the implementation of new technology and derivation of new jobs and occupations was well in place by 1964, by which time it was clear that the occupations of office machine operator had become distinctly different from that of secretary--especially with respect to occupational mobility. Office equipment had been developed by World War II for nearly every clerical task, with tasks defined by the equipment used and the equipment in use generally designated in the job title and qualifications for employment per se. (U. S. Dept. of Labor, 1964, pp. 38-39) This characteristic is especially noticeable in advertisements for data processing jobs.

By the 1950s it was noticed that not only did changeover involve significant resistances on the part of employees, but that the nature of working conditions and opportunities for pormotion in office machine operators' jobs had come to resemble factory work in many respects--further distinguishing these occupations from traditional secretarial jobs. A major reason for these distinctions in employment arises from the cost-justification of the equipment, and by extension the organization of labor required to operate that equipment most efficiently. Thus

> "The high cost of office machines often gives rise to shift work and a pressure for speed in order to make the most efficient use of equipment."
> (U.S. Dept. of Labor, 1964, p. 43)

The transformation of office work in the interests of operating efficiencies in equipment was associated by this time with the experience of "factory atmospheres" in which noise, rapid working rhythms, and physical labor involved in machine operation lead to "extreme fatigue". Coupled with the keeping of records on the output of machine operators, these factors combine to make the changes disturbing to employees, and thus to raise dissatisfactions with office work, which was already strained, beyond limitations in opportunity.

Perhaps the greatest transformation in office work during the post-World War II period was the separation of data processing jobs from clerical jobs, on much the same rationale as the separation of bookkeeping from clerical work in the 1920's. Prior to the introduction of computers and electronic data processing equipment, data-processing tasks were performed by people using "electro-mechanical data-processing" methods which were in continuous use since they were introduced in the 1880's by Herman Hollerith of the Census Bureau.

The 1880 census of 50,0-0,000 people had taken 9 years to compute, and the expectation was that given a rise in population, the compilation of the 1890 census would not be completed before the count was to be taken in 1900. Following the introduction of the "Hollerith" machine, it took only two years to tabulate the data for 62,000,000 people. (U.S. Dept. of Labor, 1959, pp. 1-2)

Data processing throughout the 1950's and 1960's largely separated computer-based operations out of offices and located them in centralized data processing shops. However, technological breakthroughs in the size and cost of computers, together with the capabilities of new generations of computers specialized for word processing and other applications, once again constituted a breakthrough sufficient to alter the definitions and qualifications for information-processing jobs. With the division of clerical staffs into machine-operators and secretaries, and with the subsequent division of secretaries into word-processing and administrative secretaries, clerical work was increasingly polarized, with a signficant proportion relegated to machine-based technical occupations, which could be removed from administrative offices as a place of work.

Moreover, the definition of those jobs increasingly constrained entry and mobility as qualifications became established in use. In 1959 the U.S. Employment Service, in cooperation with business machines manufacturers and the National Science Foundation, published a set of 13 job descriptions and qualifications for Electronic Data-Processing Systems occupations, based upon a broader job analysis study of "technician occupations". Noting that in 1959 the basic jobs in data-processing were still fluid and that the requirements for entry into these occupations had not been standardized, the descriptions which were developed referred to composites of jobs and not necessarily to specific job titles in particular organizations. The names for the specific jobs themselves reflect common usage built into the working out of job titles. (U.S. Dept. of Labor, 1959, pp. 3-4)

Data processing jobs in 1959 were divided into three categories, based upon the sequence of steps typically involved in the processing of data at that time: 1) Project Planning, carried out by the "Project Planner" required an individual with a college degree in mathematics or business administration. 2) Project Formulation, consisting in Systems Design and Programming, was carried out by the "Computing Analyst", the "Systems Analyst" and the "Chief Programmer", all of whom must have a college degree in mathematics or a closely related field. The Chief Programmer supervises Programmers, who also must possess a college degree in mathematics; the "Coding Clerk" and "Tape Librarian", however, need only be high school graduates. 3) Production occupations center on the physical operations of the computer system and are carried out by the "Supervisor of Data-Processing" and "Console Operators", "High-speed Printer Operators", "Card-Tape-Converter-Operators", "Data Typists", and "Electronic Mechanics, Computer". All of these occupations require a high school diploma, and the electronics mechanic position is generally unionized. (U.S. Dept. of Labor, 1959, pp. 6-7)

Several observations can be made about these definitions. First, they are tied directly to the state-of-the-art machine technology at the production level, as reflected in the names of the occupations--consistent with the names of office-machine operators' jobs, they refer directly to the type--and even the manufacture--of the machine-in-use. Second, machine-based training in programming and operating computers is required but not specified in the position of Systems Analyst; however, the educational requirements for Computing Analyst and Project Planner specify a broader range of knowledge than do typical machine-based occupations--including in addition to, or in lieu of mathematics, education in the physical sciences, engineering, and/or business administration. The requirements do not specify experience in or knowledge of programming or computer operations. The common thread tying these alternative qualifications together is a knowledge of calculus and differential equations, which knowledge amounts to a certified "skill" which itself constitutes a dividing line between programming and planning occupations.

Thus when considering channels of mobility, strictly on the basis of the definition of various job categories, we can see that occupants of production jobs can only advance into programming and systems analysis and project planning occupations by acquiring a college degree in mathematics or a closely related field. However, in the early stages of computerization, nearly all of the occupants of those positions entered the field by virtue of experience, and this fact is reflected in the content of the job descriptions themselves during certain phases of the development process. The 1959 data-processing definitions show this alternative route to entry into new occupations. For Project Planner,

> "...(W)here sufficient experience is found to qualify individuals, employers may waive academic training."
> (U.S. Dept. of Labor, 1959, p. 22)

For Systems Analyst,

> "Some employers consider 3 years of generalized experience in administrative, professional, technical, or investigative work, and 1 year of specialized experience in organizational analysis, work-flow planning or work simplification and improvement as a substitute for part or all of the educational requirements."
> (U.S. Dept. of Labor, 1959, p. 26)

For Computing Analyst,

> "Some employers accept less formal training if the individual possesses significant data-processing experience." (U.S. Dept. of Labor, 1959, p. 12)

And for Chief Programmer,

> "Some employers reduce the educational requirements by substituting a specified number of additional years of qualifying experience." (U.S. Dept. of Labor, 1959, p. 21)

Previous experience in greatest supply at this time was that of the industrial engineers and psychologists, and business administration specialists trained in work simplification and work measurement analysis, and in this way these perspectives were built into the initial definition of computer-based occupations, particularly that of systems analyst--i.e., those people charged with interpreting machine requirements in terms of organizational objectives and procedures. Persons holding Project Planner and Systems Analyst jobs must be qualified in the design of organizations, procedures and work flow analysis according to management analysis methods. Thus a repertoire of formal models and methods is the entry into the computer professions, while clerical work is the entry into computer operators' positions--and increasingly so as the machines become more sophisticated and capable of being operated by untrained personnel.

The 1970s, which were a period of relative organizational queiscence, with little outward evidence of significant structural transformations or conflicts save those associated with budgetary retrenchments, saw qualitative leaps in the technological capabilities--and technical capacities--of computers, partly achieved through breakthroughs in micro-processor technology. This qualitative increase in technological capability made it possible to overcome technical limitations impeding the progress of office automation--particularly in its as-yet-undeveloped interactive inquiry mode--through the implementation of mini- and micro-computers specialized to word processing applications.

The development of micro-computers and word processors involved two relative advantages over large, expensive mainframes: 1) Microcomputers were relatively low-cost and thus represented a low-risk investment for firms wishing to enter the "information age". 2) Because the costs of experimenting were lowered, microcomputers had the further benefit of permitting accessibility and use of the equipment by untrained employees, without dependence on--and domination by--technical specialists-cum-change agents in data processing shops. These advantages made it possible for organizations to acquire and implement computer technology without undergoing significant transformations in organizational control structures, thus avoiding disruptions in working arrangements associated with earlier implementations. "Word processing" was expected to permit broad accessibility and ease of use to a wide range of organizational users, and thus to bring about increases in productivity and at the same time to facilitate the upward mobility of organizational users--especially data processing specialists and clerical workers.

Data processing technology was extended back into office work via the introduction of word processing as the most widely used among a number of standard, or packaged, applications developed for small computers. The characteristics of word processing involve a reconfiguration rather than a reconceptualization or redesign of the basic technology, making use of essentially the same hardware as data processing. The notion of "word processing" is thus largely defined as an outcome of choices made by the manufacturers of computer equipment in the features to be included in hardware design and software development, and in various marketing strategies underlying implementation in different environments. According to Champine, word processing represents the identification of a market segment based upon a common set of applications, flexibly presented through software developments attached to the provision of relatively inexpensive, interchangeable small-scale computers. (Champine, 1978, p. 204)

Marketing strategies adopted by the computer industry in developing data processing and word processing technology have reinforced traditional office roles by producing a series of applications of a "structure-preserving" nature: 1) specializing clerical work on the basis of typing tasks into word processing secretaries, who are basically machine operators, and administrative secretaries, who carry on the traditional functions of secretaries; and 2) updating and reinforcing the traditional secretarial role in marketing concepts such as IBM's "administrative assistant" and Diebold's "decision support system".

The implication in each case is that the "enlargement" of secretarial jobs through the implementation of computer-based office equipment--and with it the acquisition of the requisite "technical" skills--is an avenue of mobility which will professionalize office work.

The new categories of employment added to civil service classifications do not reflect "professionalization" of the work, in the sense in which Cullen and others define professionalism, however. In 1975, Kansas Civil Service specifications distinguish clerk-typists from word processing operators on the basis of the specialization of the work to machine technology. Clerk-typists--the traditional clerical category--work at skilled typing duties in both established and moderately complex work methods and problems. While the variety and difficulty of the work varies, clerk typists have the responsibility for the "finality of action" in relatively repetitive work. Word processing typists, on the other hand, are specialized to the operation of automatic typewriting equipment, and the work "involves the responsibility for the operation of an electric typewriter and auxiliary console devices". The definition of word processing jobs focuses on the equipment used rather than on the task performed, in the manner of the clerk-typist's duties.

The responsibilities of word processing supervisors are directed to setting up and directing the operation of word processing centers, including management of the equipment and scheduling of work. The position of word processing supervisor can advance two levels, the highest involving "highly responsible supervisory and technical work" in directing the operation of large word processing centers, through analyzing work requests and identifying necessary methods to fulfill them using available technology. (Kansas Civil Service, Publ. #00-00-1-041, 00-01-2-114, July, 1975)

For none of these positions does the requirement for education exceed graduation from high school or completion of the GED. According to Tenopyr, the jobs created by the advent of mini- and micro-processor technologies should not require as much formal education, which "should enhance the ability to place those who are educationally disadvantaged". However, she notes, it may also be the case that it will become "increasingly difficult for the educationally disadvantaged to gain the training and experience necessary to compete for higher level jobs." (Tenopyr, 1981, p. 42) Ellis notes that the skills required of word processing secretaries include the ability to enjoy typing and an interest in machines, together with the ability to work with others in a team or group arrangement in "centralized correspondence centers". In fact, the requirement beyond basic skills in English grammar and usage which is most important for word processing operators is the ability "...to sit at equipment for long periods of time...". (Ellis, 1980, p. 26)

The argument in favor of increasing professionalization of clerical work is based upon this machine-based specialization and organization of work. Kleinschrod argues that secretaries did not really exist in the traditional organization as individuals in their own right. Their contributions were subsumed under those of the executives for whom they worked, with whom they identified, and who they depended upon for promotion. Word processing, in contrast, provides a clear-cut definition of secretaries' positions, identifying specific steps on a career path in which they work for the organization, and not for individuals in it, and in which their raises are tied to their own efforts. This specialization puts secretaries on the organization chart, with mobility deriving from specialization, which "...provides many career opportunities and enrichment activities that secretaries are demanding." (Kleinschrod, 1980, pp. 5-10)

What Kleinschrod is implicitly criticising is the traditional functional definition of secretarial work. Like Kleinschrod, Ellis criticises the traditional secretarial role as a "jack-of-all-trades", arguing that the assignment of secretaries to managers on an individual basis leads to a duplication of work and prevents work sharing among individuals and departments. (Ellis, 1982, p. 6) According to Kleinschrod, word processing means professional specialization. He argues that this professionalization reflects the fact that where once clerical workers were supervised by executives, they are now supervised by clerical professionals, thus carving out the new "profession" of word processing supervisor. Ellis predicates opportunities for advancement in office work on the growing need for supervisors and managers for word processing offices, who "...in many instances...will be former correspondence specialists and administrative secretaries." (Kleinschrod, 1980, pp. 5-10; Ellis, 1982, pp. 37-38)

Thus for secretaries, word processing in the current mode means professional specialization. To managers and executives, it means cost savings, achieved by producing communications which are high-quality, error-free and standardized, in the process organizing the work so as to avoid unnecessary duplication, increase employees' efficiency by specialization and by even distribution of work loads. For both Ellis and Kleinschrod, as representative of the conventional approach to contemporary office automation, the costs of word processing equipment are justified by pointing out that this specialization of secretarial work enables machines to be more fully utilized by "specialists who typed most of the day." (Kleinschrod, 1980, pp. 5-10; Ellis, 1982, p. 6)

It may be argued that the specialization of secretarial work based on typing and non-typing skills, and the organization of clerical work in production systems which break down the traditional secretarial functions and relationships to management do not necessarily constitute either a professionalization of office work or an avenue of occupational mobility for office workers. There is no line of advancement beyond word processing supervisor for word processing "specialists", and while the job of administrative secretary retains all of the functions of the traditional secretarial role save typing--including resolving many of the uncertainties involved in implementation and use of computer-based technologies in offices--there is no necessary implication that administrative secretaries will move into lower level managerial and professional positions in the organization. It is more likely that, ever the bridesmaid, the "assistant-to" may be on the forefront of the implementation of new technology time and again without significantly improving his or her status in the organization--and in some cases actually undercutting one's present position.

The fear of undercutting one's position in the organization through the implementation of computer-based technologies underlies the early resistance to computerization on the part of middle managers, who as Whisler and others have noted, were responsible for developing and installing the very changes in tasks and working arrangements which were expected to diminish their authority and to make their positions redundant. (Whisler, 1970, p. 7) In the case of secretaries, there are indications that the actual professionalization of office work (which is reasonable when the complexity of the tasks and the level of education of the workforce is considered) is actually blocked by the continuing automation of clerical work and the specialization of office jobs, and by the increasing concentration of women in clerical work, a demographic phenomenon which has accompanied the machine-based specialization of clerical tasks over the course of the 20th century. The increasing concentration of women in office work, together with the machine-based definitions of office work with their categorical limitations on advancement, combine to produce the experience of office work as progressively confining.

The Changing Demographics of Office Jobs: Technological developments in office work have been associated with the continuing, and apparently irreversible employment of women in the labor force, especially in the U.S., where the proportion of women employed increased significantly during both World War I and World War II, and did not decline as expected in the post-war years, in spite of fairly direct attempts to discourage them. After each war, there were more women working, and more women working longer periods of their lives than ever before--thus calling into question the traditional assumptions which portrayed the typical office worker as a young girl, seeking temporary employment before marriage and family. These assumptions persist today, in spite of their lack of realism. However, the proportions of women who work, and of women in the total labor force have continued to rise throughout this century, as the following table shows:

	Women as Percentage of Total Labor Force	Percentage of Women Aged 16 and Above Employed Full-Time
1890	17.0%	18.0%
1900	18.0	20.0
1910	21.2	23.4
1920	20.5	22.7
1930	22.0	24.0
1940	24.6	25.8
1950	28.8	33.9
1960	32.3	37.8
1970	36.7	43.4
1978	48.4	50.8

(U.S. Dept. of Labor, Women's Bureau, 1932, 1956; U.S. Dept. of Labor, March, 1979)

Occupational mobility can be defined in more than one way, as is clear from the trend displayed above. We can refer to the entry of persons or groups into the work force in terms of gainful employment per se as indicative of occupational mobility. We can then focus on the movement of individuals and groups from one occupational category to another as indicative of occupational mobility between categories of employment. Horizontal mobility means the movement of employees from one job to another at the same level of skill and compensation, while vertical mobility implies promotion (or demotion) from one level of occupation to another level above or below it, as defined in terms of requisite skill and compensation. In these terms, a demographic review of the composition of the office workforce reveals several characteristics of relevance to office automation efforts:

1) Clerical work is increasing in proportion to other occupational categories in the total workforce, and as a proportion of the operating costs of organizations. It has been observed that a characteristic of advanced industrialization is an increase in the proportion of white collar workers in the labor force. In both Germany and the U.S. from 1890 to World War I the number of white collar, or salaried, employees increased at a rate almost double that of industrial wage earners, or blue collar workers. By 1925 the proportion was one white collar worker for every blue collar worker. (Kocka, 1980, pp. 93-94) Between 1900 and 1960 the number of clerical workers in the U.S. increased tenfold, constituting a majority of white collar office jobs by 1920, and rising from the smallest occupational group to the largest in 60 years. (U.S. Dept. of Labor, 1964)

2) Clerical work is increasingly being performed by women, who are increasingly concentrated in clerical occupations. According to Kocka and others, very few women were employed in offices in 1880; by 1900 three-fourths of all stenographers and stenotypists were women, and during the period 1920-1929 the proportion rose to nine of every ten women. (Kocka, 1980, pp. 99-100) In terms of gainful employment,

the total number of women employed in the workforce increased by 25.8% from 1920 to 1930, and women as a percentage of all employed persons increased from 20.5% to 30.5% during the same period. From 1940 to 1968 women accounted for approximately 65% of the total increase in the labor force, raising the proportion of women employed from 25% to 40% of all workers by 1968. In 1978, women represented nearly half of the U.S. labor force, and over half of all American women over the age of 16 were gainfully employed. (U.S. Dept. of Labor, 1932; 1969, p. 5; 1979, p. 2) This phenomenon is not limited to the United States, as in a number of countries the participation of women in the labor force has passed 40%, among them England, Sweden, and Finland; the Soviet Union and most of the Eastern European countries; Nigeria, Ghana and other West African countries; and the countries of Southeast Asia, including Vietnam, South Korea, Thailand and New Guinea. (Kidron and Segal, 1981, p. 41)

The number of clerical workers doubled again in the post- World War II period, and the concentration of women in clerical positions has continued to increase since that time, the proportion rising from 53% in 1940 to 67% in 1956. By 1950, 94% of all stenographers, typists and secretaries were women, as were 95% of all telephone operators and 82% of all office machine operators. In 1960, two-thirds of all clerical workers were women, and among secretaries, typists, stenographers, receptionists and attendants, and telephone operators, 95% were women. In addition, certain jobs--such as file clerk, bookkeeper, cashier, library attendant or assistant, office machine operator, and bank teller, which were predominantly filled by women in 1960, show a qualitative shift in the proportions of men and women over the preceding decade. Bank teller's jobs were primarily filled by men in 1950, but by 1960 women bank tellers outnumbered men, increasing by an average of 211% as compared with a 12% increase for men. (U.S. Dept. of Labor, Women's Bureau, 1956, pp. 8-9; 1964, p. 14)

The number of women employed as managers and proprietors doubled from 1940 to 1950, rising to over 1 million by 1958; however, this increase did not alter the proportion of women employed as managers, which remained at approximately 17% of the total. Moreover, half of these women were proprietors of their own businesses, rather than managers of corporate departments, while half were salaried workers. In 1974, while 62% of all employed women were white collar workers, 34.9% were employed as clerical workers, 15.5% were professional and technical workers, and a mere 4.9% were employed as managers and administrators. (Hunt, 1979, p. 8)

3) Clerical work is progressively displacing professional work in the total employment of women. Kocka notes that the development and increasing significance of professionalization was "...one of the most important social changes occurring in the U.S. around the turn of the century". In keeping with Cullen's and Watson's definitions of professionalism, Kocka understands professions to be defined in terms of 1) specific competencies and specialized education; 2) numerical increases in and corporate organization of professional groups controlling entry largely through formalized university degrees; and 3) rising claims to status and income--based on expertise as an alternative to ownership as a basis for social status. (Kocka, 1980, p. 46)

Defined in these terms, Kleinschrod's and other word processing consultants' definitions of office work as "professional" based on task specialization imply a highly restricted definition of the concept, and indeed, the General Counsel for the Wage and Hour Division of the U.S. Dept. of Labor provided an interpretation of the Fair Labor Standards Act of June, 1938, which specifically exempted secretaries, bookkeepers, and stenographers from this occupational class, stipulating that they are "...not employees engaged in a professional capacity..." even where they worked as private secretaries to executives, unless they were engaged in delegating to or supervising the work of other secretaries. (U.S. Govt., 1949, p. 28) Only the supervision of clerical work carries with it professional status, which is thus narrowly defined in the absence of any special educational qualifications or membership in professional organizations.

Interpretations of the changes in women's employment following World War II vary on the issue of professional work, sometimes within the same publication, if one compares women's occupational mobility into different occupations and professions, with women's distribution in those occupations. Thus the Bureau of Labor Statistics reported in 1969 that

> "The number of women professional workers more than doubled over the 18-year period (from 1940-1968), illustrating the rising demand for workers with higher educational achievement or specialized skills."
> (U.S. Dept. of Labor, Women's Bureau, 1969, p. 91)

Among professional workers, however, the proportion of women employed in the professions declined following World War II, both in absolute numbers and in relative rates of growth when compared with those of men. Women represented 45% of all professional workers in 1940, but only 37.3% in 1969. Professional and technical workers was the only major occupational category in which women's participation decreased in the period from 1940 to 1968, in spite of increasing numbers of women employed in the professions, which rose from 1,570,000 to 4,022,000 during this period. (U.S. Dept. of Labor, 1956, pp. 8-9; 1969, p. 92)

Which occupations to include when identifying women professionals is itself an interesting question. In 1950 75% of all school teachers and 95% of all nurses were women, thus effectively defining these occupations as "women's professions". Although the number of women in the professions increased by approximately one-third from 1940 to 1950, the proportion of women in the professions declined, and the bulk of the absolute increase was concentrated in teaching and nursing, with the remainder distributed through the occupations of musician and music teacher, technicians in medical and dental offices, accountants and auditors, social workers, and librarians. (U.S. Dept. of Labor, 1956, pp. 8-12) One caveat which must be included when looking at professional employment of women is the reification of sex-linked occupational designations through the definition of occupational categories per se. Although women are represented throughout the professions--albeit in very small numbers and percentages--only classroom teaching and medical and other health workers (nurses predominantly) were counted as categories of professional employment, thus accounting for the overwhelming focus on these two professions.

Moreover, although 42% of all professional women in 1968 were non-college teachers, even in teaching the proportion of men increased slghtly over this 18-year period. Women did enjoy significant gains in several of the newer professions, and in traditional professions such as engineering which opened to the employment of women in this period, largely due to the tremendous expansion in research and development following World War II. However, although the employment of women in industrial engineering, mathematics, aeronautical engineering, personnel and labor relations, and public relations doubled from 1950 to 1960, the total number of women engineers and scientists did not increase appreciably during this period, and the proportion of women holding these positions remained small. (U.S. Dept. of Labor, 1969, pp. 94-100)

Finally, when studied in conjunction with the level of educational achievement, the displacement of professional work by clerical work in the employment of women over the long run is even more striking. In 1956, 70% of all women college graduates and 30% of those finishing one to three years of college were employed in professional and technical occupations. At the same time, among women clerical workers, 45% had completed 4 years of high school, 35% had completed one to three years of college and 16% were college graduates. (U.S. Dept. of Labor, 1956, p. 51)

Among women professional and technical workers in 1969, 79% had attended college and 58% were college graduates. By comparison, 21% of all women clerical workers had attended college in 1969, while 75% had attended high school and 66% were high school graduates. Consistent with the reification of clear distinctions between professional and clerical workers in the definition of employment categories which are otherwise clearly overlapping, the number of clerical workers who were also college graduates was not cited in this 1969 Labor Dept. publication; in fact, the category of "education" is only defined at three levels for clerical workers: 8 years or less schooling, 1 to 4 years of high school, and 1 or more years of college. (U.S. Dept. of Labor, 1969, pp. 212-213) In 1976, 67.8% of all women employed as professional, technical and kindred workers had completed 4 or more years of college; in comparison, only 16% of all clerical workers were college graduates, but 46.7% had completed one to three years of college. (Hunt, 1979, p. 18)

4) Finally, within clerical work, secretarial work (including that of stenographers and typists) is increasingly being defined as qualitatively different from and separated from the occupations of office machine operator--both in terms of the definition of the role and its associated tasks and in the opportunities for advancement associated with these roles. Increasingly, and especially with the advent of word processing, typing has been defined as a machine-based skill, and typists' jobs and those aspects of secretaries' jobs which involve typing, as well as the operation of other office machines, are being separated out of the definition of secretarial roles.

Just as in the emergence of standardization and measurement in procedure analysis and job definition in the introduction of early office equipment and continuing to the present, the conventional mode of definition of office jobs for word processing applications presumes that the definition and organization of tasks begins with measurement. The definition of word processing jobs--like those of other machine-operators--is read off the characteristic features of current technology. Activities are measured, as we have seen, by counting lines of text and keystrokes, reinforced through selection and training on the familiar designer-based model of instrumental definition of jobs. The characteristic limitation in this mode of defining jobs is that the definition of the job is so specific that it cannot be adapted to other modes of work, and includes no means for changing the nature of that definition. Moreover, the definition of jobs in the designer-based mode of development is tied so tightly to the state of the art of some technology that not only does it not provide for ongoing occupational mobility, but this means that such jobs are often the first to be eliminated in subsequent phases of development.

The specialization of office jobs to new equipment, and the development of "user friendly" machine systems can be translated as a means for eliminating the need for training by building into the design of the machines as much of the complexity of use as possible, by extension rigidifying jobs and cutting off avenues of social mobility. The transformation of occupatons which occurs by defining and increasingly tying jobs to the development of machine technologies, and the physical separation of machine-based tasks and occupations from the office environment are factors associated with the experience of changing office work as a down-grading of opportunity and skill, and a worsening of working conditions.

The physical working environment in data processing and word processing shops is less appealing and more likely to contain adverse health factors--problems of light, heat, humidity, noise, and environmental contaminants--than are conventional office environments. The micro-working environment defined by the "man-machine" relationship is often fatiguing and stressful, both as a consequence of the routinization and machine-pacing of tasks and of the restriction of physical movement of operators to confined locations. At the same time that the definiton of the job as a major element in the life chances of individuals becomes less appealing, expectations of mobility evaporate, and dissatisfactions with the job emerge even more insistently than in conventional secretarial work, which still retains an illusion of promotability. With the application of methods engineering, work simplification, and work measurement techniques accompanying the mechanization and automation of tasks, the illusion of promotability--and indeed the substance, if ever there was any--disappears and the job comes to be seen as a "Dead End".

Changes and Conflicts in Office Roles: When expectations for mobility are thwarted, the competition among individuals in the distribution of tasks to different positions may increase. Although traditional secretaries, for example, may have experienced a number of dissatisfactions with their jobs--such as unpleasant and overbearing supervisors, lack of discretion, low pay, or conflicts with co-workers--there were also compensating sources of satisfaction associated with working conditions and proximity to management in administrative offices.

In such environments, secretaries could continue to believe that promotion into positions of actual decision-making authority was possible, and once in a while that expectation might be fulfilled.

Secretaries whose jobs are defined as a derivative of machine-operations--among them keypunch and data entry operators and word processing secretaries--may become dissatisfied with their work for a broader and more compelling set of reasons. The scope of their jobs is limited to an even greater extent than is that of administrative secretaries who carry on the traditional secretarial--communication--functions, and they are also often located in environments which are physically unpleasant and limited to doing routine work under production quotas. They do not share the compensating set of satisfactions of secretarial work in traditional office environments, however, and the benefits of their "technical" expertise rapidly diminish as the technology becomes established.

For the short period of time during which new machine-based jobs are being created, satisfaction in performing those tasks and in becoming qualified to fill these occupations derives from their newness, which gives to their incumbents a feeling of being progressive, open-to-learning, and upwardly mobile via the acquisition of new skills in short supply. Moreover, differentials in pay at the same grade are often offered to machine operators in these early stages of development as an inducement to workers to try for the new positions at a time when people are needed to fill them. Once the job has been routinized, and the qualifications set, those relative advantages fade, and the second generation of occupants in these jobs never experiences them. The doors to occupational mobility are only open for machine-operators during that period of change; once that period is past, all that can be gained are improvements in working conditions or compensation. The reason for this can be seen in the orientation of designer-based development.

In the designer-based mode of systems engineering, as we have seen, the aesthetic criterion for job design is to strive to build as much skill as possible into the design of the machine, and in so doing, to separate the design of that machine from its operation and use. The outcomes of this process of rationalization frequently have the effect, first, of rendering the special certified skills obtained in the early stages of development outmoded by new equipment, and, second, of eliminating any occupational advantage thereby of specialized education and training.

In the mature stages of such "production" technologies, what had earlier been a complicated machine process requiring esoteric technical training and skill can now be undertaken by a relatively unskilled worker--or "naive user". Although the new technology is now readily accessible to the "public" (and is therefore capable of being widely marketed) those machine-based occupations which were established in the early development of the new technology become permanently down-graded to the status of "operators' jobs", only nominally technical in nature, and vulnerable, even when organized, to ongoing technological change.

In this way, ongoing technological innovation and development become a potential threat to the interests of individuals at all levels of the organization, and the definition of roles associated with the new technologies becomes, as Edwards suggested, a contested ground on which different segments of the workforce compete.

The Secretarial Role: Traditionally, the role of secretary was that of an assistant to a decision-maker, specialized on the basis of literacy. In addition to priests--clerics, from whom the term "clerk" originally derives--clerks or scribes in ancient society served as statisticians and data-gatherers in the employ of bureaucratic governments, and as aides to military and/or governmental decision-makers and leaders. Clerks were generally men who enjoyed a relatively high social standing because of their literacy and their proximity to the ruling elite, and frequently they were able to use their positions to step into the executive position when circumstances favored it. In this way, their influence and prestige were higher than would perhaps be inferred from their subordinate status.

Industrial societies saw a general rise in literacy among the population at large, and among women as well as men. Where the traditional secretary, as assistant to an executive, had been male, and where the clerical job made it possible for young men to enter firms as clerks and to work their way up the ladder to management, industrialization and mechanization transformed the secretarial job and eliminated this clear line of social and occupational mobility into management. The introduction of typewriters in offices in the late 1800's, was accompanied by the rapidly increasing employment of women in clerical occupatins, and by a qualitative limitation on the possibilities for upward mobility into managerial and professional positions.

During the period between World War I and World War II a major transformation occurred in business organization. The managerial revolution replaced the operational powers of the original class of entrepreneur/owners--the founders of the great industries--with a new generation of managers who learned their roles in business school, where they were taught the lessons learned by the early pioneers. As axioms of management, these lessons were translated into formal techniques transmitted through professional education, rather than generalizations from experience acquired working up from the bottom. Thus the second and subsequent generations of managers became specialists, and the growth of their businesses combined with the new professions of advertising and accounting generated ever-growing volumes of information, which made office work an increasingly important element in running the business, just as managers ceased having personal control over all aspects of business operations. During this period, the relationship between new generations of professionally educated specialists and the owners and managers of firms employing them came to crystallize around the enduring issues of line-staff organization, with the corollary problems of managing professionals--problems which became increasingly important as research and development activities burgeoned after World War II.

Postwar societies saw an increasing rise in the level of literacy in the general population, and a rise in service industries and occupations and in governmental regulations and reporting requirements, all of which were accompanied by concommitant increases in the volume of paperwork. Increasing size and complexity in production organizations and the increasing use of complex production technologies in organizations also increased the amount of paperwork necessary to process routine administrative functions and decision-making involving budgeting and accounting, payroll and inventory and billing. The costs of office work--the overhead on the production process per se--increased during this period relative to production costs, creating pressure for greater productivity in office work to offset the growing numbers of office employees, the vast majority of them women. Thus strains were developing in the complex occupational system of the office before computers were ever invented.

If one focuses on the functions of an office--the outcomes or services which must be provided to support the primary (or production) process--and takes as the objectives of any given position the responsibility for fulfilling those functions at some level of satisfaction and timeliness, then the various office roles divide up those functions and give to the incumbents responsibility for certain aspects of the output. In offices, the responsibilities center on the production and transmission of information, through data-gathering, -recording, and -processing throughout the organization and its environment. In this functional context, office roles can be divided between managers, professionals, and secretaries (or clerical workers, including office machine operators). 1) Managers' function is to decide and act for the organization, which consumes most of their working time in communication and interpersonal interaction necessary to negotiating such decisions, largely with an external audience. 2) Professionals provide specialist expertise, and thus input information to decision-makers on certain elements of their range of problems. Professional staff thus operate largely with an internal audience, in a functional sense--especially where they include data-processing managers, systems analysts and programmers--but maintain active relationships with other professionals working in different institutional environments, as a requirement for maintaining their expertise. 3) The secretary's role is to support the requirements of professionals and decision-makers by maintaining the physical environment of the office, taking responsibility for the physical production of the information generated by "principals", and by serving as a first contact point in dealing with the public.

Far from it being possible to stereotype office jobs through these functionally universal roles--which is the premise of order-presumptive models of office automation--different organizations will have distinctive mixes of activities in each of these roles, and will therefore be structurally different with respect to the jobs and working organization exhibited in their office arrangements. Established organizations with single product lines will have fewer professional staffers than will organizations working in high-technology fields or in uncertain environments. Large organizations will, obviously, have a greater need for large numbers of secretarial staff in order to support larger amounts of paperwork (especially in banks, insurance companies, legal offices) than will small organizations or simple "uni-organizations" having simple information requirements. One other distinguishing factor is the degree of public contact characterizing different offices. Contact with the public is a significant factor in stress and uncertainties in office work, in some cases illuminating the overlapping nature of office roles, which may in some environments be filled by one person, and in other environments may be shared among a number of persons having widely diverging conditions of employment.

Dividing the Labor: The way in which tasks associated with these office functions are distributed among managerial, secretarial, and staff roles in offices, and the way in which those tasks are supplemented by the use of various types of office equipment jointly determine the particular interdependencies making up the working structure of offices. Discrepancies and overlaps in the conventional understandings of office roles represent a significant factor in the disparity between the features and limitations of current generations of office equipment, and the actual capabilities of various ongoing organizations to produce information in the efficient and automated wasy envisioned by computer manufacturers and consultants. Viewed in this light, it is possible to see various contingencies upon which the successful implementation of office automation technology depends:

1) Electronic mail: Offered as an instantaneous form of paperless communication to replace telephoning and correspondence, manufacturers' research discloses that electronic mail is not yet widely used--except by networks of researchers. The underlying reason is the hierarchical and bounded nature of communication flow in formal organizations. The possible uses which have been advanced for electronic mail (i.e., communications) applications include "automated calendars", "automated message handlers", and "automated distribution networks".

Automated calendars would make it possible to schedule meetings without the lengthy iterations of checking each possible date with each person's calendar by storing a copy of each calendar to be accessed by a scheduling program or scheduler (secretary). However, most users jealously guard their prerogatives to set their own schedules according to their own priorities, and are reluctant to make their calendars public in this way.

Similarly, electronic mail offers the promise of overcoming two of the most irritating aspects of telephone communications: failing to reach the person being called, and being interrupted while other (often higher priority) activities are taking place. Electronic mail would make it possible to leave messages for the person being called, but once again, the privacy of those messages cannot be protected, and thus telephone communications take on the public and non-reversible character of correspondence, which largely records information intended to be public knowledge.

Finall , data communications capabilities have the potential for supporting electronic distribution networks, to reproduce and distribute reports and other stored data items and send them electronically to other locations to be printed. This application is now used fairly extensively, particularly in decentralized operations with large volumes of communications between branches and headquarters, where reporting merges with data entry operations. However, the ability to produce and distribute printed materials depends upon a highly integrated network with print facilities at each end. Printers are not only expensive, thus requiring large volumes of work to be cost-justified, but require continuing maintenance, factors which are increasingly bringing about a respecialization of the printer's craft and centralization of final copy printing in copy centers because it is simply not cost-effective to produce high-quality printed output in small localized sites.

2) The cost factors associated with automated print capabilities have implications for the breadth and depth of word processing applications. The promise of the technology is ultimately to produce printed correspondence, reports, and other text from a variety of input devices, with the logical extension removing the intermediary of the secretary and dictating equipment--replacing that function altogether with a "word processor" that works tirelessly, never takes a coffee break, and never makes mistakes. Voice entry devices have been envisioned for years, but the technology is not close to producing them at any reasonable level of reliability, much less cost-effectiveness in the near term. The recognition of spoken natural languages is the most difficult computer application yet undertaken, and artificial intelligence and natural language translation efforts were stalled as long ago as 1966, although work is now being revived around recently emerging data base concepts which have renewed interests in more language-analytic and cultural notions of frames of reference, domains or universes of discourse, and other ways of conceiving of systems of knowledge as potential "expert systems".

Word processing, therefore, remains a text-editing technology, and the possibilities for automated text-processing depend on both the capabilities of the hardware and software and on the way in which information is generated or produced in organizations. Early text-editors were programs written for large mainframe computers, in an era before the development of higher-level languages. They were relatively inaccessible to the "naive" user, and using them was difficult because of the (relatively arbitrary, from the point of view of the user) coding requirements necessary to produce and edit text. Using these text editors was, therefore, a relatively involved undertaking, which required training on the system almost at a level of other programming jobs. Automated text management programs have been improved over the years of computer use in large organizations; however, they still tend to be excessively complex and over-formal, and must be accompanied by fairly extensive training programs in order for them even to be used. Furthermore, because they are tied into the main computer system, their use in a time-sharing mode ties up resources and is often characterized by slow system responsiveness, which is irritating to users.

The development of microprocessors and stand-alone word processors ushered in the era of automated production typing and editing, made possible because having small computers with word processing--i.e., text editing--functions available at local sites eliminated the problems of slow response time on shared systems, and of limitations on access to computer resources and data files. The complex and arcane program instructions required to interact with mainframe text editors were replaced with word processing packages which streamlined the commands, building in more features (such as pagination, or hyphenation, which is a difficult task for a computer to perform) than were contained in similar text-editing programs. Human factors researchers have found a rich area of inquiry in the relationship between the display of information in keyboard placement of symbols, the definition of commands in word processing software which is activated through the keyboard or through touchpads on keyboards and screens, and in the presentation of information in video screen display formats. Significant improvements continue to be made in the physical presentation of symbolic information in human-computer interaction; however, it is not so much the physical environment, but the nature of task related issues which continue to qualify the use of word processing applications.

Word processors are not computers in the broad sense; they are limited in their capability to generate and sort information. The way in which office tasks are customarily divided between managers, secretaries, and staff professionals generally means that the operator of word processing equipment also does not generate or acquire information, but is dependent on that information being input from some "principal". This presumption is manifest in what often appears to be endless cycles of editing and revision, which involve considerable interaction between principals and word processing secretaries. The interpersonal interactions required for processing and editing text constitute a major factor in the resistance of principals to centralizing secretaries in word processing shops. Managers and staffers resisted handing in their work to a pool of operators with whom they had no opportunity to interact. Because they were now just one among many users of word processing services, from the point of view of user management, the time to produce copy for editing was longer than it would have been had their "own" secretaries typed it, and there was no control over the quality of the copy.

The development of word processors made possible other ways of structuring typing tasks, and moving word processing secretaries back into local offices--first in clustered workstations, sharing expensive equipment, especially peripherals such as printers, and later in distributed processing through localized word processing stations. Thus the continuing development of the technology made it possible to reverse the structural transformations associated with computer use, and to return the control over information-processing--at least in correspondence and report-writing where there are constraints on timeliness and confidentiality--to the hands of line managers, rendering them in the process less dependent upon data-processing and word-processing managers with their own budgetary constraints and objectives.

However, having the capability to revise reports and correspondence easily "at home", coupled with the increasing use of analytic techniques on the part of managers and staffers results in endless iterative revision cycles in which the generation and processing of text become indistinguishable, thus creating strains in the relationship between word processing secretaries and principals. In this cycle, the potential productivity benefits of the technology are often overwhelmed by the extent and difficulty of editing and revision, thus accounting for the discovery of the GAO and others that word processing has not to date produced gains in office productivity.

Professionals, interestingly, do much of their own text entry and editing as well as programming, and thus are less involved in the text-editing relationship with secretarial staff than are managers, who have exhibited more resistance to using the technology themselves--particularly to using a keyboard, and to learning programming conventions. It is thus professionals, especially those recently graduated from college, where they gained experience working with computer terminals, and not line managers who are most likely to be the users of "managerial" workstations. This brings up an interesting contingency in developing the functional capabilities of word processors and personal computers for analytic as well as text-editing uses. What it means to say that a word-processor is not a computer is that neither the storage requirements nor the software is present to permit using a word processor as a computer without extensive programming--on a host, or mainframe, computer. However, the resistance to learning programming is stronger than the (perceived) need for functionality.

> ""Middle managers, who make up over half of the users of personal computers, are not about to become data processing wizards." (Forbes, 1981, p. 125)

Although data base packages are beginning to come onto the software market for small computers, even the rudimentary sorting of information, or accessing of data files on other larger computers requires programming and time-sharing with a host computer--hence the micro- to mainframe interdependencies which are emerging to increase the size and complexity of large computer applications. By extension, small offices which have purchased stand-alone word processors will find that they are limited to just what their relatively restricted software package can do, and to date most word processing applications cannot perform data processing tasks, and cannot combine data processing tasks (from another computer or program, for example) in text-processing operations in producing reports and correspondence.

When looking at the capabilities of the current generations of equipment in terms of the traditional and ongoing tasks and functions of offices, it appears that in some--perhaps many--cases, the old-fashioned and mechanical or even manual methods of typewriting and correcting and reproducing copy are faster and more cost-efficient than attempting to perform the same tasks with a micro-computer or word processor, either as a stand-alone or as a terminal to a larger computer. A further corollary follows from this observation, which implicates the goodness of fit in manufacturers' and consultants' specialization of the secretarial position into word processing secretaries and administrative secretaries.

Secretarial Roles and Clerical Jobs: As we have noted, with the cycle of reorganizations in office arrangements, it appears that at least upper level managers did not lose their personal secretaries, as promised by the coming office automation revolution. However, the specialization of secretarial work and the creation of new clerical jobs that do not include responsibility for the whole task--or office function--which is being automated, have placed increasingly heavy pressures on administrative secretaries to manage the input and output to and from word processors and word processing secretaries on behalf of principals who often have only a vague notion of the limitations of the technology, and what it takes to fulfill a seemingly straightforward request to produce a given volume of text.

In this functional sense, the secretarial role is potentially the largest role in offices, because as a support function it is residual to that of managers and professionals. The responsibility for producing the physical output of decisions and information acquisition entails a number of perhaps unspecified tasks required to communicate information to a wide range of internal and external publics. This responsibility for producing the output also presumes that the information exists in the system in a form which can be accessed and reproduced. It is at this point that the conventional distinction between information processing--which is the secretaries' responsibility--and knowledge production and decision-making--which is the responsibility of managers as the "principals" or authors of information--breaks down.

The reasons why information-processing cannot be divided neatly between mental and manual tasks are 1) there is an unbreakable interconnection between developing the format for information and in collecting and analyzing that information; where this responsibility is split between two people, particularly where they do not communicate with each other in establishing the format, then a large element of uncertainty and additional workload enters into integrating the information with the format in which it is to be produced. 2) We are also presuming that the principals have actually produced information which is available to be reproduced; where analytical requirements enter into producing that information, we are presuming as well that an organizational data base exists in some (accessible) form which stores the necessary data in such a way that the informati-n can be retrieved and combined in reports to be "processed". Responsibility for the task-activities required to produce information is divided among secretaries, professional staffers, and managers in many ways, and it is a common element of office folklore that secretaries not only do a good bit of information gathering, formatting, and analysis generally associated with professional and managerial functions, but they also conduct a certain amount of ad hoc decision-making in their interactions with the public, presumably on behalf of the managers for whom they work.

Where hard and fast distinctions are drawn between these tasks in clarifying the definition of clerical jobs for purposes of implementing computer technology, gaps and overlaps in the responsibility--and training--for these tasks may lead to uncertainties and to interpersonal conflicts in accomplishing them. The knowledge of those activities entailed in the secretarial role which are not specified in the definition of clerical jobs represents information which is lost to the performance of those activities, and to the sense of responsibility for their outcomes. Ultimately, not even clerical work can be fully specified, because of the uncertain elements which enter into setting up a job. More important, introducing computer technology itself is an uncertain process which requires changing ongoing procedures and processes at the same time that jobs are being redefined and reorganized; in this activity many decisions and idiosyncratic actions are taken by lower-level employees which are by definition non-routine but which are necessary in order to implement the technology.

Clinging to a pre-determined division of labor--especially where hierarchical considerations limit the flow of information--may mean that necessary actions do not take place, and thus in this way the conventional status structure of the office can function as a form of negative feedback, by which the organization resists the disturbance represented by the introduction of new technology by failing to effectively distribute the responsibility for the tasks associated with implementing and maintaining it. This is a potentially degenerate situation in which not only can implementation be undermined, but uncertainties generated in the process may also have an adverse effect on the ability of offices to maintain, update, and use information in a timely fashion for purposes of strategic decision-making.

In our terms, the secretarial role involves a (perhaps implicit) abstract technology--as a support function for the office as a place and for principals as actors and decision-makers for whom that place is a location for coordinating information flows and meeting the public. In the conventional office automation methodologies, word processing secretaries' jobs are defined on the basis of machine descriptions of tasks and related skills, attached to a set of working conditions and compensations based on the job and defined specifically with respect to tasks, where tasks are derived from machine operations in routine jobs. The creation of the position of word-processing secretary is thus a translation of the role of secretary into the job of a clerical worker, focused on activities and not on outputs, for which no responsibility can therefore be attached. Instead, performance is engineered, measured, scheduled, and mach-ne-paced--and yet even high level perforamnce of these tasks may be quite insufficient to accomplish the whole job. In this sense, clerical jobs defined with respect to a set of activities comprising certain tasks or functions are abstract systems--which contain within them no methods or activities for setting up or changing that set of activities. Strains arise in the ongoing system for processing the workload in offices where the total set of tasks entailed in the secretarial role is not isomorphic to that total set of tasks specified in the clerical role.

The administrative secretary's job implicitly retains the full range of tasks associated with the secretarial role--which centers on the responsibility for accomplishing office tasks or functions on behalf of decision-makers or principals. Defined in this way, there is no clear-cut way of distinguishing the responsibilities of managers from those of secretaries within the office, which implies inherent uncertainties and potential conflicts in the role interdependencies between principals and their secretaries. It is here that the upward aspirations of secretaries portend competition with--and even downgrading of--middle managers, as projected by Whisler, Hoos, and others. (Whisler, 1958, 1970; Hoos, 1960)

In this potential competition, internal classification schemes limit secretaries to non-exempt, or classified, categories of employment having different prerequisites and prerogatives than managerial and professional positions, imposing a permanent limit on the level to which an employee can rise from the initial position of secretary.

This limit is implicitly reflected in the designation of such positions as "administrative secretaries" rather than "administrative assistants", which does imply the possibility for upward occupational mobility. This categorical limitation--unrelated to the level of skill or seniority of employees--is a significant factor in job dissatisfaction and the experience of stress among secretaries, even when they are not engaged in resolving uncertainties, but which adds to the perception of stress when they are.

For their part, line managers, whose supervisory authority and problem-solving discretion is to a great extent subordinated to plan and operating procedure, find their work constrained by and dependent upon data processing resources and expertise, resources which tend to be inflexible, inaccessible, and not well-understood from their point of view. Data processing managers find themselves constrained by budgetary and technical limitations which are not appreciated by the users of data processing services. Thus conflicts take place in the task interdependencies between managers who are constrained to working within data formats and languages developed and managed by data processing experts, and data processing managers who are subject to the often conflicting demands and overwhelming workloads generated by the requests of line managers for information services in large computer systems.

While line managers can advance into top management, they are also subject to displacement when their skills are no longer in demand or where the supervisory workload drops with attrition in the work force at production or clerical levels. On the other hand, data processing managers enjoy the occupational advantages of special expertise in their jobs, but are also subject to dismissal when their systems break down or when their skills become obsolete, just as any line manager. However, because their "profession" is based on the state-of-the-art in hardware design and operation, not only can they be displaced by ongoing technological changes, but their entry into higher levels of management is restricted by the limited scope of their expertise in the business, and by the fact that their expertise is machine-based.

Similar relations of interdependence and competition exist between administrative secretaries and word-processing secretaries, who stand in an intermediate position between line managers and data processing staff. Secretaries may be specialized to a given state-of-the-art in office equipment, but they must also translate the requirements for using that technology into the execution of information processing tasks as defined by their line supervisors in different sectors of the organization. Not only are their jobs subject to displacement through potential gains in productivity and/or continuing developments in the technology which make their machine-based skills obsolete, but they must also figure out for each task how it is carried out given the constraints of procedure and available equipment.

Once they have gained this expertise--both in the tasks and the functions performed in the offices in which they work and in the data processing technologies used in the organization--secretaries then find that no avenues of occupational mobility are open to them beyond word processing supervisor. Unless the technology changes, there are no new skills or tasks to learn or to organize, and the expertise which they acquired in implementation does not translate into promotion into managerial positions or data processing positions. In this way the role of assistance and liaison between data processing and line managers which is carried out by clerical and secretarial personnel comes to restrict their opportunities for advancement in direct proportion to their growing skills.

These role assumptions and ambiguities underly in this way the separate development of "managerial" and "secretarial" workstations, each product equipped with different functional capabilities and storage capacities. This separation in functionality may mean that automatic text processing will not be feasible without first solving incommensurabilities in the roles of word-processing and administrative secretaries, managers, and data processing staff. This resolution largely implies interpersonal interaction to jointly define the problems and their solutions, and to distribute the responsibility for the tasks making up that solution. Participation in decision-making in computerization, as advocated by Mumford et al (1975, 1979), Lucas (1975) and others since them, is therefore desirable not merely in order to elicit improved motivation on the part of employees but to reduce strains in the accomplishment of office work by interacting in the definition and division of that work to resolve uncertainties in the process.

However, given the blurring of work between secretaries and principals, together with resentments over widely diverging conditions of employment between secretaries and managerial and professional staff--who are paid higher salaries and who have greater personal freedom of movement at work, greater opportunity for promotion, and greater decision-making latitude--this limitation in functionality may be the "last straw" which raises the level of uncertainty in the workflow during implementation to an unanalyzable point at which strains in the workflow cannot be resolved without eliciting conflicts among co-workers, if at all. These strains in the workflow and conflicts among interdependent co-workers may emerge as stress for individuals working in that process, stress which is manifest in the occurrence of errors in performance, interpersonal conflicts, and a host of physical ailments and complaints.

Control and Demand as Factors in Stress: Problems in the task interdependencies associated with the use of computer-based office equipment are associated with the perception of stress, and by extension with the incidence of both conflicts and adverse health effects in office work. A recent survey conducted by the Working Women Education Fund through the sponsorship of the Occupational Safety and Health Administration elicited the following explanations for stress in secretarial jobs:

1) Word processing secretaries complain of monotony, repetitive work, and rapid work pace--especially where combined with machine monitoring of performance levels--as factors making their jobs stressful. Among users of video display terminals, stress is also associated with the imposition of production quotas and inadequate rest breaks.

The following comments from case studies reflect the experience of stress in these word processing jobs:

> "The chairs were good, and the machines were adjustable, too. But I have never been confined to one place doing key entry at such a pace. The computer at one end of the room keeps track of the keystrokes you do. The more keystrokes, the more money you might get. At the end of the day, the figures for all of us were posted. You look at your speed, you look at everyone else's and you say, 'Tomorrow I'm going to do better.' They get you thinking just like they want you to, you're really pushing hard."
>
> "One girl who is also a CRT operator came to the company right out of high school. She's been running a CRT for close to ten years, and she's fast as the wind. But it's really affected her personality. I used to wonder if something was wrong--she had no exuberance. One morning she turned to me and said, 'Rose, as soon as I sit down at that machine in the morning I feel like I'm going to cry.'" (Gregory, 1981, p. 5)

Lack of recognition, and the inability to address the problems involved in computerized jobs are also stressful, as reflected in the following observations:

> "You know, when the boss brings new clients through the office to show them around, he'll point right at me working at the word processor and say, 'Here we have our wonderful new LEXITRON' and then moves right along. He doesn't even bother to introduce me--just the machine!"
>
> "One day...I was sitting there at the terminal, entering data, reading from the screen, and I suddenly thought I was having a terrible nightmare. I didn't know where I was. It took all the discipline I had to sit there quietly, until the whole thing passed."
> (Gregory, 1981, pp. 4-5)

Not surprisingly, given the physical and occupational constraints on their jobs, certain adverse health effects were reported with significantly higher incidence among vdt users--including word processing secretaries and data entry operators--including: eyestrain, back or neck pain, aching wrists, and skin rashes. Of vdt users surveyed by Working Women, 44% responded that they believed their health to have declined as a consequence of working at their current jobs. (Gregory, 1981, p. 11)

2) Administrative secretaries complain of lack of promotions and upward mobility, and lack of respect and lack of recognition or credit for their accomplishments and contributions, as reflected in the following comments:

> "Amanda, who works for a major industrial firm here in Cincinnati, paused at her VDT momentarily just as her manager walked in. 'What are you doing?' he demanded. 'I'm just thinking,' she replied. 'Get back to work,' he snapped. 'You're not paid to think, I am.'"
>
> "I've been here almost a year, and I've got seniority among the secretaries. It's the strain--everything must be done under the deadline. We end up rushing around like crazy to make up for their delays in getting the job done so it can be typed."
>
> (Gregory, 1981, p. 5)

In addition to the lack of promotions and credit for accomplishments, certain secretarial categories--particularly legal secretaries--ranked heavy workloads and overtime as well as inadequate rest breaks as stressors deriving from the fact that their responsibilities entail expectations that they will work through breaks, over lunch and be available for overtime without notice.

Working Women's summary of the most frequently cited reasons for stress in office jobs are, in rank order, the following:

1. Lack of promotions or raises
2. Low pay
3. Monotonous, repetitive work
4. No input into decision-making
5. Heavy workload/overtime
6. Supervision problems

7. Unclear job descriptions
8. Unsupportive boss
9. Inability or reluctance to express frustration or anger
10. Production quotas
11. Difficulty juggling home/family responsibilities
12. Inadequate breaks
13. Sexual harassment.

(Gregory, 1981, p. 16)

Among those indicating that their jobs were very stressful, problems of heavy workload, supervision problems, and unsupportive bosses were ranked higher as stressors than lack of promotions or monotonous work. Office workers with stressful jobs were more subject to fatigue, insomnia, and digestive problems than was the general population surveyed. One further distinction can be made between the general population and those secretarial workers who belong to unions, among whom the lack of promotions and low pay were not ranked as stressors since these issues are managed by collective bargaining, which provides to union members wages 30% higher on the average than those of non-unionized workers. However, the lack of input into decision-making does emerge across all categories as an important aspect in the experience of job stress. (Gregory, 1981, pp. 3, 11-12)

Lack of input into decision-making on the job and constraints on the manner in which work is performed point to an interrelationship between the incidence of stress and varying levels of demand and control characterizing different jobs. As Robert Karasek points out, research on characteristics of the working environment has raised paradoxical findings in the area of job-related stress, a concept which he notes refers to internal states of the individual and which therefore cannot be measured directly. Research findings by Quinn and others have indicated that although assembly-line operatives and executives both experience their jobs as stressful, workers of higher status are more satisfied, and more mentally healthy than lower-status workers, although they experience greater emotional tension concerning their work than do workers who are less challenged. (Quinn, 1977, p. 411; Karasek, 1979, p. 286)

Similarly, studies of work load and productivity have suggested that while strain symptoms resulted from "time pressure demands", "intellectual demands" did not necessarily produce strain in the same way. Karasek argues that the seeming inconsistencies in this research area stem from failing to distinguish between stressors in the word-load and that stress which is associated with what he calls "job decision latitude". Job decision latitude is a function of skill level and decision authority, and Karasek proposes that the adverse effects of stressful job demands are, to a certain extent, reduced or cancelled out by the "opportunity to use skill and make decisions". Karasek proposes a job strain model which distinguishes and correlates these two characteristics of job decision latitude and job demands, hypothesizing that

> "...(P)sychological strain results not from a single aspect of the work environment, but from the joint effect of the demands of a work situation and the range of decision-making freedom (discretion) available to the worker facing those demands."
> (Karasek, 1979, p. 287)

"Job decision latitude" is an intervening concept in the conjunction of these two factors which acts to moderate the transformation of (what we could call the experience of) stress into action. It is unreleased energy resulting from stress, Karasek argues, which results in mental strain. He conducted a cross-cultural investigation of the effects of job stress on mental strain using National survey data comparing workers in the U.S. and Sweden. Job demands and job decision latitude were measured on the basis of a set of factors including: high skill level, learn new things, nonrepetitious, creative, allows freedom, make one's decisions, participate in decisions, have say on the job, work fast, work very hard, lots of work, not enough time, excessive work, no time to finish, and conflicting demands. Karasek found in both populations that the ability--indeed, the requirement--that workers use intellectual skill and make decisions is associated with reduced symptoms of strain at every level of job demand. There is no support, therefore, for the notion that people are "overburdened" with decisions, but rather that making decisions

> "...represents an opportunity to exercise judgment. This enhances the individual's feelings of efficacy and ability to cope with the environment; it is not a source of stress."
> (Karasek, 1979, p. 303)

Thus it is constraints on decision-making latitude which induce stress in workers--from executives to assembly-line operatives--and not decision-making per se. Specifically, "...job strain occurs when job demands are high and job decision latitude is low." Karasek's findings show that it is primarily under these conditions that workers report exhaustion, depression, sleep disturbances, nervousness, and anxiety; and he argues that these symptoms are a consequence of unresolved stress generated by the characteristics of the job. (Karasek, 1979, pp. 287, 292)

The factors which Karasek associates with job decision latitude and job demands are consistent with the expressions which clerical workers give of their experiences of the stressful aspects of their work--particularly lack of freedom, lots of work, repetitive work, and conflicting demands. These constraints contrast with the experiences of managers and professionals whose jobs entail high levels of demand, but which also involve wide areas of discretion, and thus control over the conditions under which they work.

The issue of control is central to Robert Jackall's socio-linguistic study of the manner in which workers in a bank legitimate their work--i.e., the manner in which they define their work as meaningful and acceptable, and thus as a process in which they voluntarily participate. Legitimation in this view is a process which takes place as workers explain their work to themselves and others, and as they account for the alienating, stressful, or unrewarding experiences of their jobs in excuses and justifications they give for "problems". Jackall argues that

> "...(I)t is precisely through such legitimation that workers join themselves to their work even when they find it alienating. In doing so, they help create and renew one of the central institutions in their own lives and in the social structure as a whole."
> (Jackall, 1978, p. 7)

The issue of control over one's work--implied in problems of work measurement and pacing--are important to people because control represents an independence from the technology and an assertion of human dignitiy and consciousness. Problems of control in this sense are tied to the structural bases of work. In Jackall's study workloads were determined by computerized, quantitative analysis called the Time and Allowance Practices Manual (TAPS) on which basis operations officers were expected to make use of "...at least 95% of all the minutes of their staff's workday." In Jackall's study, control is related to the influence of market cycles--referring to patterns of business activity corresponding to commercial activities of the bank. These market cycles influence the organization and experience of pressure in the work of individuals in this environment. Pressure comes with the peak points on each of these cycles, which becomes cumulative, and with the degree of public contact involved in different positions. Those who experience the least amount of control are those, by far in the majority, who work in jobs with direct public contact--i.e., "...whose jobs connect them directly to the market forces which, as it were, initiate their work tasks." (Jackall, 1978, p. 26)

The implementation of methods of control and coordination through the measurement of tasks in time and motion studies takes place in the context of these externally-driven pressures. Legitimation of that work thus requires balancing the constraints of the job as formally defined in the TAPS guidelines with the pressures determined by external cycles, a balancing act carried out by operators themselves and rationalized through the interpretations which they build up to account for their work. In this way, both Jackall's and Karasek's studies confirm our assertion that secretarial work involves broader responsibilities than that which is specified in the definition of clerical tasks.

Human Factors

The public expression of problems in office automation, and the resurgence of union activity focusing on health effects of new office technologies, call forth images of an earlier period in the Industrial Revolution during which production processes were changing as a consequence of the moving assembly line. The characteristics of what was then called the "American System" were associated with the increasing centralization of capital in ever-larger industries in the U.S. and Europe, in almost immediate and seriously adverse health effects on working people, and in waves of labor unrest during the 1920's in Britain, France, Germany, and the U.S.

At that time intractable practical problems associated with established industrial paradigms and available means for ameliorating the ills attending newly introduced production processes fueled transformations in the discipline of human factors engineering, reflecting major changes in the conceptualization of human beings in the design process. In the World War I era, human factors approaches were directed to adapting the human to designed systems through recruitment and training and job design, and through improvement in the working conditions in factories, especially focusing on lighting, temperature, and the length of the working day. In the World War II era, insurmountable problems in adapting human beings to highly complex machine systems which did not permit of incremental changes led to a transformation in approach which focused on designing machine systems which were adapted to human requirements and limitations. Today, continuing developments in computer-based automation, especially in the designer-based mode of systems engineering, read out the human element in such a way as to raise serious social-psychological issues having to do not just with the physiological effects of working in designed processes, but with intra-organizational conflicts which do not lend themselves either to an adaptation of people, nor of designed systems, but which suggest a reconceptualization of the process of design to include the participation of lower-level employees in planning and implementation. Although the traditional--psychology-based--human factors disciplines assert that they are now adding to their traditional emphasis on tests and measurement, human engineering and human relations, a research focus on issues in "organizational psychology" (Sgro, 1981, p. 12), we must argue that the scope of such theories in industrial psychology and human factors engineering is too narrow to account for problems being reported today, problems which force recognition of the influence of context--and of the design process itself, broadly conceived--in outcomes for people.

The History of Human Factors Research:

Under the press of wartime exigency, the first problem for industrial organization is that of mobilizing men and production for the war effort--the problem is essentially that of increasing productivity in the face of uncertainty. The second problem is that of maintaining working conditions at a level sufficient to support such gains in productivity and to prevent labor unrest and the deterioration of worker health and well-being, which is itself a source of uncertainty. Deterioration of working conditions leads to a breakdown in the health of workers, and to social unrest, both of which are influential in the emergence of human factors engineering out of the development of wartime production processes. The discipline of human factors engineering emerged during World War I, when adverse effects were experienced as a consequence of the implementation of new--designer-based--production processes, particularly the moving assembly line and machine pacing of work, developed in the U.S. and later implemented in European factories.

Prior to World War I Europeans, and especially British labor unions, were highly resistant to increased mechanization in industry, and where mechanization was employed British firms did not necessarily pay higher wages, as did their American counterparts. Given the constraints represented in secure trade unionism, it took the special exigency of the outbreak of the war to induce European firms to adopt American production technologies. (Bagwell and Mingay, 1970, p. 213)

According to contemporaries, business principles in the industrialized countries prior to World War I were developed through practice and accumulated experience, fragmented and conservatively guarded, progress coming only through refinements of established forms. Waste and ignorance were widespread.

> "Then came the war--not a war of muscle and sturdy blows, but a war of machines. Whole peoples went forth to fight. Every ounce of strength of every woman, almost of every child, multiplied a hundred fold by the use of machinery, was needed at home to furnish materials for the two double lines of men who faced each other across the civilized worlds...It was not a time for conservatism. Trade secrets went by the board...Waste became a crime. Standard methods were devised and enforced. The most able men headed the government war boards and became teachers to industry at large."
>
> (Farnham, 1921, p. 70)

In this environment of shared knowledge under the press of wartime exigency, the most efficient plants in France, Italy, Germany, England, and the U.S. were sought out for study. During World War I European manufacturers sent representatives to the U.S. to study American methods, at the same time that they were improving their own industrial operations. According to Farnham, European manufacturing efficiency was upgraded over a 5-year period, while American plants "modernized" in 18 months. In Europe new facilities were "mostly filled with American types of machine tools--paid for during the war". In addition, he notes widespread consolidation of capital and increasing size of European firms, and increasing participation of governmental agencies in planning and organizing industrial development. In Germany, the central government was during this time undertaking to organize all industries for the purposes of mass purchasing, research and standardization, and "scientific management". (Farnham, 1921, p. 25)

The foremost technological innovation of the day was the American "technology" of mass production--American because although all of the elements of continuous flow production were known in Europe prior to World War I, they were most fully developed in America, where unions were not as secure as their European counterparts. This abstract technology began to be transferred back to Britain in the modernization of the boot and shoe industry, in the use of American machine tools in British bicycle manufacture, and in the introduction of high-speed steel (developed in America in 1898) into the British machine-tool industry. (Bagwell and Mingay, 1970, pp. 165-170)

This American "genius" for production engineering has been reflected in nearly every field of technology, although Americans were not the original inventors of many of the techniques or implements they developed. According to Fridenson, the interchangeable parts model of manufacturing was actually originated in Europe, first in Sweden and then in France, before it was developed in the U.S. (with the contributions of Whitney and others) and re-introduced into British small arms manufacturing. In England prior to World War I some automobile manufacturers were close to developing a moving assembly-line system similar to that of Henry Ford, with the difference that the chassis were transferred from one to another group of specialist fitters, while in America the assembly line was designed to be operated by workers who were not specialists by training.

The transfer of the assembly line/interchangeable parts method of manufacture to Europe was motivated by the entry of Europe into World War I, which created an immediate mass demand for arms and munitions--analagous to the situation faced by Eli Whitney nearly 50 years earlier. The solution was to adopt the "American born assembly line." The German government deliberately refused to accelerate the "deskilling" of the workforce which accompanied the American assembly-line, but the British press "enthused warmly" over the fact that the new technique could produce a three to four-ton lorry every half-hour of the day, a "highly efficient method (which) was virtually unique in the country at the time." (Fridenson, in Krohn, 1978, p. 161)

Farnham and Fridenson both note significant differences which emerged in the implementation of these new production technologies in various European and American contexts. Farnham points out that while time studies were introduced into European factories during this period, the concept of time studies for determining a fair day's work, and of "employment management"--including attention to safety and hygiene--were elements of industrial organization in France and Italy as early as the 17th century, in America, where the workmen and foremen are comparatively untrained, "...the art of fitting the man to the job is still in its infancy...." (Farnham, 1921, p. 8)

Fridenson also notes significant differences in the contexts of implementation during this period. For example, at Renault in 1922 the rate of assembly was considerably slower than in American plants, as each car in process remained at each station for 40 minutes. The speed of diffusion was also gradual, with methods introduced one at a time, and procedures changing gradually from department to department, with innovation further limited by the availability of skilled engineers. At the Morris plant in England, groups of workers were stationed along the stationary assembly line, and the chassis were manually pushed from station to station. Ford itself installed a moving assembly line in Manchester, England, and at Cork, Ireland, after 1920. In all cases, American patents were used, or American machinery was directly acquired to build or refit new plants.

During this same period--especially from World War I into the 1920's--the ideas of "modern industrial administration" as defined by F. W. Taylor had been widely publicized in Europe, becoming influential by the 1920s. While Taylorism was recognized for its new work norms, Henry Ford's moving assembly line was even more enthusiastically adopted as the straightforward application of machinery to the needs of the production process--especially the need for in-plant transportation and materials handling. The innovation of the moving assembly line made it possible to 1) reduce production time and eliminate waste periods in the working day, thus facilitating closer control over the flow of production and limiting the expenditure of fixed assets. 2) In addition, as exemplified by the "American System" the tasks performed by workers were further subdivided into very simple operations requiring a minimum skill level. The moving assembly line also made it possible to elicit from these workers greater productivity, as being stationary, the materials in process come to the worker rather than the reverse. As a result, the worker loses what little degree of control is left over the rhythm of work. 3) In America the savings in production costs permitted a drop in prices and an increase in wages, as Henry Ford's $5 per day nearly doubled the going wage rate for auto workers. It was largely due to this increase in pay that these assembly-line methods were not more strongly resisted when they were introduced into European industries.

In France and England workers and trade union officials accepted the arguments of the necessity for economic growth, which would be served by these "advanced methods", and they expected concrete benefits for the working class to follow from their increased efficiency, although it appears that European labor received a considerably smaller share of the increases in productivity than did workers in American plants. The same efficiencies supported the use of Taylor's methods in Russia--where Taylorism was considered by Lenin to be "scientific" and as such "inevitable" because it represented a more convenient and efficient method for producing more goods at cheaper prices with the same amount of cost and effort. (Although Lenin's enthusiasm for Taylorism appears ironic today, it can be noted in passing that the conditions characteristic of the context for industrial development in the Soviet Union in this era closely resemble those of the "backward" United States in the previous century.) Individual workers in Britain and France did not oppose these production innovations as vigorously as they might otherwise have, because as skilled mechanics they believed they were insulated from the worst effects of the assembly line; although they complained about speeding up, they appreciated the comparative increase in wages. (Fridenson, 1978, pp. 167-170)

Not generally mentioned in accounts of technological development--and definitely overlooked by Farnham--are the unanticipated "side-effects" of this type of thoroughgoing rationalization of production--both for the furtehr development of technology and for the working conditions of operators in these industrial environments. Henry Ford's assembly line represents a full extension of the type of informal engineering practices pioneered by Eli Whitney and others, as a fairly strict rendering of the designer-based rationale for industrial development, quite distinct from Taylorism, which in this context appears almost as a reform movement, not appropriately recognized as such in Europe at the time, nor in the U.S. today, for that matter. Fridenson argues

> Whereas divergent tendencies could develop within Taylorism, Fordism had a one-dimensional thrust.
> (Fridenson, 1978, p. 168)

The designer-based--or informal engineering--approach to systems engineering defines social organization for production as formally isomorphic to the technical requirements entailed in the design for a product; in this sense both the product being manufactured and the social group doing the manufacturing can be factored into a set of basic elements with respect to that design. To the extent that the manufacturing process is completely defined, one could demonstrate that the organization represented in the finished product is reflected in that of the human group, with the possibility of describing--and thus organizing--both the design of the machine produced and that of the producing machine according to the same "formula", which formula represents an abstract system referring both to the product and to the organization producing it. In Whitney's case, both the rifle and the social group involved in its manufacture were material embodiments (outcomes) of the same set of relations, in the sense that a description of the assembly entails the activities of the producing group. Thus is technical representation in design an ordering relation, both materially and socially.

However this formal--presumptive--isomorphy between the designed and the producing system overlooks the origin and process of design in systems development, and even where the design is valid, this order-presumptiveness raises important questions concerning 1) the scope, or reference, and 2) the level of abstraction, or fineness of resolution of the models in which these systems are defined. True isomorphy, as a 1:1 correspondence between systems is only conceivable in the simplest of abstract systems. We always understand the various alternatives to and iterations of some design as homomorphism, more or less well-executed in different contexts.

In those respects in which the stipulations of designs do not include certain environmental conditions, the outcomes for people working in those designed systems will be unexpected, and often undesirable. Increasing complexity entails greater uncertainty in implementation, which derives from the increase in the number of possible outcomes, given increases in the number of initial conditions that must be considered. In experimental systems based on inductive empiricism as the logic of inquiry, increasing complexity means greater requirements for testing each element and combination of element, a requirement which underlies the procedural developments in operations research. And this is still no assurance that all of the relevant conditions--or factors--have been included in the design at a level of detail necessary to support construction and operation of systems-in-use.

A recurring issue in the development of science-in-industry through two world wars (in which the consequences of design are recorded) is that of the "human factor"--the potentially hazardous or undesirable effects of a product or production process on people. This factor is generally overlooked in design and revived through experience of undesirable side-effects that often accompany the implementation of new technologies. Sombart had particular contempt for Henry Ford, who he argued completely misunderstood the basic principles of social organization, and who

> "...(T)hrough his insane measures, greatly injured his country by increasing the inhuman forms of labor in big industry by several degrees.
> (Sombart, 1937, p. 281)

Waddington comments that Ford made his fortune "by saddling the modern world with the materially enriching but humanly brutalizing hierarchically organized assembly lines of mass production. It is a method that works," he argues, "but its pri-e is a bit stiffer than you might guess at first sight." (Waddington, 1977, pp. 50-51) Ironically, it is war that brought the human factor to light.

The Human Factor in the World War I Era: Although there was little evidence of but scant concern for life and limb in American mines and factories during the often violent labor disputes that accompanied the "gilded age", that perspective changed with the onset of war. As Baxter notes, concern for the health and well-being of the man on the line is strongest during wartime, when it is in the interests of collective survival that personnel be kept healthy so that they can keep on fighting. The history of modern warfare records steady improvement in the maintenance of human health over the past century. Of the 1,390,000 people who saw combat in World War I, 49,000 were killed in action or from wounds received in action, 230,000 were wounded, and 57,000 died from disease. (Morison, 1972, p. 199; Baxter, 1946, p. 105) In World War I, Great Britain, already suffering depletion of her reserve of able-bodied men, could hardly bear a corresponding rate of casualties in her industrial "army", whose increasing productivity was accompanied by a dramatic increase in accidents, illness, and even death.

Continuous-flow production, as we have seen, makes it possible to control the pace of production "externally"--that is, pacing is set by the process and not by the individual operative. The limitations on the speed of production, and therefore on the volume of output, are set by the parameters of the machine process. When a person must respond to signals which are presented at a rate which he cannot control, the presentation is "forced-paced". According to Sheridan and Farrell, forced pacing is inefficient where high response rates are required because of inconsistencies between response times and fixed intervals between inputs; moreover, they argue, forced pacing is also objectionable in regular work because of the feeling of being controlled by a machine which it gives to the worker. (Sheridan and Farrell, 1974, p. 134)

There are actual hazards which are also involved in forced pacing, particularly under uncertain working conditions. Where the process cannot be controlled by the human operator, it is not possible to adapt his or her strengths and limitations to the requirements and limitations of the designed process, with the result that limitations of human physiology can easily be exceeded, with no recourse but damage to the human body. In the order-presumptive, designer-based paradigm of systems engineering, the role of the individual is to comply with the specifications of some aspect of a designed process. On this basis, control and coordination in the system are based upon leadership, with persuasion as the mechanism for eliciting the levels of motivation in individuals that will ensure their compliance and commitment to organizational goals, as reflected in levels of performance consistent with some criterion specified in design. To the extent that the machine design is incomplete, however, the individual must adapt. In closed systems, the adaptation of the individual does not alter the configuration of the system in such a way as to enable it to adapt more successfully or efficiently to the human operator or to uncertain process requirements; thus the adaptation of the individual is frequently heroic, and often insufficient, and where the system is both stable and inadequate, the consequences can only be harm for the individual and unreliability in the process. The emphasis on productivity in human factors engineering stems from the recognition that hazards to workers were reflected in unreliabilities for designed processes; hence it was no accident that industrial reformers came to adopt a cost-benefit approach to rendering these unforeseen hazards visible and "accountable" in the same sense as any other cost of doing business. It is this understanding which underlies the field of ergonomics, as an outgrowth of early human factors engineering.

One of the earliest references to the "human factor" in industry is to be found in the Proceedings of the Western Efficiency Society, published in 1917, in which the adverse health effects of American assembly line methods on British munitions workers were noted. The work of members of the "efficiency societies" constitutes the beginnings of a broad management science movement supplemented by human factors engineering, a field stimulated by the limitations of the technologies of mass production. Human factors engineering began as a self-proclaimed movement for humanizing the industrial process, and for demonstrating that efficiency and humanism are not mutually exclusive.

> "Human engineering is an agency that has come into being as a result of demands made for the improvement of industrial conditions.
> (Grieves, 1917, p. 2)

Those industrial conditions which first aroused the concern of American industrialists and educators were those of the British munitions workers during World War I. Prior to the war, it was generally believed that organized labor in England was sufficiently powerful to limit production below the American average, to enjoy relatively higher wages for fewer hours of work. For this reason, the argument proceeded (for both the contributors to the Western Efficiency Society, and observers like Farnham and other progressive industrialists) labor gains were never satisfactory to employees because such gains failed to match increases in prices, which were the companies' only weapon against trade unions. The result was that production costs increased to the point at which British products were no longer competitive with foreign products, nad unemployment increased even while prices were high. (Farnham, 1921, p. 86; Western Efficiency Society, 1917, p. 22)

This situation changed with the advent of war, in which the exigency was so compelling that the abrogation of labor rules and rights and privileges was followed by a virtual revolution in the methods of production, methods which replaced old awkward machines operated by union members by new, "improved" machines which could be operated not only by skilled and semi-skilled men, but by unskilled men and women as well, an innovation which was hailed by the Western Efficiency Society. However, the result of this rapidly increased mechanization was a dramatic decline in the health of the British working people, and a concommitant drop in productivity. It was observed that in France and England it was possible to place a heavier burden on the working population, because those working people voluntarily gave up their protective restrictions, with the result, as reported in the Chicago Herald, that

> "...(I)t was learned that the English operatives had attempted too killing a pace. They had tried to run a Marathon race at a hundred yard dash speed. Consequently the human factor began to fag and production failed. The industrail army was willing to endure almost any strain, but its efficiency was reduced when hours were too long and the work too continuous."
> (Western Efficiency Society, 1917, p. 15)

In Britain, the Health of Munitions Workers Committee was appointed in September, 1915, to study the problems of industrial fatigue and the health and physical efficiency of workers in the munitions factories, problems brought about in consequence of extraordinary production practices which imposed long hours of labor, Sunday work and low wages--conditions leading to a general undernourishment and impoverishment of British workers. The report of this committee attributed the causes of industrial fatigue to lengthening the hours of labor which resulted in reduced efficiency due to muscular and nervous exhaustion. After an initial period in which overtime was effective, the rate of production tended to decrease until the extra hours worked produced little or no additional output, and that often at the expense of adverse effects on the quality of the products and health of the workers. The policy of long hours was implicated in increased time lost to sickness, which was greater among older workers. In addition, and not generally mentioned today, long hours in this era imposed a corresponding hardship upon managerial employees, whose hours were equally long and to whom also fell the anxieties and responsibilities for maintaining output. (Western Efficiency Society, 1917, p. 127)

In the United States, mindful of the experience of the British workers, the principal objective of the newly formed Western Efficiency Society was to consider and preserve the human factor in industrial preparedness by maintaining existing standards of work while mobilizing industrial capacity to the war effort. Consensus on the value of industrial manpower to the war effort was manifest in the cooperation of labor, government, and management (referred to as "capital") in the formation of the Committee on Labor of the Advisory Commission of the Council of National Defense. Speaking before the Council in May, 1917, Samuel Gompers expressed the commitment of labor to the war effort, and at the same time underscored the principle of human factors upon which increases in productivity depend:

> "...(G)ood working conditions are as essential to high production as high production in this time of stress is essential to the maintenance of the battle front. It would seem to be treason to the best interests of this country to desert such principles now. Now more than at any time in our national history we do not want production to fall off; rather, we want to accentuate it. Now more than ever we want the army in the factories and fields to be an army of strength fighting for democracy; we don't want a nation of working people with hearts and bodies weakened...."
>
> (Gompers, quoted in Western Efficiency Society, 1917, p. 25)

The results of statistical studies on industrial accidents, illness, and output were available at this time to demonstrate the capabilities of human workers and influence of fatigue on output, and the pertinence of the experience of the British munitions workers for American laborers was evident in the number of accidents occurring in American industries during this period. In 1917 the record was a total of 35,000 workers killed and 700,000 injured yearly, out of a total work force of 30,000,000, each of whom lost an average of 9 days/year due to sickness. (Western Efficiency Society, 1917, p. 29)

The reason for these adverse effects--and for poor working conditions in general--was attributed to the inability of management to properly supervise the production process, either because of their ignorant continuation of old autocratic managerial practices, or because their companies had grown past the point at which an employer could maintain first-hand knowledge of his enterprise. Harry Porter, an early member of the Western Efficiency Society and editor of System magazine, argued that the large scale of modern business of the time removed management from operations to such an extent that carelessness in supervision down the line led to a "harvest of accidents". Those accidents he attributed to long working hours with insufficient rest time, to undernourishment and other "outside" conditions in the worker's home life, to green untrained men, and to autocratic and despotic supervision on the part of foremen. Porter explained

> I am satisfied myself, although it is hard to prove consistently, that bull-dozing bosses are responsible for a great many accidents. They scare the men, and they draw into themselves, and get nervous...Then is when things go wrong, accidents happen."
> (Porter, quoted in Western Efficiency Society, 1917, p. 157)

At that time, the newly established practice of cost-accounting held promise for convincing manufacturers that accidents are accountable as an industrial expense, and thus that sane measures of accident prevention can be justified on the basis of efficiency, and properly accounted for. The mission of the Western Efficiency Society was thus

> "...to do our share in securing for them, the kind of conditions that will maintain their physical fitness and at the same time insure a maximum production of the proper quality."
> (Western Efficiency Society, 1917, p. 30)

A corollary objective was to undertake the education of management, first by impressing on the current generation of managers the importance of the "human factor", and second in producing a generation of properly trained managers with an appreciation for such problems. This objective was an important element in the development of education for management in the business schools then being established.

According to Cochran, there were no university courses in the study of business management until the last quarter of the 19th century, nor were there American textbooks or teachers, and probably no significant reading public for any such works in any nation. Up to that time trial and error was the dominant organizing principle, supplemented by personal experience shared among businessmen through correspondence and interaction. Education in engineering preceded managerial education, thus reinforcing the tendency of the "practical man of affairs" to take a simplistic, mechanistic view of business problems--an orientation which was characteristic of English businessmen as well as Americans. In fact, Cochran argues, a major factor limiting the growth and power of American business up to the 20th century was the scarce supply of competent managers, possessed of sufficient personal preparation and administrative training to meet the problems of coordination generated by increasing size and complexity in business. Thus it was during this era that on-the-job training, apprenticeship, and practical experience began to be seen as inadequate to the complexities of modern business management, a deficiency reflected not only in the establishment of business schools but in the rise of professional societies and associations as well, which served as educational bodies for their members. (Cochran, 1977, pp. 55-56)

The efficiency movement centered on the application of scientific method to business, and specifically focusing on the importance of the human factor, the application of scientific principles in labor management. The rationale was that the responsibility for good practices and systems belongs to employers, whose objective should thus be that the work be done with less effort, and less fatigue, and in less time--with the expressed intention of making work agreeable to each workman. The scientific method upon which this movement was based was defined essentially as the ability to generalize, to extract the essential kernel of a problem and to translate that understanding into efficiency standards. The procedure was to analyze the jobs to be done and the equipment and methods to be used for the purpose of standardizing methods and practices, and of determining an equitable wage, set forth in training and emerging as a new function of enlightened management--the "personnel" function. Scientific management, as the concept was understood by the Efficiency Societies, focused on providing equipment and training which would decrease the incidence of fatigue, and methods of recruitment, compensation, and performance recordskeeping which would serve to induce better work. (Western Efficiency Society, 1917, pp. 118-119)

A corollary benefit of this progressive approach to scientific management was that the efficiency of American production methods increased materially during the war. According to Farnham, this was partly due to increases in patriotism and partly to the pooling of experience and centralization of planning and control in governmental advisory boards--which provided an education in the "underlying principles of industry" for the participants. In this view, scientific management is the mechanism for fulfilling the redemptive ideals of science in industry as supportive of both individual and organizational growth. However, scientific management as institutionalized in Taylorism and embodied in the quantificational approaches of the Gilbreths and other time and motion specialists was oriented to countering the "soldiering"--or withholding of performance and productivity--on the part of production employees. Defining the problem of production in terms of "soldiering" was matched on the worker's side (given their experiences with the excesses suffered by British workers) by an increasingly conflicted view of the industrial process.

In the post-war era, those lessons of organization learned in wartime were put into effect in peacetime production, where wasteful competition was countered by consolidation and rationalization. In this context, industrial problems shaded over into social problems. Riots and plant takeovers in Italy, France, Germany, and Russia raised the specter of complete social-political breakdown--at the same time that industrial productivity was increasing markedly. Closely associated with this perceived social breakdown was the deterioration of industrial "relations", not only characteristic of labor and management relations, but extending to problems with "de-mobilized" soldiers whose adaptation to the conditions of war was not conducive to smoothing their return to the "normal" conditions of industrial work. Farnham attributes some of this unrest to cyclic transformations in expectations, expectations which also differ as to context. European labor, according to Farnham, "...expects to have to remain in the class in which it was born, and makes arrangements accordingly, while in America we all hope eventually to become millionaires and are not so much interested in changing the organization of society." As a case in point, Farnham then cites the initial rejection of scientific management in British factories. (Farnham, 1921, pp. 71-73)

British factories did adopt scientific management, however, and the postwar period saw a rise in laborer's expectations and in social unrest as employers in all of the major industrial powers reasserted managerial control in the face of emergent radical unions, plant takeovers, and strikes. In Italy passive strikes--refusing to work overtime, working to rule or systematic soldiering--were met with a general lockout at some plants. In return, the employees seized the factories. During the conflict, output first dropped during the passive phases of the strike, then rose during the period of seizure, and finally dropped off as problems of capitalization and commercial management--particularly the marketing of the goods they produced--undermined their physical control over the situation. (Farnham, 1921, p. 16) (Interestingly, according to Zwerdling (1978) these are the same conditions which have undermined the stability of factories run by workers' cooperatives in America during the mid-20th century.)

Radical demonstrations and strikes took place in 1920 in Germany and France as well as Italy. In Germany attempts by workers to seize the factories were undermined by the institution of worker participation in management which undercut the attempts of union leaders to organize more conservative workers, and which bound workers to their employers by reciprocal obligations for operating efficiency. The result was few strikes of but minor consequence. In France, outbreaks of organized worker unrest in May, 1920, were dispersed by the French cavalry. The French government did not at this time recognize trade unions, and attempts to organize a railroad strike in the spring led to the arrests of leaders and the introduction of heavy legal penalties, followed by the dissolution of the General Federation of Labor.

According to Farnham, among the conditions responsible for this general deterioration of working conditions and widespread unrest--which endanger industrialism with the "disease of Bolshevism"--are the following: 1) The "autocratic assertion of power" by industrialists, met by 2) unreasonable expectations on the part of workers who have come to have overconfidence in their value to employers because of the importance of their services during wartime. 3) The general reorganization from war to peace and the return from war to work also occasioned generalized unrest, including desires for the millenium and idealistic agitation by people seeking to improve their positions. (Farnham, 1921, pp. 82-84) It is interesting that Farnham does not mention the violent labor conflicts which took place in America during the 1920's, although those very conditions certainly characterized the United States following World War I as much if not more than was the case in Europe.

There were American writers of this era for whom "human factors" did imply an explicitly political theory of organization, containing within it an expectation of social reorganization of production in the interests of inducing better motivation and improving working conditions, both of which should lead to increased efficiency. For Eugene Wera, human factors meant nothing less than the passing of the old autocratic methods of management and the emergence of "cooperative democratic management", which would be founded on "voluntary submission of free men to discipline". Autocratic management is more than an opinion, he argues; it is a "system" which has principles, reflected in laws made by the ruling class and imposed upon the common laborer by force of authority, a system which derives from autocratic government. According to Wera, this system is opposed by the principle of democracy which holds that the state exists to serve social purposes, and that each man has some particular excellence which should be allowed to manifest itself. The major distinction between democratic and autocratic management, then, is the adherence of the former to the principle of consulting first and acting afterward, in contrast with the autocratic procedure which acts first and consults afterward--if at all. (Wera, 1921, pp. 116-117)

According to Wera, the value of production to the society at large, and the responsibility of the corporation to the public underly the mission of human engineering, the object of which is stimulating labor as a whole toward production at large for social purposes. Stimulating labor amounts to encouraging willing cooperation of workers in the interests of greater efficiency, an idea which shows up in Barnard (1938) and in Gulick and Urwick (1937) as the primary function of the executive. Wera attributes a lack of cooperation to autocratic management, which was generally adopted because it reflected the old military system of management, exhibited in industry in the flow of authority from ownership and managerial control and in the family in parental authority--a system which was adopted without reflection because it was the only available example of leadership.

Resistance to this mode of control arose when workers became accustomed to industrial work and, therefore, had come to identify with their vocations; when the supply and demand for labor in U.S. society reached a level of "equilibrium" laborers' aspirations grew into demands. According to Wera, at this point the battle between employers and the employed was engaged, and has been going on since that time, centered largely on unsuccessful attempts to settle on an acceptable meaning of fair compensation and fair working conditions. (Wera, 1921, p. 29)

Wera, like Farnham, attributes much of the discontent directly to aspirations which were raised during the War and then dashed in the post-war period which followed, giving rise to suffering and resentment "for actual wrong", which are the "primary stimuli of change in social order". Cooperation of labor was actively courted during the war, and the most advanced rights of labor yet enjoyed were established by the War Labor Board, following President Wilson's avowed desire for a "genuine democratization of industry".

> "And thus we gained the cooperation of labor in war. When these simple men felt that they were working for a great common cause, they believed that cooperation was established forever. But when the war stopped, virtually nothing was changed and their illusion vanished." (Wera, 1921, p. 78)

In England during the same period, although Wera does not directly mention the sufferings of the British munitions workers, he does attribute the rise of the shop steward movement to the loss of the right to strike during World War I, and the corresponding loss of authority on the part of union leaders who were subject to prosecution should they attempt to organize strikes. The rank and rile, therefore, reorganized, on the basis of "self-government", but demanded as well the right to increased participation in workshop management as the price of acceptance of scientific management. The shop steward movement accepted the need for fulfilling production requirements, but insisted on worker control over the process of production for the purpose of safeguarding the interests and health of the workers. (Wera, 1921, pp. 128-129)

Scientific management for Wera was a direct outgrowth of the inefficiency of the traditional chain-of-command approach to management, a system which passes responsibility for an increasing load of work downward from manager to superintendent, to foreman, to operatives, where it rests on "those who are least able to bear it", resulting in irregularities, mistakes, inefficiency, and misunderstandings. The chain of command as a basis for coordination was workable only so long as it remained within the realm of personal supervision and coordination of the craftsman as master of the shop, who embodied in his skill the means of production. However, such systems broke down quickly as organizations became too large and/or too diverse to permit such first-hand knowledge of operations, and the manager was forced to "deputize" his executive powers, losing control over the total process, and thus over working conditions. As a remedy for this state of affairs, scientific management systematized the process of management, now removed from personal supervision, replacing for this system the concept of "staff organization", also derived from military experience, which subdivides the functions of management in such a way that adequate instructions and orders can be provided in advance, and in writing from a group of specialists with expertise in that aspect of the process. (Wera, 1921, pp. 38-39) Thus we have early indications of the by-now-traditional controversies over line and staff organization--centralized or decentralized, hierarchical or network structures.

Scientific management is ineffective, however, in spite of its rationalization of the process of management, Wera argues, because it fails to elicit cooperation and therefore cannot alleviate industrial unrest. The reason is that "...management by experts is a bureaucratic autocracy. The secrecy and exclusiveness which surround its functions irritate workers and prevent them from thinking and from taking any interest in their work." (Wera, 1921, p. 43) This approach is paternalistic (which is the primary argument leveled against the human relations approach), and is motivated only reactively in the interests of workers by the realization that improvements would be profitable to the entire enterprise, and that factors such as "comfort, heat, ventilation, light, safety devices, lavatories, lunch rooms" and the like could be instrumental in increasing productivity. Wera points out a subtle dynamic which re-emerges much later in health hazards investigations. When improvements such as the above are motivated--or are perceived to be motivated--by a spirit of benevolent, arbitrary concessions, without community of purpose or consultation with employees, such concessions

> "...breed a feeling among working people that everything they have obtained has been fought for, that everything they contemplate obtaining has to be fought for, and that the more they fight the more they will get."
> (Wera, 1921, p. 33)

The result is indifference on the part of the employee, whose notion of adaptation is "getting the most money for the least work which will keep the job for the man". This indifference is met by an inability on the part of employers to gain control, which means for them "getting the most possible work for the least money which will keep the man on the job"--a classic stalemate. (Wera, 1921, p. 35)

In retrospect we would argue that in the U.S. this circumstance only served to reinforce the pressures to further mechanize and automate the production process, and to obviate thereby the need for labor as much as possible. This is significantly different from the infusion of political negotiation into workplace decision-making which increasingly was characteristic of the approach to the "human factor" in

British labor relations during this same postwar period. In the American labor reform movement, human engineering was early on conceived as a programme for action leading to cooperative democratic management, which according to Wera involves three strategic elements: 1) Cooperation was to be elicited by powers of exhortation and education. 2) In addition, human engineering implied a reorganization of management into teams of specialists, representing the staff functions of management and acting in the role of teacher and leader. A system of committees offers "endless opportunities" for the workers to cooperate in a constructive way in common public service, and to bring together representatives of different groups for purposes of education, collective bargaining, formation of public opinion and promotion of progress. This system also permits employees to voice their complaints and suggest ideas and otherwise to participate in shaping labor policies, all of which lead to a stimulation of the "willing cooperation of workers" in the interests of efficiency.

Efficiency would be reinforced by public presentation of performance records, and supported by management commitment to removing such harmful traditions as unpleasant and unhygienic working conditions, unstable employment, arbitrary and harsh supervision by foremen, individual wage bargaining and rivalry among workers, and careless or excessively narrow definition of duties. 3) Finally, Wera advocates adopting the emerging industrial innovation of the "employment manager" whose function it is to represent the interests of employees to management, and to handle human problems generally overlooked in the conventional emphasis on material factors. (Wera, 1921, p. 362)

Wera's programme is fairly representative of the ideals and methods of the Progressive movement in American history, which was motivated by a spirit of industrial and civic reform, spurred by the excesses of decision-makers in increasingly centralized and monopolistic institutions. These ideas--amounting to a broad view of human engineering as encompassing organizational and social issues--also emerges in socio-technical systems theory arising in Great Britain after World War II, and in the works of Likert, Argyris, McGregor, and others in the organizational behavior/organizational development school of organization theory who advocate organization structures made up of working groups or teams interconnected by managers functioning in liaison roles. Even Taylorism advocated replacing the old-style foreman by eight different "teachers", each with his own specialty, from whom workmen would receive orders and assistance. (Taylor, 1947, p. 122)

However, although the efficiency movement--with scientific management as a general model for organization management--included elements of management education, corporate reorganization, and an emphasis on workplace design and maintenance focused on issues of safety, the common thread underlying all these methods is that of increasing productivity, which can also be served by quantification alone--through recordskeeping for planning and control--without necessarily implying any of these other innovations. This we might call the narrow approach to human factors, which begins with productivity as the objective.

An emphasis on quantification and measurement emerged as a central element in World War I human factors engineering and became the strongest instantiation of management science, particularly in the education of managers. While Porter and Wera and other humanistically-inclined members of the Western Efficiency Society, and Progressives such as E.A. Ross might be concerned to improve working conditions and the lot of the worker, the straightforward importance afforded to quantification brought to prominence men like former military engineer, Frank Gilbreth, for whom measurement and quantification were the essence of scientific management and the basis for industrial progress.

> "No definite and permanent advance is made in any kind of work, whether with materials or men, until use is made of measurement. This is especially true of advancement of the human factor in industry, which varies so much that unless we use measurement and abide by the results, there is no possibility of repeating the process accurately and efficiently at will, or of predicting and controlling the future conditions that assure that advancement."
> (Gilbreth, quoted in Western Efficiency Society, 1917, p. 178)

The object of scientific management here is improving performance and increasing measures of productivity directly by better recordskeeping as the basis for control. Accompanying this focus on measurement, the efficiency movement emphasized human factors in equipment and workplace design, made necessary by manufacturers' overemphasis on machinery and material and their neglect of the human factor in management. The human factors curriculum at Purdue University in 1917 included courses of study in 1) heating and ventilation; 2) sanitation, hygiene, and first aid; 3) factory design; 4) lighting, and 5) strategies for handling unions, labor problems and wages. The factor of lighting had been found to exert a "decided influence on production" and in some lines of manufacture it was appreciably implicated in problems of fatigue. (Western Efficiency Society, 1917, p. 81)

Thus the study of human factors as it arose in the United States in the World War I era in response to events taking place in Europe as well as to working conditions at home--was closely associated with the rise of "scientific management", which represented a rationalization of older management practices and an improvement of equipment and working conditions designed to make it easier for employees to adapt to their work. In this era, however, both management science and human factors engineering were interpreted broadly as fields which included issues of work design and organization management as well as more conventional "hygienic" aspects of industrial control and coordination.

Human Factors in the World War II Era: A major transformation in the orientation of human factors research came about as a consequence of the increased complexity of problem-solving during World War II. Prior to that time, work measurement and performance standards focused on the selection of personnel on the basis of characteristics which would meet the requirements imposed by the system. The approach was, therefore, that of "fitting the man to the system". In World War II, however, increasing complexity of the equipment--and the uses to which equipment was configured and directed--required greater levels of operating skill. At the same time, universal conscription supplied human labor in "masses", making it impossible to rely on selection alone to fit the person to the task, although it greatly benefitted measurement and data collection. Machine characteristics, therefore, began to be modified to allow for limitations of operators, and in this orientation we see a great shift in the definition of objectives for human factors research in the direction of fitting the machine to the person. According to Mesiter and Rabideau,

> "Paradoxically, in the concern for human error in the data-gathering process, the substantial machine error that may enter into the use of highly complex instruments is apt to be overlooked."
> (Meister and Rabideau, 1965, p. 189)

Where the (traditional objective criterion for human factors research is the reduction of error in performance and the identification of design alternatives which affect performance as a basis for selection, the sources of error in the man-machine system are easily overlooked. Given the complexity of such machine systems, human error appears in a slightly different light, in World War II accounting for some 40% of the problems in missile testing, 63% of the shipboard collisions, flooding and grounding, and according to U.S. Air Force reports, "human error was responsible for 234 out of 313 aircraft accidents during 1961." (Meister and Rabideau, 1965, p. 17)

Meister and Rabideau consider human factors research and human factors as a separate discipline a creation of World War II. During World War II remarkable progress was made by the medical corps in reducing the death rate due to wounds to less than half the proportion suffered in World War I, and in maintaining the military population in a state of health which "...compared favorably to that of the civilian population." (Morison, 1972, p. 367) Baxter attributes the good health of American fighting men in World War II to the efforts of medical and psychiatric personnel who increased their chances of survival by diminishing the risk of infection and reducing the hazards of combat fatigue. (Baxter, 1946, p. 105)

There were medical problems, however, which arose in adapting to new equipment developed in this war, and which exceeded the adaptive capabilities of human physiology--even as extended by appropriate training and selection procedures and medical care. Problems associated with new types of aircraft, especially, stimulated cooperative research between flight surgeons and physiologists and equipment designers and manufacturers, both in the U.S. and in Great Britain. In this way human factors research came to be officially included among the missions of World War II operations research.

New types of problems associated with new technologies and new strategies raised physiological questions for which civilian scientists at the beginning of the war did not have answers. There was thus a need to establish research programs in this area to develop information necessary to effective design and institution of procedures, tactics, and training programs. Few civilian scientists had experience with problems of military aviation, or had conducted research on night vision. However, basic principles of the physiology of vision, for example, were known, such as the characteristic responses of the rod cells and cone cells of the eye to dim light, and researchers applied available knowledge to problem-solving for the design of new equipment and new practices. Given that the rod cells used in night vision are located in the outer regions of the retina, vision of a dim object is improved by looking away from it a little bit; therefore, better lighting systems were designed for aircraft cockpits which used wave lengths of red light to enable cone vision for precision viewing within the aircraft, without interfering with rod vision for distant, dim objects outside the aircraft. (Baxter, 1946, p. 387)

This example points up two characteristics of human factors research in this context: an emphasis on equipment design and improvement, and an emphasis on developing effective procedures and practices. In responding to problems of lack of oxygen for pilots flying new aircraft with higher ranges, researchers initially tried to teach fliers something of the physiology of flight, to recognize the sensations associated with oxygen deprivation and to take avoidance measures. The possibilities for improvement of human physiology are limited, however, and human factors research increasingly made use of special equipment--simulators and other measuring instruments--to test the effects of equipment during design, and to redesign existing equipment so as to minimize the adverse effects on the person using it.

Thus the practical purposes of human factors investigations in complex man-machine systems are extended to 1) improving the design characteristics of engineered systems and 2) verifying that behavioral or performance factors in that system meet the specified performance requirements. "Human factors evaluation, therefore, encompasses not only the testing of the system in its final configuration but also the testing of the original design concepts and initial design products." This process of evaluation requires "following design from its requirements stage to that of the released blueprint and interpreting at each stage the correctness with which principles of man-machine design have been applied." (Mesiter and Rabideau, 1965, p. 17)

Meister and Rabideau argue that "the importance of the relationship between human factors and governmental systems cannot be overemphasized", and suggest that the growth of human factors as a discipline is largely due to the relationship with government. During World War II, the Committee on Medical Research was established within NDRC in July, 1941. The membership of the CMR included representatives of the Army and Navy Surgeons General and Public Health service and four appointed civilians representing the University of Pennsylvania, Johns Hopkins University, Columbia University, and Harvard. The mandate of the committee was to save lives of the troops directly by improving health care methods for the sick and wounded and indirectly by preventing disease, and by minimizing the adverse effects of new equipment.

As Chairman of the Division of Medical Sciences of the National Research Council, Dr. Lewis H. Weed of Johns Hopkins University organized a Committee on Aviation Medicine in Fall of 1940. This committee visited aircraft factories and held conferences with designers and strategic planners to identify the stresses on the human body which would be associated with new equipment. These people laid the groundwork for the CMR, which was created the following year. Linking these various committees, Dr. Detlev Bronk was simultaneously Coordinator of Research for the Office of the Army Air Force Air Surgeon, a member of the Committee on Aviation Medicine, and Chief of the Division of Aviation Medicine in OSRD. These strong ties facilitated analysis of practical problems, the results of which "were quickly translated into new equipment and operational procedures."

quickly translated into new equipment and operational procedures." (Baxter, 1946, p. 379)

Much of this research made use of simulators, machines which would reproduce the local environment for test of human capabilities in designed equipment. A human centrifuge was constructed; pioneered by Canadian Air Force scientists, this machine made it possible to record precise measurements of various physiological reactions to forces of specified magnitude. At Wright Field a human factors laboratory was established to study equipment design and operation for projects such as the automatic triggering mechanism for tracking enemy fighters, based upon the lessons learned in antiaircraft research. (Baxter, 1946, pp. 391, 403)

A common element in the methods of human factors research is a focus on the requirements of the jobs that people will be doing together with a measurement of the characteristics of the people who will be doing them. This information can be used in the design of equipment and procedures, and in the selection and training of operators. According to Baxter,

> "In this respect research on personnel problems is exactly like research on material problems. In deciding upon selection, tests, training methods, or operating procedures, it is just as necessary to try out alternative ideas, to measure the results, and to select those that work best. Materiel research is necessary to develop new weapons. Psychological research is necessary to ensure their most effective use." (Baxter, 1946, p. 403)

In research on selection and training the orientation is twofold: 1) Equipment can be studied and modified so that it is better adapted to the capabilities and limitations of the average person who will be using it, a type of research that is most effective when it takes place at the pre-production stage of development when changes can be incorporated in the design to make its control and operation easier. 2) Or this information can be used to guide recruitment and assignment, and thereby to avoid misassigning personnel to jobs for which they are not suited. The Navy led in research of this type, requesting the assistance of psychologists to study the performance of different individuals on watch duty. Operations research methods were used in a testing program on board cruisers in North Atlantic convoys; records were kept on the performance of each person from which it was determined that "the best man could spot a ship nearly four times as distant as that observed by the poorest lookout", a finding used for selection and assignment. (Baxter, 1946, p. 395)

Industrial psychologists were increasingly recruited for this type of research, having volunteered their services to the war effort in World War I. The Navy and Army both requested assistance in improving methods of classification and assignment, and in assisting with training for operators of specialized equipment. Formal requests for these services resulted in the establishment of a committee with the NRC on Service Personnel, Selection and Training, which subsequently became the Applied Psychology Panel of NDRC. The major efforts of that committee were directed to selection, classification and training, stimulated by a recognition of operational problems in different types of task. Tests were devised to measure aptitude for learning radio code, for example, or for a person's ability to be a good telephone talker, and the results entered on his permanent record. Training was particularly important for operators of radar equipment, and it was found that it was actually sometimes the case that training made people worse rather than better, placing the deficiency clearly on the training, and stimulating work in the Applied Psychology Panel on fundamental learning principles. (Baxter, 1946, p. 400)

Postwar extensions of human factors research in the United States tended to be focuses in the automobile and aerospace industries--where extensive use was made of the type of simulator pioneered in aviation research. According to Meister and Rabideau, the majority of human factors personnel in the U.S. in 1965 were employed by firms contracting for the U.S. government, particularly in the military and the space agencies, such as NASA, a group of specialists of which 60% are psychologists, 30% engineers, and the remainder physicians, physiologists, and anthropologists, computer specialists and mathematicians. In 1965 relatively few human engineers were employed in private or commercial industry, primarily because commercial products at that time were not machines or systems. (Meister and Rabideau, 1965, p. 11) However, the introduction and widespread use of computers, as an especially complex type of machine system, requiring systems adaptations of the companies in which they are installed, is bringing human factors research approaches into user organizations as well as the design of computer systems. By extension, the conventional form of human factors research based upon the theoretical knowledge and research methods of the biological sciences, physiology and medicine, along with systematic operational analysis of equipment use and environmental specifications has been the model for U.S. government health hazards investigations which attempt to account for specific health hazards in working environments in terms of a combination of physiological and psychological factors and operations records in ongoing environments.

There are problems with extending the scope of human factors investigations, based upon a focus on adapting the individual to designed systems, or even adapting systems design to individual users, which cannot resolve the source of errors, nor the resolution of those errors in work design and organization. In the context of complex man-machine systems, human error is in many cases a consequence of error in the prior design of the system, which cannot be identified in measures of performance alone, and in these circumstances "workmanship deficiencies can be empirically related to system inadequacies," for example, the use of out-of-date or inaccurate blueprint information, inadequate equipment, or inspections standards which do not identify incorrectly functioning equipment. In other cases, poor lighting, inadequate storage, complexity of new equipment design or production technologies, speeded up schedules and overloading all can increase the probability of human error. In such cases "the fault lies with the production process rather than the workers". These factors, however, are "usually beyond the scope and responsibility of most human engineers."

Thus the objective of human factors research, defined in terms of reducing human error in the performance of tasks in complex systems in the interests of safety and reliability leads beyond the traditional focus on 1) fitting the man to the machine and 2) measurement of perormance, given machine characteristics, to an evaluation of the total system, including design. However, the full examination of human factors in the broader context of complex engineered systems is inhibited by the orientation of the field, which Meister and Rabideau describe as a design support tool. They argue that for most engineers, design is still the primary concern, and human factors a secondary adaptation, reflected in the fact that few designers or human factors personnel have more than a passing acquaintance with the production area. (Meister and Rabideau, 1965, p. 276)

In American context of development, operations research and human factors has been identified with designer-based methodologies, which separate considerations of design from those of implementation and use. In the British context of development, postwar operations research came to be more closely identified with a broad view of akin to Boothroyd's "articulate intervention", which places design considerations in the service of social and organizational objectives rather than the reverse. In the U.S., operations research came to be identified with the narrow view of systems engineering, reflected in an equation of management science with applied mathematics, extending organizational control through an emphasis on quantification and automation--which largely leaves unspecified the role of the human being in the system, and certainly does not include the more political orientation to social and organizational issues that was characteristic of American human factors approaches in the World War I era.

In Breat Britain, operations research came to be identified more with the broad view of systems engineering upon which socio-technical systems analysis is based, an area which emphasizes issues of organizational design and learning. Most socio-technical systems analysts trace the origins of this discipline to Trist and Bamforth's 1951 research on the technology of longwall coal mining in England. This research was initiated by psychiatric reports of "an epidemic incidence of psychosomatic disorders" among miners whose working relationships had recently been reorganized The introduction of mechanization in the interests of increased productivity was accompanied by a transformation of small self-regulating work teams in which each individual performed a whole task, into larger units made up of independent task-related teams. This transformation created conditions in which efforts on the part of each group to optimize working conditions for itself resulted in creating adverse conditions for others, thus setting in motion "insoluble conditions for interpersonal and intergroup conflict" which ultimately resulted in low levels of performance. On the basis of this investigation, researchers came to think of performance as a function of a mutual interdependence between social and technical factors in production. Given this perspective, it follows that outcomes will be sub-optimal whenever the technological or the social system is optimized at the expense of the other, and that human relations techniques for conflict resolution will be ineffective in resolving conflicts built into work organization. (Herbst, 1974, pp. 3-4)

Included in socio-technical systems approaches are organization theories--such as those of Burns and Stalker (1967) or Joan Woodward (1963)--which attempt to account for resistance to new technologies, and to organizational development as it was sponsored after the war by governmental agencies attempting to transfer wartime accomplishments to civilian industrial development. Early approaches to socio-technical systems analysis assumed that a given form of work organization is appropriate to a particular technological system, from which it followed that problems and conflicts could be resolved by redesigning that system to achieve joint optimization of technical and social requirements. Social-technical systems inquiry and design as an active, directed mode of research advocating the experimentation and implementation of "autonomous and composite types of work organization as a basis for extending the participation of workers in decision-making" was the basis, particularly in Europe where political conditions were favorable, for carrying out social and technical experiments in a number of industries in Norway, Holland and Ireland, through the Industrial Democracy Project. This research demonstrated that changes could be induced in work organization by initiating primary changes either in the social, economic, or technological systems of that organization; however, the bulk of this research has been applied to studying a given technological system in order to design a "more appropriate correlated social system".

Herbst argues that two major problems arise at this point in the programme of socio-technical systems analysis and design: 1) modern technological systems are intentionally designed to decompose jobs to the simplest repetitive components requiring a minimum of training and discretion on the part of workers; and 2) much more effort and "working through" is required to implement changes in existing working organizations than is implied by a study of technological feasibility alone, as field researchers found out by experience. (Herbst, 1974, pp. 5-6) Just as informal engineering can be distinguished from systems analytic design methodologies, human factors engineering was significantly different in the early World War I era and in the World War II era. By the end of World War II ergonomics came into currency in England to reflect these broader social-psychological issues which human factors engineering could raise but not explain.

The Ergonomics Research Society was founded in July, 1949, out of the perceived need to "continue the successful wartime collaboration between the various relevant disciplines. The society originally included representatives of the British military operations groups, including the Naval Motion Study Unit, the Applied Psychology Research Unit, the Institute of Aviation Medicine and the Army Operational Research Group. This society first met under the provisional title, the "Human Research Society" but the term "ergonomics", proposed by Prof. Murrell, was chosen to represent the group because it did not imply an exclusive orientation either to physiology or to psychology or funcational anatomy.

The impetus for forming this group came from the recognition that the results of wartime operations research collaboration could not have been obtained within any one discipline, and the feeling that, no other satisfactory "institution" existing in the civilian society, some sort of society was needed where collaborative work could be undertaken and the results easily communicated. The example of fruitful collaboration between physiologists, psychologists, anatomists, and engineers and designers had worked to such mutual profit during wartime, that many people were interested in carrying this type of multi-disciplinary work into peacetime studies in universities and in industry. (Edholm and Murrell, 1974, p. 14)

The form of organization for the Ergonomics Research Society was patterned on the model of the learned society, with its emphasis on scientific meetings where research results could be entertained, and included a view toward communicating practical results to industry. The first conference was organized on the theme of Human Factors in Equipment Design, drawing on the participation of physiologists and psychologists from abroad, and this first conference took the form of a scientific programme which reported research but which made little attempt to apply that research to industrial objectives, which met with some criticism, and was considered by some in the group to be a major handicap to furthering research interests.

The early work of the society built on work done during the war by the military, drawing heavily on personnel research carried out by the Service Personnel Research Committees, which fielded studies involving physiological and psychological methods of research which attempted to ascertain

> "...the best means of increasing the operational efficiency, safety and comfort of soldiers, sailors and aviators under different environmental conditions and, conversely, the adaptation of ships, fighting vehicles, aircraft and weapons to the convenience and capabilities of those who have to use them."
> (Edholm and Murrell, 1974, p. 13)

The work of the society in the postwar period was therefore oriented to adapting research methods developed in wartime to peacetime production problems of the "human factor in industry".

Although the first conference was enthusiastically received by the scientific community, which welcomed the sharing of information from research that formerly had been classified and unpublished, unfortunately, "Acceptance of the concept of ergonomics amongst industrialists has been much slower." The authors suggest that one of the reasons for this disparity in interests may have been due to the fact that few scientific members had had any experience in industry, and they found when contact was established that it would be necessary to assess the value of his work "in terms of money: how much would be saved as a result of introducing methods based on his findings." (Edholm and Murrell, 1974, p. 15)

A second symposium was organized at Oxford in June, 1952, this conference attempting to elicit industrial interest by focusing on "The Measurement of Human Performance"; and the third annual symposium was directed to the "Scientific Study of Human Work in Industry", with papers presented on physiological measurement of work, measurements of heat stress, effects of working in underground factories, and accounts of studies of performance on repetitive work and thermal comfort. Out of this orientation, although the idea of creating an "applied science of ergonomics" was not envisioned at the time the society was founded, the idea emerged (almost immediately) that "an ergonomist can be an individual applying research results rather than being engaged on research...." (Edholm and Murrell, 1974, p. 8)

Among the earlier civilian examples of the work of the society was a study concerning working conditions in the post office, which emphasized the necessity of making accurate assessments before undertaking improvements in working conditions. However, the reluctance of industrialists to fully support this type of multi-disciplinary scientific research, and the generally reduced state of resources for undertaking such projects--limited in terms of their potential profitability--left the field open for other applications of this type of operational research. The clients for postwar applications of operations research in Britain, therefore, emerged in the unions, not in management, leading to the predominant labor-orientation of the British socio-technical systems movement.

As was the case in the early emergence of human factors research in the World War I era, the rise of British socio-technical systems theory came about as a consequence of perceived failures and hazards in complex systems, hazards brought to light through the resistance of workers to new technologies and methods of work. Where mechanization involved the transformation of methods of work and the organization of working groups, resistance to new technologies has tended to center on issues of safety and health hazards. In socio-technical systems approaches, the resolution of these industrial conflicts entails the recognition that social, technical, and economic forces all are interrelated in the development of any organizational system, and hence that a change in any of these factors will have repercussions for all the others. (Khandwalla, 1977, p. 231)

In distinguishing ergonomics from human factors engineering we have seen two major emergent factors involved in systems engineering as the application of scientific method in industry: 1) the "organizational" factor so noted by participants to the operations research movement and incorporated in new methods of research and development in postwar environments; and 2) the "human" factor, which emerged in the recognition of a set of "side-effects" associated with designed systems, which are not accounted for in the design or the design process. The degree to which both the organizational and the human factor are integrated into processes of system design and implementation making up the whole of system development describes two fundamentally distinct orientations--a broad and a narrow approach to operations research and systems development--which are reflected in correspondingly broad and narrow approaches to human factors engineering.

Once systems become so complex that they must be designed with a model of the environment, and the human operator, built into the specifications, the scope of human factors research necessarily broadens to include the entire design and implementation cycle, rather than being a secondary adaptation following from the process of design. The difference is in thinking of design and implementation as mutually reinforcing and interactive aspects of system development, or in viewing system development as made up of two essentially separate processes--design and implementation, perhaps carried out by different people in different departments, or even organizations. It is becoming increasingly common, in fact. for external consultants to participate in processes of implementation, thus moving responsibility for this type of management function outside the firm altogether.

The fundamental limitation in systems analysis and design in a designer-based mode lies in considering implementation a residual and secondary function of design, which follows and therefore derives from the design stipulations, presuming, therefore, that the burden of adaptation falls upon the "user" of the equipment or procedures. This approach entails inherent uncertainties, which cannot be discussed or resolved within a rigorous designer-based systems engineering approach. Thus as the thrust--and complexity--of research objectives has changed, the orientation of research, design and implementation developed in Britain and subsequently in the U.S. a broader conceptualization of the role of operatives in defining the working process, which is reminiscent of the emergence of the shop steward movement in England and the progressive movement in the United States in the World War I era. With the formation of the Human Factors Technical Group on Organizational Design and Management in 1981, the discipline has come full circle to address issues first raised nearly a century ago, and of relevance to the issues of office automation being confronted today.

Union Approaches to Computer Ergonomics:

It is interesting that problems in the implementation of office automation technology are focusing on concerns for the health and well-being of clerical employees working with computer-based equipment, and it can be argued that this orientation is an artefact of union approaches to computer ergonomics which is based upon the history of industrial hazards and human factors research just reviewed. Although women in general, and clerical and secretarial employees in particular, have been characteristically resistant to unions in the U.S., and while union membership and participation have declined since World War II, a combination of conditions--to include:

1) thwarted expectations; 2) routine, machine-based organization of work; and 3) the centralization of numbers of office workers in service locations--has been the occasion for a resurgence of union activity and for various spontaneous expressions of dissatisfaction within offices and in the popular press. Concerns have been raised over the radiation emissions from video display terminals, which have been faithfully reported in the media. The health issue is a subject of research currently being undertaken by unions, in the U.S. to include AFSCME (American Federation of State, County, and Municipal Employees), CWA (Communication Workers of America), The Newspaper Guild, SEIU (Service Employees International Union), UAW (United Auto Workers), "9-5/Working Women", and a number of European clerical unions.

In Europe, clerical unions, in cooperation with government agencies, have conducted human factors research on office automation and much of this research has been incorporated into governmental legislation and regulation, especially in West Germany and Scandinavia, and to a lesser extent in Great Britain. Union approaches directed to American office workers have emphasized health issues and health hazards research, advocating such practices (in common with their European counterparts) as limiting employee time at computer terminals to 2½ hours at a sitting, and prohibiting pregnant women from using vdt terminals at all.

In response to public complaints and requests for investigation largely initiated by unions--first among them the Newspaper Guild, both in Europe and the U.S.--the National Institute for Occupational Safety and Health (NIOSH) undertook a series of on-site health hazards investigations in offices. They reported that, although the fear of radiation was not warranted at present, given current equipment, other physical factors in offices were potentially--and in some cases, demonstrably--adverse to human health and safety. Most serious were problems having to do with lighting and vision in conjunction with viewing video displays; muscular-skeletal problems associated with sitting in relatively fixed positions at workstations; and problems associated with airborne contaminants in the larger office environment.

In the course of their research, NIOSH investigators reported additional findings of high levels of fatigue and stress involving social-psychological "stressors" in the working environment of offices, associated with the definition of the work--or the application of computer technology. These findings are problematic, first because they exceed the scope of traditional human factors engineering methodologies for studying human-machine environments at the broad organizational level; and second, because the phenomena reported have in some cases a particularly systemic quality, associated not just with the environments of work, but with the dynamic qualities of change and conflict within those environments.

NIOSH researchers noted dissatisfactions with the structuring and scheduling of work and with the lack of opportunity for mobility, expressed in complaints over the "dead-end job". Jacqueline Messite reports on an investigation in which inquiry and problem-solving initiated by workers' complaints, when solved led to an increase in those complaints. She argues that once a health concern is triggered the problem becomes social as well as toxicological, and it is important to look for changes, for agents or processes which are new or different, including hardware and chemicals, work methods, and air conditioning systems. In some cases, complaints generate or represent a crisis of concern within the organization that cannot be resolved merely by removing the offending agent; although specific environmental problems may trigger such concern, that concern may extend beyond the specific agent to reflect underlying dissatisfactions which are associated with the perceptions of health problems. Under these circumstances, Messite argues, a triggering event occurs which leads to increasing concerns and anxiety, which lead to a spread of complaints and informal discussions. Once begun, any change in the workplace can then work to trigger another event in a continuing cycle of environmental complaints and interventions. (Messite, 1981, pp. 31-32)

This phenomenon indicates the systemic nature of problems in office automation, and suggests that the type of problems being experienced are deeper and more complex than it is possible to explain within traditional theories and methods of organizational research and development, particularly within the traditional human factors engineering model associated with the design and management of technologically-induced change in ongoing organizations. Conventional human factors methodologies have been relatively successful in accounting for two of the three areas of ergonomics and implementation problems outlined in NIOSH studies and other (largely union-based) research endeavors: health hazards associated with video display terminals, and health hazards associated with office environments in general can be studied in a relatively straightforward fashion with conventional methods. The issue of stress-related health hazards associated with the definition, organization, and supervision of office work does not lend itself to quantitative problem-solving strategies, however, although there are indications that stress leads both to physiological problems and to poor decision-making--both of which are constituents in measures of productivity.

Stress has been defined in several ways: Working under stress" implies a definition of stress as some measure of job complexity or workload. Other definitions identify stress as a physical reaction to the effort required to maintain improper or inappropriate physical positioning during task-accomplishment, in which case stress is equivalent to physiological strains. Other researchers have identified stress as a social-psychological problem, which is involved with working conditions, and with the nature of the work and the amount of control or discretion involved in that work for the individual operator. A hidden definition of stress not explicitly named identifies stress as alienation; not only are workers under the physical strain of heavy workloads and uncomfortable physical surroundings, but stress is generated by jobs which are defined and organized in such a way that they have become monotonous, meaningless, and offer no discretion or career mobility for their occupants. It also appears that stress is an intervening factor in the experience of other--more concrete physical and physiological problems such as back ache, irritability, stomach problems, headaches, and interpersonal conflicts.

One characteristic of directed, or goal-oriented research is that the findings do not always support the original, narrowly defined research objective, but raise instead totally different patterns which go beyond the logical and definitional capability of the original hypothesis or model to account for them. In an earlier era, the Hawthorne studies "went looking" for human factors which necessarily referred to individual physical and performance capabilities, and found instead social factors which could not be sufficiently accounted for in a social-psychological model which referred only to individuals. The result has been that human relations models have failed to be confirmed by test, the needs they identified in individuals have yet to be demonstrated empirically, and, by extension, the methods derived from these models have yet to elicit the high motivation and associated increases in productivity they predicted.

The orientation of research in the area of computer ergonomics and office automation appears to be taking a similar path. Research has been goal directed by being initiated by a set of complaints indicative of problems in people's mental or physical health and well-being. Research is justified today--as it was in the early 1900's--on the argument that these problems interfere with productivity, and thus that their solution is cost-justified. In looking for the causes of complaints, researchers focused first on the hardware--particularly vdt's--involved in the man-machine relationship. They found: 1) that vdt use does not present a radiation hazard distinguishable from background radiation;, and 2) that vdt use does not necessarily cause eyestrain if the workstation is properly configured and if eyesight problems are first corrected.

The best documented, and most theoretically-grounded area of research appears to be in the field of vision, which has been studied extensively in industry throughout the 20th century. Problems do exist, as demonstrated in NIOSH research, in the design of vdt's, including problems with luminance (screen brightness, contrast, and sharpness), flicker and glare and reflection problems. Beyond the unit itself, vision problems are just as frequently brought about by improper placement of machines with respect to the ambient lighting in the workplace environment. It appears, in passing, that the use of fluorescent lighting is itself a problem, in conjunction with the use of vdt's especially. Researchers for the Committee of Vision for the National Academy of Sciences found that vision problems reported by operators are associated with changes in the function of the eye. In myopia, these changes appear to be transient and temporary, and such changes may, indeed, be especially associated with the use of vdt's. Vision problems also relate directly to the number of hours an operator works at a vdt terminal, suggesting guidelines for the organization of work, which is the approach taken in Scandinavia and other northern European countries. Other vision changes are seen in the incidence of glaucoma, in which changes are not transient and which thus represents a disabling effect; however, glaucoma appears to be related more closely to microwave radiation than to radiation in vdt operation.

Research is continuing in both of these areas, but it is a particular difficulty of this type of research that the subjective reporting of complaints must be connected with physiological changes and with the nature in which the technology is used. At the practical level, researchers conclude that there may indeed be vision problems associated with the use of vdt's, especially for older persons, and they recommend that eye examinations precede this type of employment, and that glasses be provided to operators.

3) Elimination of radiation and vision hazards leaves ergonomic factors having to do with the adaptation of the physical environment to support a range of physiological requirements. One surprising finding was that a significant percentage of complaints--especially those having to do with respiratory ailments, irritability, and skin rashes--are attributable to physical causes, not in the man-machine relationship, but in the office environment at large, in the form of airborne pollutants and contaminants. A second area in which a significant proportion of complaints are found relates to muscular-skeletal problems associated with desks that are too high, rigid placement of terminals, and chairs that are not easily adjustable to a height accommodating the level of the work and thephysical size of the operator. These problems account for much of the backstrain reported and for associated irritability and for some eye strain. However, the complaints did not end when these problems were corrected.

4) The strongest finding at this level shows that the greatest incidence of physical and mental problems and complaints is associated with long periods of use of the terminal, which implicates social variables in the organization of work, which goes beyond the scope of conventional human factors explanations. At this level, there are

significant methodological problems associated with conducting research wich must connect practical problems, subjectively reported, with laboratory experiments subject to constraints of statistical significance and experimental control. The investigation of subjective complaints embeds scientific inquiry in a public context where the advocacy of one point of view makes it difficult to maintain standards of validity as the investigation becomes politicised, which it inevitably does when findings are used as legal grounds for collective action and for governmental regulation and setting of standards. The differences between the thrust of research in Europe, where workers using vdt's are already represented in clerical unions, and in the U.S., where the majority of clerical workers are not unionized, is particularly striking in this regard.

In fact, we have argued that methodological limitations in conventional systems engineering and human factors engineering are especially important in the study of computer ergonomics. The "information crisis" is a consequence of our inability to anser certain questions necessary to a model of articulate intervention in the implementation of computer technologies in office environments. Such a model would necessarily have to account for the full range of social and technical factors entailed in any explanation of ergonomic phenomena associated with computer implementation in offices, including:

1) Equipment: Current equipment exhibits a number of characteristics. Central processing units are powerful and reliable, and the remarkable decline in the relative costs of equipment in comparison with computing power is due to continuing development in the speed

and storage capacities of the hardware. Current problems have drawn attention to the effects of using video display terminals; however, more significant--and implied in the man-machine relationship even where vdts are not used--is the factor of computer architecture in accounting for ergonomic problems in office automation. Issues concerning the configuration and functionality of computer technology center in three areas: a) data base organization--i.e., the organization of information within the computer system, corresponding with the organization of information representing the firm. b) Human interface issues refer to the development of controls and commands--as part of hardware and software design--which allow people to use the equipment as a tool without having to know how it operates internally. c) Problems in software development involve the reliability, usefulness, or "functionality", and efficiency of systems in producing information--problems which are frequently implicated in social conflicts in the workplace.

There is a wide disparity in the level of development in the various components of computer technology. While great advances have been made in speed and storage of central processing units, input-output peripherals are expensive and plagued with problems, including problems iwth the fidelity of reproduction, the durability of the equipment, as well as health-related problems associated with the use of vdt's and printers. Problems with peripherals implicate software as well, in which limitations on the capability of equipment and/or software may result in major manual operations which are necessary to produce information in a form which the system can accept, and to take output from the system in a form which is acceptable to users.

2) Applications: The development of increasingly "higher-level" languages of use, with the ideal of natural language translation by machine--i.e., machine processing of languages--while central to the accessibility and functionality of the technology in organized contexts remains at a primitive level, far below the kinds of application which have long been conceived for office automation. We still lack the ability to fully generate expressions or to develop and display reflexive understanding in system self-knowledge, which would be fundamental to the operation of truly intelligent systems capable of interactive inquiry. In part this limitation is due to the as yet rudimentary development of data base design. We can now reproduce expressions fairly faithfully in various media, if we first provide systems with a completely formalized and grammatically correct input, and a dictionary or data base which can match any possible utterance which may be input. Clearly the way to manage this requirement has been to limit the possible utterances or requests which can be presented to the system in the definition of input functions. We can also analyze and compose expressions fairly well within severely bounded frames of discourse and given complete and reliable instructions for use.

Beyond these accomplishments, however, the complexity of expression in natural language use--language behavior, in Narasimhan's terms--and the uncertainty generated by the choice of terminology (including levels of reference and interconnectivity in the information being represented) can easily overwhelm the information-processing capabilities of machines, demonstrating the necessity for a model-theoretic grounding of software design in linguistic analysis.

A major issue in the development of software--computer languages and applications--is the level at which language is formalized, an issue which affects the levels of skill required in the user population and thus the accessibility of the technology to a broad range of users. Closely related are issues of syntax, or the rules for organizing information in the language-of-use, upon which is based the capability for structuring the data base and the information which can be produced from it to "capture" variables of interest in any problem situation. The accessibility, ease of use, and ease of learning associated with computer technology are expressed in the notion of "user-friendly" design and implementation. However, there is a limitation to the tailoring which can be performed on a designed system in ongoing environments. In computerization, bottlenecks in software development, in artificial intelligence research, and in attempts at natural language processing by computer all illustrate the parameters of conventional inductive research. These limitations are a significant factor underlying problems in the use of computers. Uncertainties and unresolved problems in applications arising from the narrowness of the conventional research and development paradigms can increase the mental workload required to complete certain tasks--thus constituting a stress on the system and creating strains for operators.

3) Ergonomics: Physiological explanations of mental workload and stress in various working environments would be necessary to account for health hazards associated with computerization. However,

we do not currently have sound theoretically-grounded explanations for those effects which might account for the full range of physiological, cognitive, and organizational factors in explaining outcomes of use for human health and well-being. As Dainoff has noted,

> "...(E)vidence of causal linkages between specific ergonomic attributes of the workplace, and specific patterns of symptomatology are lacking."
> (Dainoff, 1981, p. iii)

Current research is producing information in the following areas, however, necessary to developing such explanations: a) Conventional human factors research has investigated the nature of pattern recognition and the ease of recognition associated with the display of symbolic information. Research has also been conducted over the years which details the limitations and capacities of human beings, and human physiological systems--especially vision, which has been extensively studied in industry since the 19th century. b) Psychological research has been carried out on the nature of learning processes, also for decades. Much of that research, especially that of the Simon, Newell school, has been restricted to the experimental study of pattern recognition, limited to relatively artificial laboratory settings, and to "nonsense" learning of paired lists and other restrictive pieces of information divorced from ongoing contexts of use. The understanding gained is, therefore, relatively simple-minded when compared with the requirements of real-world environments, and is not at a level of sophistication necessary to explain the relationship between adverse health effects--such as coronary heart disease, for example--and various aspects of office automation.

We know much less about the interactions of those physiological limitations with cognitive, motivational, and attentional factors in perception and recognition; and we are at the beginnings of research into th emanner in which the human brain generates and processes information--particularly symbolic information--implying some additional theory of "mind" or "culture" to account for shared patterns of thought. Experimental schools of research on learning processes and the functioning of the brain--such as the work of Pribram and others--entertain broader concepts of knowledge and information than conventional pattern recognition experiments, and share with earlier versions of psychology (in the work of Dewey and Mead) the central premise that learning is integrally bound up in involvement and action in the environment through participation and reinforcement of events.

c) Extensive research is probably going to be required into the nature of reading processes, and literacy in general, as important in office work, and as significantly implicated in problems in the use of video display terminals. Kolers and others have pointed out that the person "adds" something to the perception of symbols in reading text. This insight may prove to be important in designing screen formats and commands by which users interact with computer systems in the processing of information.

The question of what is added--on the one hand to the data, and on the other hand to the formal models for organizing that data--points up a major finding in contemporary human factors research which is important in the design of work and of computer hardware to be used by people. Human factors researchers, following on the work of Sheridan and Farrell (1974) and others, have demonstrated the ineliminable elements of decision and reflection in action--a finding which directly contradicts the conventionally assumed dichotomy between mental and manual work--especially in office work. The implication is that, even for the most routine task, the user of the equipment is never a mere "operator", but must bring something to the task to resolve all of the activities presumed in performance. That "something" is the understanding of the task and the elements of skill and judgment involved in recognizing what must be done and doing it.

Accounting for the understanding of the task and the provision of the requisite expertise for accomplishing it through recruitment and training guidelines requires a further explanation of the division of labor in office work, of differences in organizational environments or local "cultures", as well as an explanation of the influence of new office technologies on organizational structures and working environments. This information is necessary to account for contextual factors involved in job design and the organization of working relationships, and in this aspect it is important to see the context for implementation as defined by the history of office roles and occupations in conjunction with the capabilities and limitations characterizing successive generations of office equipment. In this endeavor, it is significant to note that human factors issues have typically been met reactively, if at all, in all but the most progressive of organizations, and even then research is often initiated in response to conflicts and problems of productivity in working environments.

A final caveat must be issued regarding the interaction between the research and the phenomenon of office automation, especially as that research has been carried out by unions and by management in the course of implementation activities. We have noted that automation is pursued more intensely when there is conflict in the workforce, and especially when that conflict is fueled by resistance to the technology being introduced. There is evidence to suggest that where working conditions are experienced as undesirable, where work is defined narrowly and instrumentally--i.e., where machine-based--and where work-related issues cannot be expressed openly in the course of implementation, that strains will occur in the operation of systems which will undermine that implementation. Strains in working arrangements andinterdependencies can result in errors which cannot be resolved by either the old or the new methods of control; this is what is referred to in discussions of the "information crisis". Strains may also be exhibited in health problems and fears of health problems--problems which are symbolic of larger, more threatening, but "fuzzy" issues involving the re-definition of jobs.

Furthermore, if change is being implemented in organizations already experiencing internal conflicts--especially if the introduction of office automation equipment is proposed in-order-to control internal conflicts and resistance to work design and rationalization--then situations which normally could create uncertainties and conflicts in the short run may become transformed by degenerating into chronic situations which the participants define as conflicted. Under these circumstances, systems may become progressively disorganized over time s individuals withhold their commitment and withdraw their cooperation in a whole network of interdependencies--just at the moment when these task-related decisions (not covered in formalized implemntation strategies) must be made. It is at this stage that labor unrest and unionization may become factors in the further development and transformation of office systems.

Ironically, the manner in which union approaches tend to respond to managerial initiatives in the direction of re-organization and job design based upon new office equipment reflect presumptions of order quite consistent with--and thus reinforcing of--the fundamental tenets of management science and methods engineering. The first premise in the critique of new technology tends to be an uncritical acceptance of conventional designer- and manufacturer-based definitions of office automation and data-processing occupations. Where union strategies presume such conventional industrial definitions of work, they serve to further imprison alternatives, and in some cases help to accelerate rather than to retard movement in the direction of deskilling of work and the downgrading and displacement of office occupations. An example of this reflexive predicament can be seen in the strategies of 9-5, a clerical union based upon the dual premises of office work as women's work and office work as machine-based.

It could be argued that unionization is a viable alternative to professionalization, which performs the same function for its members as workers' combinations in unions; however, the objectives of combination are quite distinct. Unions exist to limit entry to scarce jobs, to facilitate training and organization of workers, and to protect workers' interests vis-a-vis their employers. In contrast,

> "The essence of the idea of a profe-sion is <u>autonomy</u>--the maintenance of the control over work tasks by doing these tasks." (Watson, 1980, p. 149)

The union strategy, on the other hand, is dependent upon and contained within the division of labor established by a particular technology and a specific organizational environment; the professional strategy, according to Watson, is to look to the traditional "free" or "status" professions of law and medicine as models for opposing external control over one's work. Thus, Watson argues, professionalization is an occupational strategy which challenges prevailing work organization and control in the interests of autonomy for its members and practitioners. (Watson, 1980, p. 149)

Ironically, occupational--or trade--association is at once not only dependent upon the established micro-division of labor based upon machine technologies, but quite apart from the sort of professional development which breaches established organizational structures and provides autonomy to its members, the strength of workers' combinations is directly proportional to their restriction to low-level jobs and adverse working conditions. Moreover, as their resistance to given technologies or organizational arrangemements increases, their very occupational power may be the impetus for further development in the machine technology which will eliminate their job categories altogether--and with them the resistance and conflict of dissatisfied workers.

This dynamic--and degenerating--relationship between continuing technological development and resistance to change is by no means new or peculiar to computerization. Charles Babbage noticed in 1849 that the combination of workers in trade unions for purposes of protecting their jobs in the face of technological change tended to hasten the development of technological innovations which would eliminate those jobs--and, by extension, their striking incumbents. A particularly ironic disadvantage often accrues to the operatives of machinery as a consequence of their attempts to combine in protecting their interests against further invention. While the public may suffer temporary increases in price for commodities as a result of strikes, Babbage argues, it is the working man who suffers in the long run when mechanization is developed "in consequence of a strike amongst the workmen". When manufacturers respond to strikes or threats of strike by improving the machinery of production, operatives very often lose their positions altogether, with much greater harm to themselves and their families than that which is done to their employers.

Babbage describes a case in the gun-making industry in which the impetus of a strike among operatives for an advance in wages caused the superintendent of the factory to turn his attention to the process of production. He succeeded in altering the tools used for making musket barrels such that

> "...a great dimunition of human labour took place in that process, and the operatives who had acquired peculiar skill in that particular branch ceased to derive any advantage from it." (Babbage, 1849, p. 68)

A similar case occurred in the same trade but a few years later, when following the end of the war the number of line operatives was reduced in keeping with the reduction in demand. Under these circumstances, combination among operatives was easy, but the effects of combination and strike on the company were great enough to encourage it to experiment with a new production process as potentially less costly than either another strike or acquiescing to workers' wage demands.

> "The operatives who had combined were, of course, no longer wanted, and as the process to which they had been habituated required peculiar skill and considerable experience, they had hitherto been in the habit of earning much higher wages than other workmen of their class. They were, therefore, instead of benefitting themselves by their combination, reduced permanently, in consequence of this improvement in the Art, to a considerably lower rate of wages."
> (Babbage, 1849, p. 68)

Thus two conflicting processes may be operating simultaneously in the development of office automation, and in this development strategies for containing resistance and conflict through progressive automation and specialization of office jobs may collide with strategies on the part of workers to improve their positions through increased education and training, and through combination in professional and trade union associations--thus driving research and development even further in the direction of order-presumptive designer-based automation of office systems in which the role of human beings is problematic at best. In this way the solution to problems of office automation becomes an element in the problem.

Conclusion

Health effects, interpersonal conflicts, errors, and excessive costs in implementation stand in a relationship of mutual causality to the phenomenon of stress in office automation. Stress is generated by situations characterized by the following conditions:

1) The execution and outcomes of tasks are uncertain;
2) Employees believe that their occupational mobility and/or job security will be reduced when the job is rationalized--i.e., when that uncertainty is removed; and
3) The communication network is restricted such that uncertaintainty is not resolved either in supervisor/employee relationships or in task-related interdependencies among employees.

Where communication is restricted to reporting relationships, the interdependencies entailed in implementation and use of computer technologies are often left unfulfilled. Supervisors may not share the expertise entailed in the task requirements, and supervisors' styles of interaction may discourage communication; it is also often the case under such restricted networks of communication in established hierarchies that interdependencies among employees at the working level cannot be resolved, because those individuals do not have an opportunity to interact in carrying out those tasks.

Under these circumstances, attempts by individuals to relieve the stress and gain control over their working conditions in resolving the uncertainties associated with implementation may create change conditions which increase the total uncertainty in the system, and which, therefore, increase the uncertainty in other employees' jobs--thus increasing the amount of stress and uncertainty overall, and increasing the possibilities for conflicts among those whose tasks are mutually dependent.

In the broader view, systematic errors and labor unrest may come about in the contexts of development and implementation in processes of implementing computer technologies in offices for three fundamental reasons: 1) The restrictions of mobility associated with professionalization; 2) errors in the process, especially those which endanger persons working in that process; and 3) explicit articulation of the structure of the organization in the course of implementation, with recognition of structural arrangements raising assumptions to an explicitly controversial level which must be addressed as an aspect of implementation.

These conditions contradict deep-seated expectations of social mobility which traditionally accompany technological change in the U.S. especially. Implementation activities transform the structure of the organization being entered in such a way as to limit the possibilities for further development. Excessive and progressive narrowness and uninterpreted formalisms in systems development limit the acquisition of information which is important to rationalizing the process, which results in strategic errors in the relationship of the organization in its external environment, as well as process errors in relating designed-production processes to the social-technical operations which instantiate that process in ongoing environments. This is the source of most of the health hazards experienced in man-machine relations.

These systematic errors point up the primacy of the process of system design over any of its products, and the need to maintain the vitality of the processes of design and problem-solving. This vitality implies structures which are not restrictive of communication flows, which are not determined by cost-considerations exclusively, and which are not so internally conflicted that implementation of design must rely on political decisions, which override in many cases technical interdependencies upon which reliable system operation depends.

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