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THE ORIGINS OF THE COMPUTER INDUSTRY: A CASE STUDY IN RADICAL TECHNOLOGICAL CHANGE.

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THE ORIGINS OF THE COMPUTER INDUSTRY

A CASE STUDY IN RADICAL TECHNOLOGICAL CHANGE

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ABSTRACT

THE ORIGINS OF THE COMPUTER INDUSTRY

A CASE STUDY IN RADICAL TECHNOLOGICAL CHANGE

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This dissertation is a case study of the computer as an example of radical technological change. It is concerned with investigating the ways in which radical technological changes like the development of the computer are different from more ordinary incremental technological change. The historical focus is on the period from 1935 to 1955, though events outside of this period are introduced as appropriate.

Chapter One introduces the study and defines exactly what computers are (in the early stages of their development, computers were only one among many kinds of advanced calculating machinery). Chapter Two presents an historical summary of the development of computing, beginning with a survey of previous computational methods and the development of desk calculators, and discussing the various computer projects that developed in the years just before, during, and after World War II.

Chapter Three argues that the technical preconditions for the development of the computer were present by the mid-1920's, and that

a substantial, though latent, demand for computers had developed by the same time. Thus the fact that computers were not developed until fifteen years later (a delay which, in the absence of World War II, would have been much longer) means that other factors must have been primary in determining the timing of computational innovation. This chapter develops a behaviorist analysis to account for this delay, and emphasizes that such a delay for non-market reasons is likely to be greater for radical innovations than for incremental innovations.

Chapter Four argues that, while the computer was a dramatically labor-saving invention, the economic literature which seeks to explain the bias of invention in a labor-saving direction is inappropriate in explaining the development of the computer.

Chapter Five argues that the diffusion of computational technology was inhibited by the radical nature of the computer, by the fact that it created a new industry which, at first, lacked many of the mechanisms for diffusion which established industries have. These inhibitions on diffusion also reduced the role of economic influences in determining the pattern of diffusion.

Chapter Six argues that the "crisis" which caused the computer to be developed was World War II, and that in the absence of the War the computer's development would have been much delayed. I also argue that there are many special characteristics of war that make it particularly suitable for the development of radical innovation, and that there were many characteristics of the post-war "Cold War" period which made it suitable for the refinement of this technology.

Chapter Seven argues that patents played a modest role in computational development, and suggests a variety of reasons why this was so. There is no reason to believe that patents play any special role in the development of radical technology.

This dissertation concludes that radical innovations are less likely to be influenced by conventional market forces, and that studies of innovation which emphasize the impact of market forces are biased in their emphasis on incremental innovation.

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Many friends have contributed, with their interest and encouragement, to the completion of this dissertation, of whom I must mention specifically two. David Jefferson was primarily responsible for stimulating my interest in computing and in the intellectual excitement of the innovations associated with it. Heidi Hartmann provided crucial support and encouragement while this dissertation was being completed. Peggy Donahue and Cathy Somers did the real work of typing the manuscript. The errors which remain are of course the responsibility of the author and not of those who have assisted him.

TABLE OF CONTENTS

Chapter

I. Introduction 1

II. Historical Summary 13

III. The Timing of Computational Innovation 82

IV. The Factor-Saving Direction of Computational Innovation 134

V. The Diffusion of Computational Innovation. 167

VI. The Impact of Federal Government Spending on Computational Innovation 203

VII. The Role of Patents in Computational Innovation. . . . 227

VIII. Conclusion 250

Appendix A: The Concept of Vicinity 258

Appendix B: Chronology of Computer Installations and Applications. 265

Bibliography 279

CHAPTER ONE

INTRODUCTION

"What laws govern the growth of man's mastery over nature?" With these words, Jacob Schmookler begins his inquiry into the factors which influence technological change.[1] This dissertation seeks to answer the same question, though it inevitably takes a narrower focus. There are many kinds of technological change, and many ways of studying it. Most studies of technological change have considered as many innovations as possible, partly to provide a large number of degrees of freedom for statistical tests, and partly to improve the generality of the results. This inevitably raises the question of how different innovations with different degrees of significance should be weighted. Obviously some innnovations are much more important than others, but no obvious measure of significance exists by which these innovations could be weighted appropriately. The result is that a series of studies have been conducted, of which some use very inclusive definitions of invention (e.g., Schmookler's use of patent statistics),[2] while others use relatively

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[1] Jacob Schmookler, Invention and Economic Growth (Cambridge: Harvard University Press, 1966), p. 1.

[2] Ibid.

exclusive definitions (e.g., lists of "significant" inventions in particular fields).[3]

The computer, I would argue, is an innovation which does not fall into either of these two categories. It is clearly more significant than the average patent; it is also more significant than the average "significant" invention in, say, the petroleum refining industry. The reason is that the computer is not just one of several technologicial developments in an industry. Rather, it is a seminal invention--one which creates its own industry, and which increasingly is revolutionizing other industries, from communications to watchmaking. It is still to early to say whether or not the computer will turn out to be an "epochal" invention--one which, like the railroad or the automobile, transforms the economy into which it is projected. Such inventions generally do not reveal their full effects for a half century or so after their initial development, and the computer has only been in existence for some thirty years. But its meteoric development so far and the pervasiveness of its impact suggest that its significance may be as great as the revolutions in transportation.

The computer, then, falls into what I would call a third class of "radical" inventions, and there is some reason to suspect that the "laws which govern" this class of inventions may be different from those which govern more ordinary, incremental inventions. Radical inventions have more far-reaching, less forseeable effects, so that one would expect that

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[3] Ibid. and Edwin Mansfield, Industrial Research and Technological Innovation (New York: Norton, 1968).

the ordinary market processes which may dominate ordinary inventions may play an attenuated role in radical innovation. Because radical innovations typically create an industry where none existed before, the institutional structure within which ordinary inventions develop is lacking.

The intent of this dissertation, then, is to examine the case of the development of the computer as a species of radical technological change in terms of some of the outstanding questions in the economics of technological change: What are the determinants of the timing of innovation? What are the determinants of its factor-saving bias? What are the determinants of its rate of diffusion to imitators? What are the impacts of government activities, such as war, and government institutions, such as the patent system, on innovation? In particular, we shall be interested in comparing the appropriateness of neoclassical answers to these questions to that of alternative analyses, and we shall be interested in considering how our analyses are affected by the radical nature of the computer.[4] The study of only a single innovation (or, rather, series of related innovations) is obviously anecdotal, so that the results of this study can only be suggestive. This is inevitable, however, given the small number of observations in the class of radical innovations. Some questions cannot be resolved on the basis of statistical significance tests.

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[4]One question which this dissertation does not address is the impact of industry structure on technological change. I do this because I consider the question, insofar as it relates to computers at least, pretty much closed. See John Jewkes, David Sawers, and Richard Stillerman, The Sources of Invention, 2nd Ed. (New York: Norton, 1969), pp. 341-345, and Gerald W. Brock, The U.S. Computer Industry: A Study of Market Power (Cambridge: Ballinger, 1975), ch. 11.

Outline of the Study

The study proceeds as follows: The remainder of this chapter considers a number of technical characteristics of computers which are important in defining the nature of the innovation with which we are concerned. It is important to understand the exact sense in which the computer was novel. These include the distinction between a computer and a calculator, the distinction between analogue and digital machines, and the differences between electronic and electro-mechanical components.

Chapter Two is an historical summary of the development of the computer up to the early 1950's. These historical facts will be referred to throughout the study, so it is important to acquire some familiarity with them at the outset. The economic analysis of these facts will be deferred to later chapters.

Chapter Three begins this economic analysis by considering possible explanations for the timing of the development of the computer. It examines neoclassical explanations for the timing of innovation and finds them inadequate in the case of the computer. It then develops a behaviorist analysis of the development of the computer which is more useful not only for explaining the timing of its development, but also for analyzing other questions considered in succeeding chapters.

Chapter Four considers another issue, which has spawned an enormous literature since Hicks first raised the question explicitly in the 1930's--the factor-saving bias of technological change. The computer is, of course, labor saving, and this chapter will consider how suitable this extensive literature is for explaining the factor-saving biases of innovation at various stages of the computer's development.

Chapter Five considers another important issue in the literature on technological change: the mechanisms of diffusion of technological change from the original innovators to later imitators. We shall offer, again, a basically behaviorist analysis of diffusion, and will suggest that the suitability of this analysis is likely to be greater for radical than for ordinary innovations.

Chapter Six considers the influence of government, particularly in its war-making capacity, as an agent of technological change. In the case of the computer, this non-economic (in the usual sense) agent was particularly important, and we shall consider the reasons why this was so.

Chapter Seven examines the role of the patent system in the development of the computer. Unlike the role of the military, the role of the patent system was quite modest. We shall argue that the internal contradictions of the patent system led innovators largely to ignore it. This was due primarily to the rapid rate of progress in computer developments--the patent system, it turns out, is useful primarily when the rate of technological progress is moderate.

Chapter Eight summarizes and synthesizes the results of the previous chapters and examines specifically the ways in which radical innovations differ systematically from ordinary innovations.

Computers versus Calculators

The word "computer" has had several meanings over the years, and it has no clearly defined meaning now. Prior to the 1940's, the word "computer" referred to a person who computed. Machines which computed were called calculators. In the 1940's, the words "computer" and "calculator" were used to a large extent interchangeably. Since 1950, two separate meanings have emerged, and the meaning of "computer" given here, while more technically precise than that in common parlance, will be useful for our purposes and corresponds to the definition to which I think most computer scientists would subscribe.[5]

There are two important characteristics of a computer for purposes of distinguishing it from a calculator. A computer is "programmable" and it is "universal." "Programmability" simply means that it is possible to enter a list of commands into the machine which provide it with a complete set of instructions (or algorithm) for solving a particular problem. Once this series of commands is fed into the machine, no further human intervention is necessary. The computer is capable of solving the entire problem by itself. This characteristic might also, therefore, be called "automaticity," since it implies that the machine, once given its initial instructions, can proceed automatically until the problem is solved.

The other defining characteristic of a computer is more subtle. While automaticity was a fairly obvious characteristic for the computer pioneers to build into their machines, universality did not so obviously recommend itself. The concept of universality stems from an

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[5]I am indebted, for the discussion which follows, to David Jefferson for extended conversations and correspondence on these points.

article by Alan Turing in 1936.[6] This article defined a set of numbers, a subset of the real numbers, called the "computable" numbers. The defining characteristic of a computable number is that it can be derived by arithmetic procedures from other real numbers on the basis of a series of intructions. That is, it can be derived by a "machine process"--it can be "computed." Turing pointed out that, for any computable number, it would be possible to build a special purpose machine for computing just that number. There would be at least one (and probably many more than one) machine for each computable number. Turing also argued that it would be possible to build a single machine which would be able to mimic all of these special purpose machines. That is, it could be programmed so as to produce any of the computable numbers. Now, instead of having at least one machine corresponding to each of the computable numbers, we have at least one program corresponding to each of the computable numbers. Each program turns the computer, for the moment, into a special purpose machine which is suitable for computing just that number. A machine which can be so programmed to produce any of the computable numbers is a universal machine, also called a universal Turing machine or a general purpose machine.

There is one problem with this definition. Strictly speaking, in order to be truly universal in this sense, a computer would have to have an infinitely expandable memory. In practice, no computer has an infinitely expandable memory. We shall therefore modify our definition

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[6] Alan Turing, "On Computable Numbers, With an Application to the Entscheidungsproblem," Proceedings of the London Mathematical Society, Second Series, vol. 42, no. 2144 (November 11, 1936), pp. 230-265.

slightly so as to ascribe universality to any machine which can compute all the computable numbers except for those which require more than its allotted memory. A "computer," then, is a machine which is both programmable and universal. A calculator is a machine for performing arithmetic operations on numbers which lacks either or both of these characteristics. This definition will become clearer later on when we encounter some of the early machines and describe why they were either calculators or computers.

Analogue versus Digital

The distinction between analogue and digital machines is also an important one, because the choice between these two styles was an important one for each designer of a calculating instrument to make. The most widely known example of an analogue calculating device is the slide rule. The corresponding digital device is the hand-held electronic calculator. A number can be represented in one of two ways. It can be represented by a string of numerals (the numerals being the marks we make on paper, 1, 2, 3, etc., which represent the numbers, the abstract mathematical idea), such as "214.362," or it can be represented by setting out a scale, establishing a zero point on the scale and the length of a unit on the scale, and then making a mark on the scale the distance of which from the zero point represents the number we have in mind. The former method represents the number in digital form; the latter method represents it in analogue form. Thus the scales on a slide rule represent numbers as distances along the scale; an electronic calculator represents numbers as a string of numerals lit up in its register.

As Jeremy Bernstein put it, "An analogue calculator measures, while a digital computer counts."[7] In an analogue machine, numbers are represented in terms of a physical phenomenon--length along a scale, voltage at an electrode (also measured along a scale), etc. The structure of the machine is such as to mimic the structure of the mathematical problem which we wish to solve. The numbers, represented as physical phenomena in various places in the machine, are related to one another within the machine in the same way that they are in the mathematical logic of the problem. The structure of the machine thus becomes a physical analogue of the mathematical structure of the problem. Numbers are translated into physical phenomena within the machine, the machine manipulates these phenomena in a way analogous to the way in which a mathematician would manipulate the numbers in his head, and the result of the machine's manipulations is that a physical phenomenon occurs at some point in the machine which, when measured, constitutes the answer to the problem.

On the other hand, as Bernstein says, a digital machine counts. All of its computation procedures are accomplished by treating numbers as a certain number of units (the unit is simply the smallest degree of accuracy of the machine--it may be one hundredth, or one thousandth, or whatever). It adds by counting the number of units found in both addends. It subtracts by counting the number of units left after the number of units in the minuend is reduced by the number of units in the

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[7] Jeremy Bernstein, The Analytical Engine: Computers--Past, Present and Future (New York: Random House, 1964), p. 24.

subtrahend, and so forth. One of the major conclusions of Boolean algebra was that all complicated computations can ultimately be resolved into these simple counting operations.

There are advantages and disadvantages to both kinds of machines. If one wishes to solve only a single kind of problem, it may be simpler to design a physical analogue to that problem than to define the problem in terms of a large number of counting operations. Moreover, if the counting mechanism is slow, and the number of counting operations is large, then the analogue machine may be much faster than the digital machine. On the other hand, an analogue machine, because it is designed to mimic the mathematical structure of a particular problem, is inevitably a special-purpose machine. There will be some problems which it cannot solve. A digital machine can potentially be made universal, in the Turing sense, so that it can solve any "computable" problem. If one has a variety of problems to solve, and if a fast counting mechanism can be invented (or if one's problems involve a small number of counting operations), then a digital machine is preferable.

A digital machine is also, again potentially, more accurate than an analogue machine. Because numbers are represented in an analogue machine as distances along a scale, the accuracy of the machine is limited by the accuracy with which the operator can set the marker along the scale (or read the marker's position along the scale, when transcribing the results of the calculation). Even in process control applications, where data are often fed into the machine and read out of it mechanically, the accuracy of the machine is limited by the accuracy with which the parts can be machined. Analogue machines are therefore accurate to no more than about one part in ten thousand, or four or five decimal places. A digital machine must inevitably round off numbers at some point, so that it can never be perfectly accurate with non-round numbers, but the degree of accuracy can be extended to any finite extent. Therefore a digital machine is potentially more accurate than an analogue machine. Of course, if the data with which one starts are less accurate than one part in ten thousand, then this consideration is not important.

Electronic versus Electro-mechanical

Analogue machines operate using a wide variety of mechanisms. Digital machines, once we get beyond the scale of desk calculators, nearly all work with a large number of components called "gates." A gate is simply an electrical component which is connected to three wires, A, B, and C. When the gate is signalled by an electrical pulse entering on A, it closes a circuit between B and C, thus setting other gates into action. It is thus a "gate" between B and C, controlled by A. Counting circuits that can manipulate numbers arithmetically can be designed as a suitable arrangement of these gates. The speed with which the machine can calculate depends upon the speed with which these gates operate (it also depends upon the speed with which data in the memory can be accessed, but we cannot survey here the much wider variety of technologies which have been used in building memory components). Miniaturized transistors are now used for such gates, but in the early 1930's and 1940's the choice was between using telephone relays and vacuum tubes. A telephone relay involves the motion of a piece of metal actuated by an electro-magnet to close the circuit. A vacuum tube closes the circuit by using a magnetic field to alter the flow of current directly. No mechanical motion of pieces of metal is necessary; hence the switching procedure can occur much more rapidly, about a thousand times as fast, even in a primitive vacuum tube machine. Machines using vacuum tubes (or transistors) are called electronic, because the only moving parts are electrons. Machines using relays are called electro-mechanical, because they use mechanical moving parts actuated by electro-magnets. Electronic machines are, as I have said, much faster, but in the 1930's they were considered too unreliable, given the large number of vacuum tubes necessary to perform a calculation. The failure of a single tube could ruin a calculation. The choice between electronic and electro-mechanical machines was another major technical choice facing the computer designer of the 1930's and 1940's.

CHAPTER TWO

HISTORICAL SUMMARY

The history of computational methods and devices is a long and fascinating one. The people involved were surprisingly colorful, given the nature of their enterprise. We can only outline that history here, giving the background necessary for the theoretical analysis to follow.

Developments in Arithmetic

Until the mid-nineteenth century, practical advances in computing were dominated by advances in mathematics and arithmetic methods, rather than by the development of devices.[1] Devices were created, but they were of little practical significance until the late nineteenth century.

Probably the earliest method of computation involved counting on one's fingers ("digital" computation, the root of the word "digital" as used in Chapter One). As the numbers involved grew larger, people ran out of fingers and toes and started using rocks or pebbles (Latin: calculi, pebbles) to assist them, counting on fingers until they ran out of fingers, and then using a stone to represent a handful of fingers.

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[1] Florence A. Yeldham, The Teaching of Arithmetic through Four Hundred Years (1535-1935) (London: Harrap & Co., 1936).

This style of computation led to the major ancient and medieval computational device, the counting board. The counting board is rather like an abacus (Greek: αβαχ, a slab), except that instead of having the stones moving on wires, they are placed on lines incised or drawn on a board. Each line represents a power of ten, and the number of stones on each line represents the number of units, tens, hundreds, etc., represented. The spaces between the lines were also used--to represent fives, fifties, etc.--thus making the board easier to read and economizing on stones (the number "eight" is easier to read as a five and three ones than as eight ones). The Roman numeral system is a natural expression of this arrangement of stones. All computation was done on the board; the numeral system was only used to note the final result. Thus the common opinion that the Roman numeral system must have been a nuisance to calculate with is misplaced, since it was not so used.[2]

The counting board was perfectly satisfactory for addition and subtraction, but it could only be used with some awkwardness for the "higher operations" of multiplication and division. Fortunately, most people had little use for such higher operations, and had no reason to learn how to perform them. Those who knew how to multiply and divide were considered skilled mathematicians.[3]

The growth of commerce in northern Italy in the twelfth and thirteenth centuries led to an increased need for computation. Extended

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[2]J. M. Pullan, The History of the Abacus (New York: Praeger, 1969), ch. 2.

[3]Louis C. Karpinski, The History of Arithmetic (New York: Rand McNally, 1925), p. 120.

partnerships involved merchants in the division of profits among a number of partners, which also led to the use of double-entry bookkeeping. The counting board became less and less satisfactory. The number system we use now, the positional base-ten system, was first developed in India by the middle centuries of the first millenium. It was picked up by the Arabs in the eighth or ninth century (hence the name "Arabic numerals": "Hindu-Arabic numerals" would be more accurate) and became known to Europeans in the tenth century (Gerbert, later Pope Sylvester II, wrote of them in 980). Early writings about this new system by Europeans were confused, however, in that they did not understand the use of the zero. The first competent text is that of Leonardo Fibonacci of Pisa in 1202.[4]

Various textbooks circulated in manuscript around Europe for the next two and a half centuries, particularly among merchants and scholars. Their use gradually spread, both occupationally, from merchants and mathematicians to court clerks, and geographically, from Italy northward. Arabic numerals were at first considered not quite good enough for formal use, so that merchants would use them in their calculations, and then express the final result in Roman numerals. Laws sometimes specified that tax receipts had to be made out using the "official" Roman numerals. Often both systems were used simultaneously, sometimes even in the same number (e.g., expressing a date MD64). Usage would switch back and forth from one style to another depending upon the conservatism

[4]David Eugene Smith and Louis C. Karpinski, <u>The Hindu-Arabic Numerals</u> (Boston: Ginn & Co., 1911).

of the clerk in charge of the notations.[5]

Their use spread more quickly after the invention of printing about 1450 allowed the printing of arithmetic primers (which usually included instruction in bookkeeping and other business procedures). In England, Arabic numerals were in sufficiently common use in the sixteenth century to be generally recognized, and by the seventeenth century Roman numerals had begun to look a little old-fashioned. However, the issue books of the Royal Exchequer accounts continued to use Roman numerals until 1826.[6]

The Europeans also improved upon the system they had picked up from the Arabs. What we now call "long division" appeared as an improvement over the cumbersome old "scratch" method in the late fifteenth century.[7] Decimal notation of fractions was invented by Simon Stevin, a Dutch mathematician, and published in a book by him in 1585.[8] Fractions had always been a nuisance, partly because people felt compelled to express all fractions as sums of unit fractions--e.g., 5/6 would be represented as 1/2 + 1/3.[9]

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[5] Pullan, op. cit., pp. 34-56.

[6] Hilary Jenkinson, "The Use of Arabic and Roman Numerals in English Archives," Antiquaries Journal, v. 6, no. 3 (July, 1926), p. 264.

[7] David Eugene Smith and Jekuthial Ginsburg, Numbers and Numerals (New York: Columbia University, 1937), p. 31.

[8] Yeldham, op. cit., p. 51.

[9] Morris Kline, Mathematical Thought from Ancient to Modern Times (New York: Oxford University Press, 1972), p. 8.

Another important advance in computational methods was Napier's invention of logarithms in 1614. A London mathematician, Henry Briggs, spent ten years actually calculating the tables of logarithms, which thereafter made multiplication and division much easier. Kepler's calculations of planetary movements, on which Newton's theories of gravitation were based, depended in turn upon the recent availability of Briggs' Tables of Logarithms.[10] The idea of logarithms was soon incorporated into an analogue calculating device, the slide rule, by William Oughtred in 1632. The slide rule was a major calculating device for more than three centuries, only recently being displaced by the hand-held calculator.[11]

The Development of Calculators

Mathematicians are notorious for hating calculating, and this disaffection early led to efforts to mechanize the procedure. While several analogue calculating devices (primarily designed for calculating astronomical data) were developed by various ancient and Moslem astronomers and astrologers, the first digital calculator that we know of was invented by Wilhelm Schickard in 1623.[12] The machine was lost in a

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[10] B. V. Bowden, Faster Than Thought: A Symposium on Digital Computing Machines (London: Pitman, 1953, p. viii.

[11] Clyde B. Clason, Delights of the Slide Rule [sic] (New York: Crowell, 1964), ch. 1.

[12] Herman H. Goldstine, The Computer from Pascal to von Neumann (Princeton: Princeton University Press, 1972), pp. 5-6. Smith claims that a digital counter was invented by Heron of Alexandria in the first century A.D., but as Drachmann's discussion of Heron's work shows, this device was only an analogue device, with no carrying mechanism, rather

fire and Schickard died of plague, so his invention does not seem to have influenced anyone. Blaise Pascal, the mathematician and philosopher, invented a somewhat simpler machine around 1643.[13] It didn't work very well, and was useful primarily for addition and subtraction, but it did become widely known among educated people (Diderot described it in his encyclopaedia) and spawned a number of imitators. Leibniz, the co-inventor of calculus, invented an improved version in 1673 which could multiply and divide by repeated addition or subtraction.[14] Over a dozen other machines were built during the seventeenth and eighteenth centuries.[15] The most important thing about all of these devices was that they were curiosities. None of them worked well, none were made in quantity, none did any significant calculating, and none had any economic significance. They were the typical product of gentlemen of leisure toying with clockworks.

In the nineteenth century, calculators began to take on some economic significance. A machine was developed in France by Charles Xavier Thomas (Thomas de Colmar) in 1820. This was the first machine to be

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like an electric watt-meter. See Thomas M. Smith, "Some Perspectives on the Early History of Computers" in Zenon W. Pylyshyn (ed.), Perspectives on the Computer Revolution (Englewood Cliffs: Prentice-Hall, 1970), p. 10, and A. G. Drachmann, The Mechanical Technology of Greek and Roman Antiquity (Copenhagen: Munksgaard, 1963), pp. 160-161.

[13] Goldstine, op. cit., p. 7.

[14] Ibid.

[15] Ernst Martin, Die Rechenmaschinen und ihre Entwicklungsgeschichte (Pappenheim: Johannes Meyer, 1925), pp. 41-57.

manufactured commercially. It was rather delicate and went out of adjustment easily, but it was a commercial success.[16] Quite a number of patents are recorded, particularly on key-driven machines (most of the previous European machines worked by turning dials).[17] Most of these key-driven machines were too delicate to do any useful work, but one was finally developed in 1884 by Dorr E. Felt (the "comptometer") which was quite satisfactory and which monopolized the nascent industry for the next twenty years. William S. Burroughs developed a somewhat superior machine in 1892 which gradually superseded Felt's machine. Both of these machines were steadily improved with printing mechanisms and other special features added, and several competent competitors entered the industry. More advanced calculators were also developed for direct multiplying (i.e., without the necessity of repeated addition, as was required by most calculators). Such calculators became widely used for business and scientific applications during the first half of the twentieth century. A number of manufacturers designed various special purpose machines for accounting, banking applications, etc.

Babbage

We must now backtrack and follow up a separate line of development which began in the early nineteenth century in Europe. Desk calculators are best suited to business applications where large quantities of data

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[16] Jerry M. Rosenberg, The Computer Prophets (London: Macmillan, 1969), p. 90.

[17] Joseph A. V. Turck, Origin of Modern Calculating Machines (Chicago: Western Society of Engineers, 1921).

must be processed, but where the processing for each datum is quite simple. In processing a payroll, for example, the number of hours a worker has worked that week are multiplied by his hourly wage, a number of deductions are made for taxes, pension payments, etc., and the result is given for the actual amount to be paid to him. There may be a large number of workers for which this processing must be done, but the processing for each worker is quite simple.

In scientific computation, on the other hand, the initial data may be quite simple, but the computation itself might be quite complex, involving hundreds of steps. We can divide any machine-aided computation into two basic steps--input/output, where the initial data are fed into the machine and the final result is reported out, and central processing, where the machine actually executes the computation called for on the data fed into it. In business applications, the input/output step involves most of the work, while the central processing is trivial. In scientific applications, the opposite is true.

One particular problem with doing scientific computations on desk calculators is that, since a computation typically involves a large number of steps, and since the desk calculator can only remember the one number which is represented in its register, intermediate results must be written down and re-entered later on, as they are needed. There is always a possibility of human error at the input/output stage, and since a machine without a memory requires repeated input/output of the same numbers, a strong possibility develops of errors creeping in midway through the calculation.

The first person to act to solve this problem was Charles Babbage.[18] Babbage was an English mathematician of the early nineteenth century, a man of inherited wealth and academic attainment (he was Lucasian Professor of Mathematics at Cambridge, the same chair that Newton had held).[19] He had a wide variety of theoretical and practical interests (such as the most efficient pricing scheme for the Post Office), and probably the most important of these was the problem of automatic calculation. Like most mathematicians, he considered it "unworthy of excellent men to lose hours like slaves in the labor of calculation which could safely be relegated to anyone else if the machines were used."[20] He was particularly concerned with automating the process of calculation so that the machine would go automatically from one step to another with neither fresh instructions from the operator nor need to re-enter the data into the machine. What is striking about Babbage is that his view of the problem to be solved in machine calculation is so similar to that which was eventually recognized by the computer builders of the 1940's. Where everyone else whom

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[18] J. H. Muller proposed a machine similar to Babbage's in 1786, but made no effort to construct it. See Goldstine, op. cit., p. 19.

[19] On Babbage see B. V. Bowden, op. cit., ch. 1; Philip and Emily Morrison (eds.), Charles Babbage and his Calculating Engines: Selected Writings by Charles Babbage and Others (New York: Dover, 1961); and Maboth Mosely, Irascible Genius: A Life of Charles Babbage, Inventor (London: Hutchinson, 1964).

[20] Gottfried Wilhelm von Leibniz, in an unpublished manuscript (1685) translated by Mark Kormes and published as "Leibniz on His Calculating Machine" in David E. Smith (ed.), A Source Book in Mathematics (New York: McGraw-Hill, 1929), p. 181.

we have considered was concerned with building calculators, Babbage was the first to conceive of building a computer (in the sense of the word as defined in Chapter One). He intuitively grasped the idea of the universal computing machine.

Babbage's first effort was not, in fact, along these lines. He first became seriously interested in machine calculation about 1822. In 1823 he received a grant from Parliament to supplement his private resources in building a machine. The interest of Parliament was aroused by the fact that one application of Babbage's machine would be to calculate Nautical Almanac data for the Royal Navy. Nautical Almanac data require extensive "scientific-style" computations, and errors inevitably crept in. The consequence of these errors was that ships were lost at sea, grounding on rocks because they had used erroneous data in calculating their positions.[21] It turns out that the formula for calculating Nautical Almanac data can be approximated by a sixth order difference equation, and the successive values of the dependent variable in the difference equation (in this case, the data to be tabulated) can be generated by simply adding up differences. Babbage's first machine, therefore, was called a Difference Engine and was designed to produce automatically any sort of tabular data (such as the Nautical Almanac data) the formula for which can be approximated by a difference equation (which, it turns out, according to a theorem due to Weierstrass, includes any continuous function).[22] The Difference Engine was therefore not a computer, because

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[21] Bowden, op. cit., p. viii.

[22] Goldstine, op. cit., p. 18.

its range of application was limited, but it was suitable for calculating virtually all tabular data, such as tables of logarithms, sines and cosines, etc. Babbage also envisioned that the results would automatically be set in stereotype to be printed, thus avoiding any human error in transcription.

Babbage was a very difficult man to work with, and he had very exacting standards. Probably for these reasons, progress on his machine was slow. He had a physical breakdown in 1827 and ceased work on the machine for good in 1833. One reason for his cessation of work was that he had conceived a more elaborate machine, a machine which would qualify under our definition as a full-fledged computer. He called this second machine the Analytical Engine.

The Analytical Engine was quite similar in concept to a modern computer. It had a memory (called the "store") large enough to store 1000 numbers of fifty decimal digits each, and a central processing unit (the "mill") that could perform all the basic arithmetic operations. Data were fed into the machine on punch cards rather like modern computer cards, and the program was also fed into the machine on cards. The idea for the cards was suggested by the Jacquard loom, a programmed loom designed to weave virtually photographically detailed patterns in cloth. The harnesses of the Jacquard loom were programmed by punched cards.

Alas, Babbage encountered the same difficulties in transforming his machine into a tangible reality as he had with the Difference Engine. He kept dreaming up new and improved designs before he had finished the earlier design, and, as a result, he never finished building anything.

From the first, his plans had been overly ambitious (none of the early computer builders in the 1940's considered building a machine with a memory as large as Babbage envisioned). He was also stymied by the difficulty of machining parts as accurately as was necessary. His machine was entirely mechanical, powered by a steam engine, with thousands of closely fitting parts. Machines of that age in Britain were still virtually hand made, with each part filed down so that it would fit with the others.[23] The labor required for building the Analytical Engine would have been enormous, and the economic demand for it was not sufficiently overwhelming to justify its construction.[24]

Babbage's designs were largely forgotten, at least in the United States, so that when Americans began working on computers nearly a century after Babbage's work, they profited little from his investigations. He continued to be widely known in the British intellectual community (Keynes, for example, was a great admirer of Babbage),[25] and proposals were occasionally made to build a machine according to his designs, but

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[23]See D. F. Galloway, "Machine Tools" in Charles Singer, et al., A History of Technology, vol. 5, The Late Nineteenth Century, c. 1850 to c. 1900 (Oxford: Clarendon Press, 1958), pp. 636-657, and Samuel Smiles, Industrial Biography: Iron Workers and Tool Makers (London: John Murray, 1908), pp. 251-255, 343-344.

[24]C.W. Merrifield, et al., Report of the Committee, consisting of Professor Cayley, Dr. Farr, Mr. J.W.L. Glaisher, Dr. Pole, Professor Fuller, Professor A.B.W. Kennedy, Professor Clifford, and Mr. C. W. Merrifield, appointed to consider the advisability and to estimate the expense of constructing Mr. Babbage's Analytical Machine, and of printing tables by its means. Drawn up by Mr. Merrifield, Report of the British Association for the Advancement of Science, Dublin, August, 1878, pp. 92-102 (London: John Murray, 1879). Reprinted in Brian Randell (ed.) Origins of Digital Computers (New York: Springer-Verlag, 1975), pp. 53-63.

[25]Morrison and Morrison, op. cit., p. xi.

no one with the requisite resources considered the computational problem sufficiently serious to be worth the effort.[26]

This is strictly true only for the Analytical Engine. Several copies of the Difference Engine were constructed. In 1834, a Swedish lawyer and publisher named Georg Scheutz read of Babbage's Difference Engine in the Edinburgh Review[27] and resolved to build his own version of Babbage's machine. He and his son completed a simple model in 1835 and a more elaborate version (with financing from the Swedish Academy) in 1853. Their success was due partly to temperament and partly to the fact that even their more elaborate version was simpler than Babbage's, calculating only to fourth differences, rather than sixth (their machine was exhibited in London and Paris, much to Babbage's pleasure, and then sold to the Dudley Observatory in Albany, N.Y.). A copy was built for the British Government by a private firm, Dunkin & Co., and was used for producing life expectancy tables in the Registrar-General's Office. Other copies were built by Wiberg (1863),[28] Grant (1871),[29] and Hamann (1910).[30] By the 1920's, multiple

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[26]F.J.W. Whipple, "Calculating Machines" in E. M. Horsburgh (ed.), Handbook of the Exhibition of Napier Relics and of Books, Instruments, and Devices for Facilitating Calculation, Napier Tercentenary Celebration (Edinburgh: Royal Society of Edinburgh, 1914), pp. 69-75.

[27]D. Lardner, "Babbage's Calculating Engines," Edinburgh Review, v. 120 (July, 1934, pp. 263-327. Reprinted in Morrison and Morrison, op. cit., pp. 163-225.

[28]R. C. Archibald, "Martin Wiberg, his Tables and his Difference Engine," Mathematical Tables and Other Aids to Computation, v. 2 (1947), pp. 371-373.

[29]George B. Grant, "On a New Difference Engine," American Journal of Science, 3rd Series, v. 4 (1874), pp. 277-284.

[30]A. Galle, Mathematische Instrumente (Leipzig: B.G. Taubner, 1912), p. 45.

register bookkeeping machines had advanced to the point that they could be operated as "difference engines." [31]

Hollerith

The next major development included both the first successful use of punched cards as an input-output medium and the first use of electrical arithmetic circuits. In the 1880's, the Census Office of the United States faced a crisis. The size of the population and the number of different kinds of tabulations had grown to the point that the traditional methods of the Census Office were incapable of keeping up with the workload.[32] It was apparent that the final tabulations of the 1880 Census would be completed barely in time to be published before the 1890 Census. The Census Office conducted a contest for an improved tabulating technology, and the winner, over two competitors, was Herman Hollerith, a mining engineer. Hollerith's idea was to transcribe the data from the raw census sheets onto punched cards. (Hollerith apparently had this idea from John S. Billings, a physician in charge of vital statistics for the Census.)[33] The punched cards would then be fed through tabulating machines, which would count the holes corresponding to facts about the census respondents. There would be one card for each

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[31] Leslie J. Comrie, "On the Application of the Brunsviga-Dupla Calculating Machine to Double Summation with Finite Differences," Monthly Notices of the Royal Astronomical Society, v. 88, no. 5 (March, 1928), pp. 447-459.

[32] Rosenberg, op. cit., pp. 110-117.

[33] Leon E. Truesdell, The Development of Punch Card Tabulation in the Bureau of the Census, 1890-1940 (Washington: G.P.O, 1965), pp. 30-34.

person counted in the census. The card had 288 locations, each of which might or might not be punched. A hole in a particular location would indicate, for example, that a person was illiterate. To find the number of illiterates living in a state, all the cards for that state were run through the tabulating machine and the machine counted how many had a hole in that particular location. T tabulating machine had a dial for each location on the card, so that a single run of the cards through the machine would give a tabulation for all the data on all the cards. The sensing of the holes was done by having the cards pass under electric wires. If there were a hole in the card, the wire would pass through the card and touch an electrical contact on the other side, sending a pulse to the dial and advancing it one unit.

Hollerith's invention, while primitive by comparison with later tabulating equipment, was a smashing success. It was used to tabulate the 1890 Census and allowed the results of the Census to be published only a couple of years after it was taken. Hollerith established a company called the Tabulating Machine Co. in 1896 with a plant in the Georgetwon section of Washington.[34] He subcontracted some of the fabricating work to Pratt and Whitney and Western Electric, and built machines which he rented to the Census Bureau for the 1900 Census. Hollerith employed no salesmen,[35] but had no difficulty in renting additional machines to other governments, railway companies, and insurance companies.

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[34] Rosenberg, op. cit., p. 119.

[35] Saul Engelbourg, "International Business Machines: A Business History," Columbia University doctoral dissertation, 1954 (Ann Arbor: University Microfilms, 1954), p. 54.

In 1904 the Census Bureau quarreled with Hollerith over rental rates, and consequently secured a $40,000 appropriation to develop its own tabulating equipment. An engineer named James Powers was hired who developed a similar machine operating on a somewhat different principle (using mechanical pins rather than electrical brushes as the sensing mechanism). These machines were used in the 1910 Census, and Powers set up his own company, the Powers Tabulating Machine Co., in 1911.

Hollerith's company was merged in 1911 with two other companies (which made butchers' scales and punch clocks) to form the Computer-Tabulating-Recording Co.[36] The promoter of this firm was a well-known wheeler-dealer named Charles R. Flint. In 1914, Flint brought in as President Thomas J. Watson, who had just been fired as Sales Manager at National Cash Register and who had just barely avoided conviction on criminal antitrust charges. In 1924, Watson renamed the company the International Business Machines Corp. The Powers Tabulating Machine Co. was bought out by Remington Rand in 1927.

As of 1910, The Powers equipment was superior to that of Hollerith. But the electrical sensing principle used by Hollerith was inherently more flexible, and Hollerith was able to recover his technical lead between 1910 and 1920. C-T-R regained the Census contract in 1920, and for the next thirty years controlled about 85% of the tabulating equipment business.[37]

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[36] William Rodgers, Think: A Biography of the Watsons and IBM (New York: Stein & Day, 1969), p. 67 ff. The "computer" in the name refers to the computing scales sold to butchers, not to anything resembling a modern computer.

[37] Engelbourg, op. cit., pp. 323-325.

Hollerith's invention was important for three reasons: First, it led to the development of an input-output medium that would be very useful in later computer developments. Second, it led to the first development of electrical circuits for doing arithmetic--at first simply counting, but later addition, subtraction, multiplication, and division. Third, it led to the creation of two business organizations with a vested interest in what we would now call the large-scale data processing market, firms which would therefore have an interest in pursuing computer developments forty years later.

Supporting Developments

Much was happening, of course, in the world at large that was not directly connected with calculating machines but which had an important impact on them. Some of these effects were already implicit in Hollerith's development of the tabulating machine.

First, the ability to do precise machine work at a reasonable cost became greatly advanced during the nineteenth century.[38] The development of interchangeable parts as the basis of manufacturing guns, sewing machines, bicycles, etc., was an important manifestation of this development. Thus, by the end of the nineteenth century, any difficulties in making precision parts which might have inhibited Babbage were no longer a problem.

Second, the improved ability to control electricity led to the creation of a number of inventions based upon electricity. The most

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[38] Galloway, op. cit.

important of these, for our purposes, were the developments in communications, namely the telephone and the radio. The development of the telephone in 1876 led to the development of automatic switching apparatus. This apparatus was based upon the telephone relay, a "gate" which we discussed above in Chapter One. This component was quickly made use of in a calculating machine, namely Hollerith's tabulator. I.B.M. later added multiplying circuits based on these telephone relays as special features on their tabulating machines (in the early 1930's). Carrying on arithmetic operations via electrical circuits is much simpler than the complicated mechanisms that Babbage envisioned.

The development of the radio led to the development of the vacuum tube as an amplifying device for weak signals. The vacuum tube, as pointed out in Chapter One, can also be used as a "gate," so that the development of the radio also made available a useful component for the use of calculating machine inventors. The possible use of the vacuum tube as a gate was recognized by World War I.[39]

Third, the natural sciences were becoming increasingly mathematicized. Difficulties in accomplishing computations were increasingly becoming a fetter on the further development of the natural sciences. During the 1920's and 1930's, therefore, several scientists and academic engineers became interested in the possibilities of automated computation.[40]

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[39] C. L. Fortescue, "Discussion" (of a paper by W. H. Eccles on three-electrode vacuum tubes), The Radio Review, v. 1, no 1 (October, 1919), p. 36, and W. H. Eccles and F. W. Jordan, "A Trigger Relay Utilizing Three-Electrode Thermionic Vacuum Tubes," ibid., v. 1, no 3 (December, 1919), pp. 143-146.

[40] Goldstine, op. cit., Part I, chs. 3, 4, and 6.

Fourth, and relatedly, the military was increasingly making use of developments in pure science. The most important case of this, for our purposes, was the development of a staff of mathematicians in the Army for carrying on ballistics research. As the military made more use of scientific manpower, that scientific manpower put more pressure on the military to provide it with the computational resources to make use of their increasingly sophisticated mathematical formulations.[41]

Analogue Calculators

One of the principal products of the mathematicization of the sciences was a computing program at MIT based upon analogue calculators and headed by Vannevar Bush.[42] Bush was heavily involved with studies of electric power systems, and many of the formulae characterizing such systems were differential equations. Many of these equations were quite complex and their solution was either impossible or extremely time-consuming. He and his associates therefore designed a series of analogue machines, chief among which was the differential analyzer. The differential analyzer could take any function, even one for which no algebraic expression was known, and either differentiate it or integrate it. The machine consisted of a number of shafts and gears laid out horizontally on a table. There were also six integrators,

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[41] Ibid., Part I, ch. 9.

[42] This work is described in a series of articles in the Journal of the Franklin Institute, chief among which is Vannevar Bush, "The Differential Analyzer: A New Machine for Solving Differential Equations," J. of the Franklin Institute, v. 212, no 4 (October, 1931), pp. 447-488. See also J. Crank, The Differential Analyzer (London: Longman, Greens & Co., 1947).

which were mechanical units with three shafts emerging from them, A, B, and C. Each shaft can be considered a variable, with the number of revolutions of the shaft equal to the value of the variable. If $A = f(B)$, then the integrator would cause C to rotate in such a way that $C = \int A dB$. Functions were entered by drawing them on graph tables and moving a pointer connected to two rods over the curve of the function so that the movement of one rod would represent the horizontal change in the function and the other would represent the vertical change. The output would similarly come out on a graph table. The machine also had special gearing so that variables could be added and multiplied.

The machine was quite useful for solving the problems for which it was designed, and a number of copies were built around the world (including one built in Britain of Meccano set parts). It did, however, have the usual shortcomings of analogue machines. First, it was only accurate to slightly better than 1%. Second, while it could solve quite a wide range of problems (even problems which did not intrinsically involve differential equations could often be worked around so that they could be solved using them, much as Babbage worked around problems so that they could be solved using difference equations), it could not solve everything. Moreover, when a new problem was to be solved on the machine, many of the shafts and gears had to be disassembled and reconnected to suit the needs of the new problem, a procedure which took a couple of days. This problem was later solved by the construction of an electrical differential analyzer which could be programmed by punched paper tape in a few minutes, but by that time digital machines had advanced to the point that the whole differential analyzer concept was nearly obsolete.

The differential analyzer is important to us because it offered a concept of how a calculating machine should be built competitive to that of the digital machines with which we shall be primarily concerned. Many scientists, particularly those associated with MIT, shied away from the idea of building a digital machine because the analogue differential analyzer seemed to work so well.

The Mark I

We are now in a position to proceed to the heart of our historical subject matter, the digital calculating devices developed in the period surrounding World War II. We have mentioned greatly increased requirements by scientists for computation. One of the branches of science for which this was particularly true was astronomy. We have also mentioned that tabulating machines were steadily improved, including having an adding circuit added to their basic counting function.

While tabulating machines were not designed for scientific work, and are not really well suited to such work, they are better suited than desk calculators. Tabulating equipment at least has the advantage that, once the data are punched onto cards, no further human data entry is required. Tabulating equipment could not do the long series of steps in a scientific calculation automatically, but at least the intermediate outputs from one step were automatically punched onto cards, which could then be fed into the machine as input for the next step. No manual punching of intermediate results was required.

These possibilities suggested themselves to astronomers in the late 1920's. The first of these was a British astronomer named Leslie

J. Comrie who used tabulators to produce lumar tables.[43] While Comrie used I.B.M. tabulators, I.B.M. had no direct role in assisting him. I.B.M.'s entry into scientific calculation began at Columbia University with an educational psychologist named Benjamin D. Wood.[44] Wood was involved with educational testing at Columbia and had a monumental test scoring problem. He inquired of various business machine manufacturers about the possibility of using their machines for test-scoring and tabulating purposes. Thomas J. Watson was the only one who responded, and he donated a number of standard I.B.M. card-processing machines to establish the Columbia University Statistical Bureau. A Michigan school teacher, Reynold B. Johnson, who had invented a test-scoring machine was later bought out by I.B.M., who used his machine at Columbia and elsewhere.

At the same time, there was an astronomer at Columbia named Wallace J. Eckert. Eckert was already aware of the work that Comrie was doing with tabulating machines in England, and, when he learned of Wood's use of I.B.M. machinery at Columbia, he set about getting similar machines donated for astronomical computing.[45] Watson was apparently flattered by the idea of his humble machines doing scientific work, so he was responsive and the result was the establishment of the Thomas J. Watson

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[43] Leslie J. Comrie, "On the Construction of Tables by Interpolation," Monthly Notices of the Royal Astronomical Society, v. 88 (April, 1928), no. 6, pp. 506-623, and L. J. Comrie, "The Application of the Hollerith Tabulating Machine to Brown's Tables of the Moon," ibid., v. 92 (May, 1932), pp. 694-707,

[44] Rodgers, op. cit., ch. 7.

[45] Ibid., p. 141.

Astronomical Computing Bureau. The Bureau originally used ordinary tabulating machines, but soon added the prototype of the multiplying tabulator and also a "difference tabulator" which was designed to do the same work as Babbage's Difference Engine, though by a different mechanism. The Astronomical Computing Bureau did work for astronomers not only at Columbia but also at other major universities, charging a fee to cover expenses, but operating as a non-profit organization.[46]

In 1937, Howard Aiken was a graduate student in physics at Harvard whose proposed dissertation involved extensive computation of Bessel functions.[47] He became interested in securing some sort of mechanical assistance in this problem and was referred by Harlow Shapley to Eckert's computing project at Columbia. Aiken opened negotiations with I.B.M. and persuaded them (i.e., Watson) to finance the construction of a much more elaborate programmed calculator, though one which would still use conventional, proven I.B.M. components.

Aiken had formulated his ideas for the machine by December, 1937, began working with I.B.M. engineers in 1939, and had the machine completed in 1944. It was built in I.B.M.'s workshops in Endicott, N.Y., and then disassembled and moved to Cambridge, where it was formally turned over to Harvard on August 7, 1944. Its official name was the IBM Automatic Sequence Controlled Calculator, but it was most widely

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[46]Wallace J. Eckert, Punched Card Methods in Scientific Computation (New York: Thomas J. Watson Astronomical Computing Bureau, Columbia University, 1940).

[47]Rodgers, op. cit., pp. 168ff.

known as the Mark I.[48] It was an enormous machine, 51 feet long, 8 feet high, and 3 feet deep. It was programmed by paper tape and produced output primarily on electric typewriters. It was used for producing tables, and the typewriter output was photo-offset to avoid error in transcription. It had a memory capacity of 72 numbers of 23 decimal digits each, and could multiply two 23-digit numbers in six seconds.

About the time the Mark I was nearing completion, Charles C. Bramble, who was supervising the Navy's program of computing firing tables, talked with Aiken about the possibility of using the Mark I for this use. A contract was let, so that the first use of the Mark I was for computing firing tables (about which we shall have more to say below in connection with the ENIAC), although this application did not motivate its construction. The Navy thereafter contracted for two more machines, the Mark II (officially known as the ARC--Aiken Relay Calculator) in 1948 and the Mark III (ADEC--Aiken Dahlgren Electronic Calculator) in 1951. These were built solely by Harvard, without any participation by I.B.M. Harvard also built a fourth machine, the Mark IV (Harvard Magnetic Drum Calculator), in 1952 for the Air Force.[49]

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[48] Howard H. Aiken and the Harvard University Computation Laboratory Staff, A Manual of Operation for the Automatic Sequence Controlled Calculator, Annals of the Computation Laboratory of Harvard University, v. 1 (Cambridge: Harvard University Press, 1946).

[49] Francis W. Dresch, "Brief History of the Dahlgren Calculators" (Dahlgren, Va.: typewritten, c. 1952), and Charles C. Bramble, interview by the author at his summer home in Franklin, N.H., September 7-8, 1974.

The Bell Labs Calculators

George Stibitz was a mathematician working at the Bell Labs before the War who also became interested in computing. He started out by taking some telephone relays home with him and putting them together in circuits on his kitchen table.[50] He eventually interested the company in supporting the project to build a machine, and he built their first machine, a complex number calculator, in 1940. This calculator was designed to solve certain problems in electrical network analysis that were of interest to Bell Labs, and was demonstrated at the meetings of the American Mathematical Society that year in Hanover, N.H. Actually, the machine was in New York City. It was operated over a data link from an electric typewriter in Hanover, surely the first example of computing via remote terminal. The machine was special purpose, used electro-mechanical relays as components, and therefore operated at about the same speed as the Mark I. Stibitz became active during the War in the Office for Scientific Research and Development, and supervised for them the construction of a series of other machines at Bell Labs--a Relay Interpolator, a Ballistics Computer, and the Mark IV. the first general purpose machine built by Bell Labs. Bell Labs built two more machines after the War, but generally shied away from getting involved in computers any more than was necessary for their own research. They were concerned about the possible antitrust consequences of appearing

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[50] Rosenberg, _op. cit._, p. 150.

to enter the business machines industry.[51]

Zuse

The Zuse computers had little if any impact on the development of computers in America, since they were unknown to most of those working on computers here, but fairness requires that they be mentioned, if only to avoid the impression that the computer was purely an Anglo-American invention.

Konrad Zuse was a 24-year-old German aircraft engineer in 1934 when he first became interested in computers.[52] Like most computer enthusiasts, he had become frustrated at the difficulty of doing the computations required for his work. After thinking about the design of a machine for a couple of years, he quit his job in 1936 and began working on a machine in his parents' apartment. This machine, the Z1, was finished in 1938. It was a purely mechanical machine and it didn't work very well because the parts didn't fit properly. He abandoned it and constructed the Z2 out of telephone relays. He was then drafted and spent a year in the service before he persuaded one of the German aircraft research organizations to support his computational work. He finished the Z3, which appears to be quite similar to the Mark I, in 1941. These were all electro-mechanical machines. He had proposed building

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[51]George Stibitz and E. G. Andrews, interviewed by Henry S. Tropp, March 2, 1972, Smithsonian Institution, The National Museum of History and Technology, Computer History Project (Washington: Smithsonian Institution, 1972), pp. 34-35. Interviews in this series will hereafter be referred to as Smithsonian (CHP).

[52]See Randell, op. cit., ch. 4, and William H. Desmonde and Klaus J. Berkling, "The Zuse Z3," Datamation, v. 12, no. 9 (September, 1966), pp. 30-31.

an electronic machine in 1937, and in fact built, with H. Scheyer, an adding circuit with 100 electronic tubes in 1942, but the German Government turned down his proposal to build the larger machine. After Scheyer was drafted, Zuse started his own company in 1943 with about fifteen employees and built a couple of special purpose machines for aeronautical testing and also another general purpose machine called the Z4. Most of these machines were destroyed in air raids. He continued actively in the computer business after the War, but his machines quickly became obsolete compared with those in America and Great Britain.

Cryptoanalysis

During World War II, two projects in cryptoanalysis--code-breaking--were organized, one in Britain and one in the United States. The very existence of these two projects has only recently been revealed, and the details of the machines built for them remain largely classified. They both had a significant impact upon later computational developments.

The most successful project, apparently, took place in England. The British Admiralty had had some success at breaking German codes during World War I and had continued keeping track of German codes between the wars.[53] During the 1930's the Germans developed an enciphering machine called Enigma, of which British Intelligence managed to steal a copy in 1938 or 1939 (with the help of the Polish Secret Service).

Any code can be broken if it is used enough times. Regularities eventually become apparent, allowing the decipherment of the code. The

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[53] Frederick W. Winterbotham, The Ultra Secret (London: Weidenfeld and Nicholson, 1974), ch. 2.

secret to maintaining security is constantly changing the code. Enigma was designed to make this easier. The message was typed onto a keyboard. The machine was composed of a number of wheels with letters on their periphery. The wheels were connected to one another electrically, and the pattern of connection could be adjusted in millions of different ways. Each setting corresponded to a different code.

Enigma was used to encipher messages sent by radio, hence subject to monitoring by the other side. As long as the Enigma settings were changed frequently according to some previously communicated secret plan, the code would be virtually unbreakable (or, rather, it might be breakable, but only after such a long time as no longer to be useful). The goal of the cryptoanalytic project (which was housed in Bletchley Park, a country house in Buckinghamshire) apparently was to design a machine which could examine a great number of possible permutations of settings of the Enigma, so that they could arrive at one which made sense in a reasonable amount of time. The first machine, which was ready early in 1940 and which was described as "a bronze-colored column surmounted by a larger circular bronze-colored face, like some Eastern Goddess who was destined to become the oracle of Bletchley...,"[54] was designed by C. E. Wynn-Williams and Alan Turing and was called the "Heath Robinson."[55] It was mainly electro-mechanical but had electronic

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[54] Ibid., p. 15

[55] Donald Michie, "The Bletchley Machines," in Randell, op. cit., pp. 327-328.

counters (which Wynn-Williams had described in 1931).[56] It simultaneously read two paper tapes photoelectrically at a speed of 2000 characters per second. The coded message was apparently punched on one tape and a "program" was punched on to the other which ordered various possible translations of the data tape, accepting or rejecting them according to some criteria built into the machine.

A series of improved machines succeeded this first one, including the "Peter Robinson," "Robinson and Cleaver," and "Super Robinson," the latter designed by T. H. Flowers, a telephone engineer, in 1944, with four simultaneously read tapes.[57]

A second series of machines, the Colossus series, was fully electronic. The were designed by Maxwell H. A. Newman, Flowers, A. W. M. Coombs, S. W. Broadbent, W. Chandler, Irving J. Good, and Donald Mitchie. Some Americans were also brought over from time to time to work on the project. The Colossus machines had only a single tape fed in with the coded message. The "program" was stored in an electronic memory, and all switching components were electronic, including about 2000 tubes. The Mark I Colossus, which read its paper tape input at 5000 characters per second, was ready by December, 1943. The Mark 2, which read 25,000 characters per second, was ready in May, 1944. By the end of the war, ten had been built and several more had been ordered.

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[56] C. E. Wynn-Williams, "The Use of Thyratrons for High Speed Automatic Counting of Physical Phenomena," Proceedings of the Royal Society of London, Series A, v. 132 (1931), pp. 295-310.

[57] Mitchie, op. cit.

These machines were extraordinarily successful at code-breaking and played a very important role in winning the War.[58] Their impact on later computer developments is more problematical. Some Americans seem to have been exposed to them, but those who were aren't talking about it, so it is difficult to assess what impact they had on American developments directly. Their designers, however, were leaders in the post-war British computer effort, so they clearly influenced the later British machines, and these had some influence on American designers. The intelligence produced by these machines was given the highest possible classification in Britain, "Ultra Secret," and the machines collectively came to be know as the "Ultra Machine."[59]

The British sent a copy of the Ultra machine (it is not clear which model) to the Americans. It was set up in the Pentagon and was used for breaking Japanese codes, since the Germans had sold Enigma machines to the Japanese. The Americans also had their own code-breaking project about which even less is known than the British project. The Americans built a machine called the Project 13 in 1943-44 which has been described as a "first-class vacuum tube computer."[60] It is not clear whether the American machine was more or less successful than the British

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[58] Winterbotham, op. cit.

[59] For more on Ultra, such as there is, see Irving J. Good, "Some Future Social Repercussions of Computers," International Journal of Environmental Studies, v. 1 (1970), pp. 67-79, and Brian Randell, "On Alan Turing and the Origins of Digital Computers," Machine Intelligence, v. 7 (1972), pp. 3-20.

[60] Isaac Auerbach, interviewed by Henry S. Tropp, February 17, 1972, at Philadelphia, Smithsonian (CHP), p. 13.

machine. Nothing is publicly known about it except that it was electronic.

The ENIAC

Of all the wartime computational projects, the ENIAC (Electronic Numerical Integrator and Computer) turned out to be the most important. Before considering the ENIAC project itself, however, we must go over a little background material.

The importance of the ENIAC lies primarily in its use of electronic components in a large-scale calculator for the first time. The idea of using electronic components apparently occurred to a number of people in the 1930's. We have already mentioned the Eccles-Jordan article of 1919 describing an electronic "flip-flop," i.e., a memory component, and the use of electronic components by Zuse in Germany and by the cryptoanalytic groups in Britain and the U.S. A number of articles appeared during the 'thirties describing electronic components used in counting circuits, beginning with Wynn-Williams in 1931.[61] These were designed primarily to count the pulses generated by a Geiger "counter" (which, notwithstanding the name, only generates pulses; it doesn't count them) for measuring radiation. E. William Phillips, an actuary, published a paper on "Binary Calculation" in 1936,[62] and claimed in 1964 that his article would have discussed the use of electronic components for the proposed calculating machine had it not been for obscure

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[61] Op. cit.

[62] E. W. Phillips, "Binary Calculation," Journal of the Institute of Actuaries, v. 67 (1936), pp. 187-221. Reprinted in Randell, op. cit. (1975), pp. 293-304.

patent difficulties.[63]

A few American firms, namely N.C.R., I.B.M., and R.C.A., apparently investigated the use of electronic components for computational purposes in the late 'thirties and early 'forties, but, while a few patent applications were filed, none of the results of their work seems to have been published until after the War.[64] One anonymous I.B.M. publication even claims that J.W. Bryce investigated the use of electronic components for computation in 1915![65] Norbert Wiener apparently proposed an electronic digital binary calculator in 1940, but was refused financial support by the Government.[66] None of this unpublished work seems to have had much impact on computer developments, however (aside from the cryptoanalytic work). One project which was also unpublished, but which had a very important impact, was the work done by John V. Atanasoff at Iowa State College (now University).

Atanasoff taught mathematics and physics at Iowa State College. He became interested in solving differential and integral equations and

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[63] E.W. Phillips, Presentation of Institute Gold Medals to Mr. Wilfred Perks and Mr. William Phillips, Reply by Mr. Phillips, November 23, 1964, Journal of the Institute of Actuaries, v. 91, Pt. 1, no. 388 (1965), pp. 19-21.

[64] Byron E. Phelps, "The Beginnings of Electronic Computation," Report TR 00.2259 (Poughkeepsie: Systems Development Division, I.B.M. Corp., December 9, 1971), pp. 2-5. On the N.C.R. work see ibid. and Randell, op. cit. (1975), p. 291n; National Cash Register Corp., Annual Report (1952), p. 12, and (1953), pp. 24-25; and Jerry Mendelson, interviewed by Henry S. Tropp, January 3, 1972, Smithsonian (CHP), p. 72.

[65] I.B.M. Corp., "New Methods for Knowing," Form 500-0002 (New York: I.B.M. Corp., January, 1960).

[66] Norbert Wiener, Cybernetics: Or Control and Communications In the Animal and the Machine (New York: Wiley, 1948), pp. 9-11.

studied Vannevar Bush's work solving such equations at MIT with differential analyzers. Atanasoff apparently decided that such equipment was unsatisfactory for his purposes and decided to try to build a calculator using electronic components. His work is, so far as I have been able to tell, the first definite effort to use electronic components for computing purposes. He began his machine in 1937 and had it working in 1940.[67]

Atanasoff's machine was a special purpose calculator, and was not terribly important in terms of any calculating it did itself, but it did have an important influence on others. In December, 1940, John Mauchly, who was teaching physics at Ursinus College in Philadelphia and was interested in solving computational problems in connection with weather forecasting, met Atanasoff at the meetings of the American Association for the Advancement of Science in Philadelphia. Atanasoff told him of his machine, and. after the Spring term ended in 1941, Mauchly went out to Iowa and spent several days at Atanasoff's house learning about the machine.[68] Soon after this visit, Mauchly took a position at the Moore School of Electrical Engineering at the University of Pennsylvania. This was a fortuitous move, because about the same time the Moore School took over part of the computational work of the Army Ballistics Research Laboratory.

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[67]Goldstine, op. cit., pp. 123-126.

[68]"Findings of Fact, Conclusions of Law and Order for Judgment," Honeywell Inc. vs. Sperry Rand Corp., et al., 4-67 Civ. 138, District of Minnesota (October, 1973), p. 48. Reprinted in U.S. Senate, Subcommittee on Antitrust and Monopoly, The Industrial Reorganization Act (S. 1167), Hearings, Part 7, The Computer Industry (Washington: G.P.O., 1974), p. 5819.

Ballistics computations played an important role in computational developments, so a little background on their nature might be appropriate.[69] It is, of course, possible to fire an artillery shell entirely by eye--such was the approach used during most of the nineteenth century. One first fires a ranging shot, and then has a forward observer report back how much the distance and the horizontal aim (azimuth) must be corrected. Eventually, at least if one is firing at a stationary target, the gunner will find the mark. This is, however, a waste of shells, and if one artillery site is firing at another, it may be a matter of survival who "finds the range" first. Toward this end, mathematicians in the late nineteenth century sought to develop mathematical expressions which related the distance of the target, weight of the shell, muzzle velocity of the gun, wind velocity, etc., to one another to allow the correct elevation of the barrel and azimuth to be calculated. There was a constant tension between making the equations accurate and making them sufficiently simple that they could be conveniently calculated. By World War I, firing tables based on these equations were in use, though their relatively unsophisticated nature caused some interesting firing errors to be made (for example, the "Big Bertha" cannon used by the Germans fired twice as far as originally predicted because the ballistics equations in use ignored the thinning of the air, hence reduced drag, at the high altitudes through which the shell passed).

After World War I, these equations were made progressively more sophisticated. (There seems, at least in the Navy, to have been some

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[69]The following is based upon material in Goldstine, <u>op</u>. <u>cit</u>., Pt. 1, ch. 9, and Pt. 2, ch. 2.

tension between line officers, who were inclined to stick with the old ranging technique and ignore firing tables altogether, and the mathematicians brought in to the ballistics labs, who generally sought to make the tables more sophisticated.)[70] The more sophisticated the equations, the more computational work was required to produce each firing table. A firing table was required for each new kind of shell, cannon, bomb, depth charge, or torpedo brought into use. During the inter-war years, the introduction of new ordnance was at a sufficiently moderate pace that the computational problems could be mostly solved on desk calculators. But in the years immediately before and during World War II, the number of new types of ordnance increased dramatically, so that the old methods of computing the tables became inadequate.

The Army had established a ballistics research group during World War I at Aberdeen Proving Ground which was formally constituted as the Ballistics Research Lab in 1938. It included a number of first-rate mathematicians. In 1935 it made its first attempt to automate its computations by having built a copy of Bush's differential analyzer, which was suited for the sort of calculations required to produce a firing table. Between 1941 and 1944, they also acquired several I.B.M. tabulating machines and multipliers, similar to those in use at Columbia.

Nevertheless, the Ballastics Labs were unable to keep up with the quantity of work expected of them. This was the situation facing them: The calculation of a single trajectory for a firing table required approximately 750 multiplications. A complete firing table required the calculation of some 3000 trajectories. So a firing table required

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[70]Bramble interview, op. cit.

perhaps 2,250,000 multiplications. There are various other supplementary calculations, so that the total calculating time is perhaps three times the time required for the multiplications alone, assuming that the sequence of calculations can be done automatically (i.e., no time added for reentry of intermediate results). For a human operating a desk calculator, this time must be approximately doubled to allow for this reentry of intermediate results. If a human can do a multiplication on a desk calculator in, say, ten seconds, then the total time to do a trajectory is 12 hours, and the total time to do a firing table is 36,000 hours. If the operator works a forty-hour week, we will require 900 man-weeks to calculate a firing table. The Ballistics Research Labs had a peak staff, during the War, of some 200 people, so their maximum output of firing tables, using desk calculators, would be one every 4½ weeks.[71]

The differential analyzer speeded this up considerably. It could do all the calculations for a trajectory in about fifteen minutes. Therefore it could do a firing table in about 750 hours. It required five or ten people to man a differential analyzer, so that, in theory, one could imagine the staff working on about 30 differential analyzers, thus producing a firing table every 25 hours.

There were two problems with this solution. First, a differential analyzer of any reasonable degree of accuracy is a precision instrument, requiring at least a year to manufacture, so it did not offer any immediate solution. Getting thirty of them built, in the absence of any

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[71]Goldstine, op. cit., p. 135.

established production line, might have taken several years.[72] Second, even if the 30 differential analyzers had been available immediately, they still would have been insufficient to keep up with the demands on the BRL. By 1944, the BRL was receiving requests for six firing tables per day,[73] about 18 times what the assemblage of differential analyzers could handle. Some more powerful calculating instrument was required. (Increasing the size of the staff, incidentally, was not really a solution either. The reader can easily calculate the size of staff required to handle the work-load using existing equipment. The staff of 200 mentioned above had already required "recruiting trips to colleges all over the Northeast."[74] While college graduates were not strictly speaking necessary for punching desk calculators, they greatly reduced the amount of training necessary and were much better at correcting their own mistakes, since they had some understanding of the nature of the calculations they were performing.)[75]

This then explains the demand for increased computing power. The circumstances which led this demand to induce the production of a machine were as follows: In 1941, the BRL contracted with the Moore School of Electrical Engineering in Philadelphia to take over the operation of its differential analyzer.[76] In 1942, Herman Goldstine, who had received

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[72]Herman H. Goldstine, interviewed by the author at the Institute for Advanced Study, July 23, 1975.

[73]Goldstine, op. cit. (1972), p. 166.

[74]Ibid., pp. 134-135.

[75]Goldstine interview, op. cit. (1975).

[76]Goldstine, op. cit. (1972), p. 130.

his Ph.D. in mathematics at Chicago in 1936 and had been an Assistant Professor of Mathematics at Michigan, was drafted and assigned to the Ballistics Research Labs to assist in the calculations (he had taught a course in exterior ballistics). In September, 1943, Goldstine was put in charge of the work being done at the Moore School.

We will remember that in 1941 John Mauchly, fresh from examining Atanasoff's electronic calculator at Iowa State, took a job at the Moore School. He had also seen Stibitz' machine at Hanover in 1940. During the following year, he thought of Atanasoff's machine and in August, 1942, wrote a paper proposing an electronic calculator incorporating improvements on Atanasoff's machine.[77] This paper was circulated around the department and was read by J. Presper Eckert (no relation to Wallace Eckert at Columbia), who was a graduate student in the department at the time. Eckert was intrigued and read up on the modest literature on digital electronics (e.g., Eccles-Jordan, Wynn-Williams, etc.). During the Fall of 1942, Mauchly pressed his ideas on the newly arrived Goldstine, who was receptive to them. In the Spring of 1943, Goldstine discussed the possibility of constructing an electronic calculator with John Grist Brainerd, who was a senior professor at the Moore School and their liaison with the BRL and the Ordnance Department. Brainerd concurred in the idea and in April, 1943, sent a proposal to Ordnance to provide funds for the construction of a machine (an act which involved some risk for him and for the school, since, as we shall see, the conventional wisdom in respectable engineering circles was

[77] John W. Mauchly, "The Use of High Speed Vacuum Tube Devices for Calculating," reprinted in Randell, op. cit. (1975), pp. 329-332.

that electronic machines couldn't work). The proposal was informally approved that month (the key people in securing approval were Paul N. Gillon, Assistant Director of the BRL, and Oswald Veblen, a distinguished mathematician--and nephew of Thorstein Veblen--who was chief scientist at the BRL), work began at the end of May, and a contract was let in June. The initial estimate of the cost was $150,000, but it eventually turned out to cost $486,804.22.[78] The money came from discretionary funds of the Ordnance Department and had nothing to do with the "official" source of funding for military R&D projects, the National Defense Research Committee. This was a good thing, as the NDRC was dominated by Vannevar Bush and Samuel H. Caldwell from MIT, who believed in analogue computation, and George Stibitz of Bell Labs, who believed in using electro-mechanical rather than electronic components. (Perhaps this is why Wiener's proposal was turned down; it was made directly to Bush.) The NDRC people wrote letters periodically disapproving of the ENIAC project, but were in no position to interfere since it was not their money.

The ENIAC was a monstrous machine, consisting of 40 panels arranged in a U 80 feet around, 8½ feet high, and consuming 120,000 watts at 240 volts.[79] A 24 horsepower ventilating system was necessary to keep it from overheating. It is said that all the lights at the Moore School

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[78] Goldstine, op. cit. (1972), pp. 150, 154.

[79] Lewis B. Tabor, "Brief Description and Operating Characteristics of the ENIAC," Proceedings of a Symposium on Large-Scale Digital Calculating Machinery, January 7-10, 1947, Annals of the Computation Laboraof Harvard University, v. 16 (Cambridge: Harvard University Press, 1948). The technical details vary somewhat from one report to another.

dimmed when it was turned on. The machine operated electronically and calculated, for its time, very quickly--an addition or subtraction in .0002 seconds, a multiplication in .003 seconds, and a division or square root in .02 seconds. The machine had very restricted memory. It operated in the decimal system (for some reason, Mauchly rejected Atanasoff's recommendation for binary operation) and had electronic storage for only 20 ten-digit numbers. It also had three "function tables" where numbers could be stored in advance by manually setting switches. These would correspond to what is described today as a "read-only" memory, and had a capacity for 312 twelve-digit numbers, but had limited usefulness because no intermediate results could be stored there by the machine.

The machine was programmed by plugging wires into plugboards in an appropriate arrangement. Data were fed by punch cards, but the program was not fed into the machine on an external medium as on the Mark I. In a sense, then, the machine was "hard-wired"--the program was embodied in the physical wiring of the machine, so that the machine had to be rebuilt, in a sense, whenever a new problem was set up on it. This naturally slowed down programming--usually at least a day was required for rewiring it for a new problem. This was not too much of a problem as long as the machine was dedicated to a single application like calculating firing tables, but it did make it less suitable for general purpose use.

The ENIAC was innovative mostly because it was the first machine to make large-scale use of electronic components (Atanasoff's machine had used about 500 tubes;[80] the Colossus machines about 2000;[81] ENIAC used 18,000).[82] Electronic components had been avoided by previous computer builders for various reasons, but primarily because they were thought to be too unreliable. The conventional scientific wisdom of the day was that the "mean free path" problem was insuperable. Enrico Fermi was particularly concerned with this.[83] In order for the computer to operate, all of its tubes must be operating simultaneously. If P is the probablity that a tube will fail within X minutes, then the probability that at least one failure will occur among N tubes within X minutes is $1 - (1-P)^N$. If N is 18,000, as in the ENIAC, then P must be exceedingly small to keep the joint probability of failure within reasonable bounds. The average time to the first tube failure is called the "mean free path" between failures, and the conventional wisdom was that the mean free path would be too short to get any computing done. This problem turned out to be greatly exaggerated. Eckert reduced the voltage on the tubes below their rated capacities and found that this greatly

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80 John V. Atanasoff, "Computing Machine for the Solution of Large Systems of Linear Algebraic Equations," unpublished memorandum (Ames, Iowa: Iowa State College, August, 1940), reprinted in Randell, op. cit. (1975), p. 320.

81 supra, p. 42.

82 Tabor, op cit., p. 31

83 Interview with Eckert and Mauchly in John Costello, "The Little Known Creators of the Computer," Nation's Business, v. 59, no. 12 (December, 1971), p. 60.

extended tube life. In the event, the mean free path turned out to be about 20 hours, greatly exceeding expectations. In any case, the machine calculated so quickly that even if the mean free path had been one or two orders of magnitude less than what it was, the machine still would have been faster than an electro-mechanical machine. Eckert notes that "The card machines associated with the ENIAC were by far the greatest source of unreliability. These difficulties were caused by frequent jamming of the card machines, by several breakdowns of the Geneva movements of the punching units, and by trouble with some of the bearings and other parts of the IBM units."[84] The tube failure rate was only one per 376,000 tube hours, and at one point the machine operated 100 hours without a failure. (The breakdown rate increased afterwards when the ENIAC was shipped to Aberdeen,[85] but this was because the soldered joints had been shaken up in transit.[86] Immediately after the move, the mean free path was down to about 25 seconds, but, as the weakened joints were repaired, the machine returned to its former reliability.

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[84] J. Presper Eckert, "Reliability of Parts," Lecture 20 in University of Pennsylvania, Moore School of Electrical Engineering, Theory and Techniques for Design of Electronic Digital Computers: Lectures Given at the Moore School, 8 July 1946-31 August 1946 (Philadelphia: University of Pennsylvania, July 23, 1946), p. 20/5. Lectures in this series will hereafter be cited as Theory and Techniques.

[85] Stibitz and Andrews, op. cit., pp. 9-10.

[86] Richard F. Clippinger, interviewed by Richard R. Mertz, at Cambridge, Mass., December 17, 1970, Smithsonian (CHP), pp. 21-25.

As it happened, the ENIAC was not finished until after the War was over. The first problems were not run until December, 1945, nearly a year after Goldstine's original estimate.[87] The machine was not accepted by the Army until June, 1946.[88]

Von Neumann and Stored Programming

When we discussed the concept of universality in Chapter One, we avoided any discussion of how a machine might actually be designed so as to achieve universality. In Turing's article,[89] he had in mind a machine with a paper tape feeding program and data into the machine. The data would be stored in some sort of memory, and the program tape would move back and forth, guiding the computation. The "and forth" turns out to be an important characteristic of the machine. This is because it happens that a necessary condition for universality is that the machine be able to accomplish a procedure called "conditional branching." Conditional branching is a procedure equivalent to the "if" statement in Fortran. It tells the machine "if A, then B," where A is a fact (e.g., the sign of the result of the previous calculation being positive) and B is a command (e.g., to set a variable equal to a certain value, or to proceed with a certain subroutine in a program). On the basis of the result of one stage in the computation, the machine will follow either one or another branch in its program. Conditional branching is necessary

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[87] Goldstine, op. cit. (1972), p. 226.

[88] Ibid., p. 234.

[89] Alan Turing, "On Computable Numbers, With an Application to the Entscheidungsproblem," Proceedings of the London Mathematical Society, Second Series, vol. 42, no. 2144 (November 11, 1936), pp. 230-265.

for universality because many of the computational routines are necessarily iterative. They compute increasingly precise approximations to the desired number. The machine can only decide when to stop if it knows when it has achieved the desired degree of precision. Conditional branching allows the machine to compare the last iteration with the penultimate iteration and decide if the difference between the two is significantly different from zero. On the condition that the answer is no, the machine branches to a command which terminates the computation; on the condition that the answer is yes, the machine branches to a repetition of the subroutine used to calculate another iteration.

One way of achieving conditional branching is through the technique that Turing had in mind: having the program on a tape and having the machine move back and forth on the tape. If the machine wants to run a subroutine over again, then it simply backs up the tape to the beginning of the desired subroutine. In can back up and rerun the subroutine as many times as necessary. Babbage's Analytical Engine also apparently was intended to use this method.[90]

In any electronic machine, however, this is an inadequate procedure, because the tape is inevitably a mechanical contrivance. Moving the tape back and forth takes much more time than the electronic operations of the machine's central processor. If the electronic portions of the machine had to wait for these mechanical operations to be accomplished every time they had to run a subroutine over again, they would spend most of their time idle, and the advantages of the electronic components

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[90]Randell, op. cit. (1975), p. 8.

would be largely lost. Some other method for achieving universality was necessary.

Until 1944, these problems had never occurred to anyone (unless they arose with the Ultra machine). The first person who posed these problems, to our knowledge, was John von Neumann. Von Newmann was one of the intellectual prodigies of the twentieth century.[91] He was born in Budapest in 1903, the son of a banker. His career followed a relatively conventional path until he graduated from the Swiss Federal Institute of Technology in 1925 with a degree in chemical engineering, having distinguished himself in mathematics. His career then accelerated. He received his doctoral degree the following year in mathematics at the University of Budapest, taught in Berlin and Hamburg for four years, and then came to Princeton as a visiting professor in 1930 and became a permanent professor in 1931. In 1933, he joined the new Institute for Advanced Study in Princeton, with which he was associated off and on until he died in 1957 of bone cancer.

In 1936, Alan Turing came to Princeton for two years, and von Neumann became aware of his work on computable numbers. While von Neumann's early work was primarily in theoretical mathematics, he was increasingly, in his thirties, becoming inter ed in applications. One particular application in which he was interested was in analyzing fluid dynamics. Fluid dynamics are described by non-linear partial differential equations, which are extremely difficult to solve by direct analysis.

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[91]For biographical material, see Goldstine, op. cit., (1972), pp. 167-183.

In connection with his work on fluid dynamics, von Neumann became interested in analyzing shock waves and blast waves and consequently became a consultant for the Ballistics Research Lab in 1937. He also became a consultant to the Los Alamos Laboratory in 1943, working on the problem of achieving criticality in nuclear explosives.

As a result of his consulting work with the BRL, von Neumann came to be standing on the train platform at Aberdeen, Md., one day in the summer of 1944. Herman Goldstine was on the same platform, and introduced himself to von Neumann, whom he had leard lecture and greatly admired, but never met. Von Neumann became greatly interested when informed that Goldstine was working on a calculating device that could do over 300 multiplications per second, and later that summer came up to Philadelphia to look at the ENIAC, partially constructed. Von Neumann signed on as a consultant to the ENIAC project.

His impact on the project was apparently quite immediate. He first visited the ENIAC project early in August, 1944, and Goldstine's letters indicate that the idea of stored programming, which Goldstine attributes to von Neumann, had been hatched by the end of August.[92] The idea of stored programming is that the program is fed into the machine on a paper tape or punched cards, or whatever, but it is stored in the electronic memory of the machine so that it can be accessed at high speeds. Given the iterative nature of many computer programs, it is much faster to "consult" a repetitively used subroutine electronically that to have to run back over a mechnical program tape over and over again.

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[92] Goldstine, op. cit. (1972), pp. 198-200.

None of the machines built or contemplated in 1944 incorporated this idea (Colossus may be an exception). Both the Bell Labs machines and the Harvard Mark I and II were programmed by paper tape, but the paper tape appears to have been capable only of going forward, not back (at least automatically--it could be manually cranked back by the operator, but not by the program control), so that any repetitive iterations had to be programmed repeatedly onto the tape. The Harvard machine was later supplemented by a subroutine reader, which was used to read a continuous loop of tape.[93] The machine could be commanded to do as many iterations of the subroutine as were necessary for the solution of the problem. This approach, of course, limited the number of subroutines on which iterations could be run in the same problem (the subroutine reader could only read two loops), and was developed, in any case, after von Neumann conceived of the idea for the ENIAC project.

The ENIAC, as we have seen, was programmed by plugboards. It was therefore not really programmable in the usual sense of the word. Thus the idea of stored programming was a major advance. After 1950, virtually all of the large computing machines built were stored program machines. The great advantage of stored programming is that it allows for universality, and the great advantage of universality is that it economizes on hardware and it dramatically expands the range of problems which can be solved by computer. It would be wasteful of hardware to build a machine for each new application. Even if hardware were not scarce, the effort of designing a new machine for each new application

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[93] Richard M. Bloch, "Mark I Calculator," Proceedings of a Symposium on Large Scale Digital Calculating Machinery, January 7-10, 1947 (Cambridge: Harvard University Press, 1948), p. 29.

would greatly inhibit the range of applications explored. The great significance of the computer has been in the ever expanding range of applications: the fact that the stored program concept guaranteed that any computable application was possible with existing machinery (aside from memory limitations, which are steadily being relaxed) greatly liberated users and allowed them to exercise their imaginations freely.

Stored programming also makes possible high-level programming languages, without which modern computers would be virtually unrecognizable. The expanding range of applications for computers has been predicated on the fact that it has been unnecessary to learn machine language in order to program the machine. High level languages economize greatly on programming time, even if the programmer already knows machine language.

Goldstine, as I have said, attributes the idea to von Neumann and, given the timing with which the idea emerged, this seems the most reasonable conclusion. Nevertheless, Eckert has claimed since the late 'forties that he and Mauchly dreamed up the idea.[94] Eckert's claim seems to be partly based on a desire to assert patent rights over the idea, which von Neumann never claimed. It is difficult to assign credit in this sort of case where discussions among the five or so principals were continuous, and in which ideas were picked up from one person, developed by another and then modified by a third.[95]

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[94]Goldstine, op. cit. (1972), pp. 223-224.

[95]See statement by von Neumann, ibid., p. 198.

Von Neumann's other major contribution was simply a general analysis of the problem of designing a computing machine from a logical point of view. His ideas were summarized in a memorandum he wrote in the Spring of 1945.[96] This is the first analysis of a computer which describes it in the terms which have become familiar since. Von Neumann noted the need for a central processor, a memory, stored programming, and binary operation.

What is striking about von Neumann is that he appears to be the first to view the problem, in designing the computer, as one of designing a machine which will mimic the activities of the brain. This is apparent in the first draft mentioned above, and is also clear in his later work.[97] Other designers of calculators all seem to have had particular problems to solve, and to have been concerned with designing a machine which would efficiently solve the problems that they had in mind. They viewed their machines as calculators. Von Neumann was the first to view the problem as it is viewed today, of designing a machine

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[96] John von Neumann, "First Draft of a Report on the EDVAC," Contract No. W-670-ORD-4926 (Philadelphia: Moore School of Electrical Engineering, University of Pennsylvania, 30 June 1945), reprinted in Randell, op. cit. (1975), pp. 355-364. EDVAC (Electronic Discrete Variable Computer) was the successor to ENIAC and the machine in which the stored program concept was expected to be embodied.

[97] Ibid., section 4.2 (pp. 360-361 in Randell), and John von Neumann, The Computer and the Brain (New Haven: Yale University Press, 1958). Von Neumann's approach to the problem was to some extent influenced by Norbert Wiener, with whom he formed, in December, 1944, a group called the Teleological Society to discuss common patterns of communications and control in machines and animals. Von Neumann was also strongly influenced by a paper by W. S. McCulloch and W. H. Pitts ("A Logical Calculus of the Ideas Immanent in Nervous Activity," Bulletin of Mathematical Biophysics, v. 5 (1943), pp. 115-133). See Goldstine, op. cit. (1972), p. 275, and Wiener, op. cit., p. 23 and ch. 5.

which will mimic the action of the human brain--the original universal computing machine.

This is paradoxical, in a way, because ever since calculating machines have been made, the general public has been quick to dub them "mechanical brains." The public, with its high regard for the abilities of scientists, is quick to assume that mental functions can be accomplished artificially. The scientists, on the other hand, who actually construct such marvels, are generally much more cognizant of the problems in simulating mental processes and have generally been much more pessimistic about the possibilities of doing so. They have generally buried themselves in their own little problems, and assumed that anything grander was impossible. The achievement of von Neumann was to transcend the credulity of the public and the skepticism of the scientist.

Post-War Developments

By this point, the major breakthroughs in computer design, which I would list as Eckert's success in making electronic components feasible and von Neumann's concept of universality via stored programming, had occurred and further developments, while not inevitable, were largely set into motion. Computer developments proliferated after the war, and no one project was critical to the development of others. One important consequence of the wartime developments was that various government agencies were sufficiently impressed with the usefulness of computers that securing funds was much easier. A fair amount of hustling was still necessary, but funding was no longer the insuperable bar to progress that it was before the war.

The Moore School.--To begin with, by late 1944 the ENIAC project was well enough along that the design work was mostly finished and most of the remaining work was straightforward construction.[98] Consequently the designers turned their attention to the possibility of building a second, improved machine. It was at this point that von Neumann entered the scene, and the design concepts he developed were directed not toward ENIAC, but toward this second machine, which was to be called EDVAC (Electronic Discrete Variable Computer). Army Ordnance contracted with the Moore School for the construction of this machine. At first, the people working on it included the same cast of characters as had worked on ENIAC, but after the War, the principal ones drifted off to their own pursuits. One group, von Neumann, Goldstine, and Arthur Burks, left for the Institute for Advanced Study to build a computer there. Another group, Eckert and Mauchly and several others, started their own company, known at first as the Electronic Control Company and later as the Eckert-Mauchly Computer Corporation. The remaining staff at the Moore School did finish the EDVAC in 1950 and shipped it off to the Ballistics Research Labs at Aberdeen.[99] The Moore School did build a third machine for their own use called MSAC, but after 1950 they ceased being a major computing center.[100]

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[98]Goldstine, op. cit. (1972), p. 198.

[99]Ibid., p. 240.

[100]U.S. Office of Naval Research, A Survey of Automatic Digital Computers, by N. M. Blachman (Washington: Office of Naval Research, 1943), p. 68.

Institute for Advance Study.--Von Neumann and Goldstine were interested in continuing their work with computers, but the interest of the Moore School was waning, partly due to the disruptive effects of patent disputes over the ENIAC. Von Neumann wished to return to the Institute for Advanced Study and managed to persuade its director, Frank Aydelotte, to support the construction of a computer there, despite the general disinterest of the IAS in non-theoretical work. The decision by the IAS was partly influenced by von Neumann's emphasis on the usefulness of the computer for investigating topics in pure mathematics which were inaccessible by the conventional analytical means. A complex funding consortium was set up to finance the machine (which was to cost $300,000). The IAS itself contributed $100,000, RCA contributed $100,000 worth of work on electronic tubes, and the rest came from the Office of Naval Research, Army Ordnance (which was hedging its bets, contributing to both the EDVAC and IAS projects), the Office of Air Research, and Princeton University (whose contribution was in the form of faculty time).[101]

Von Neumann was somewhat like Babbage in that he found it difficult to be sufficiently satisfied with a machine to be willing to finish it. For several years he assured those who inquired about the machine that its completion was 18 months away. This time period can to be known as the "von Neumann constant."[102] It was originally expected to be finished

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[101] Goldstine, op. cit. (1972), pp. 239-246.

[102] Robert D. Elbourne, interviewed by Richard R. Mertz at the National Bureau of Standards, Gaithersburg, Md., March 23, 1971, Smithsonian (CHP), p. 22.

in 1948,[103] but was not ready until the beginning of 1952. Von Neumann worked on the machine until 1955, when he left to become an Atomic Energy Commissioner in Washington. Goldstine continued to run the machine until von Neumann died in 1957, when he left to take a job at I.B.M.[104] Burks had left to teach philosophy at Michigan soon after joining the project.

The IAS machine was similar to the EDVAC in its most fundamental respects, but it did pioneer the use of parallel (as opposed to serial) processing, and it made use of a new electronic memory tube developed by a British engineer named Frederic C. Williams. The Williams tube was soon made obsolete by core memories. Probably the most significant impact of the work at IAS was in working out the detailed problems of logical design and programming. A steady stream of memos and reports issued from the IAS which were read by computer researchers at industrial, government, and university research labs.[105] As a result, a number of copies of the IAS machine were built: ORDVAC (ORdnance Discrete Variable Automatic Computer) for Army Ordance, AVIDAC (Argonne's Version of the IAS Digital Automatic Computer) for the Argonne National Laboratory in Illinois, ILLIAC (ILLInois Automatic Computer) for the University of Illinois, ORACLE (Oak Ridge Automatic Computer and Logical Engine) for the Oak Ridge National Laboratory, MANIAC (Mathematical Analyzer,

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[103] J. H. Curtiss, "A Review of Government Requirements and Activities in the Field of Automatic Digital Computing Machinery," Lecture 29 in Theory and Techniques (August 1, 1946), p. 29/24.

[104] Godlstine, op. cit. (1972), p. 319.

[105] Ibid., pp. 262-270.

Numerical Integrator and Automatic Computer) for the Los Alamos Scientific Laboratory, JOHNNIAC (JOHn von Neumann Numerical Integrator And Computer) for the Rand Corporation in Santa Monica, and the TC-1 (Telemeter Computer, Model 1) for the International Telemeter Corporation in Los Angeles. These were all completed between 1952 and 1955, many of them coming on-line only shortly after the IAS machine because of its delay in construction. Other similar machines were built in Sweden, Germany, the Soviet Union, Israel, and Australia (SILLIAC).[106] Many of the other machines, while not direct copies of the IAS machine, were strongly influenced by it (e.g., the IBM 701).[107] Needless to say, the acronym game started by ENIAC got rather carried away. At one point Northrop built a machine called QUAC--QUadratic Arc Computer. They had also considered calling it DIAPER--Digital Integrator and Parabolic Error Resolver.)[108] Most of these machines were financed by various armed services and the Atomic Energy Commission, as well as by other Government agencies.

Eckert-Mauchly Computer Company.--About the same time that von Neumann, Goldstine, and Burks left the Moore School to join the IAS, in 1946, Eckert and Mauchly left with several others to start their own company, the Electronic Control Co. (later the Eckert-Mauchly Computer Corp.). Eckert and Mauchly had apparently been interested in the profit-

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[106]U.S. Office of Naval Research, op. cit., passim.

[107]Stanley Frankel, interviewed by Robina Mapstone at his home in Hollywood, Calif., October 5, 1972, Smithsonian (CHP), p. 17.

[108]Jerry Mendelson, interviewed by Henry S. Tropp, January 3, 1972, Smithsonian (CHP), p. 60.

making possibilities of computers almost from the beginning.[109] Toward the end of the ENIAC project, Mauchly was spending nearly all of his time preparing patent applications. He apparently never revealed to Goldstine and the others his debt to Atanasoff.[110] While Eckert and Mauchly clearly did not forsee the enormous growth in the computer business, they did feel that there would be a market for at least a dozen or so machines, and if they could make a profit of $100,000 or so on each one, the could gather a tidy fortune, somewhat in the manner that Harold Pender, Dean of the Moore School, had done with his International Resistor Company.[111]

One of their first contracts was with Northrop Aircraft to build an airborne guidance computer for the Snark missile.[112] This computer was called BINAC (BINary Automatic Computer), and was finished in 1950. They also got a contract from the Census Bureau (with the National Bureau of Standards acting as contracting agent) for the UNIVAC (UNIVersal Automatic Computer), which was completed in 1951. They also picked up contracts with the American Totalizator Company,[113] Prudential Insurance, and the Army Signal Corps (the Prudential contract was due to the

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[109] Goldstine interview, op. cit. (1975).

[110] Ibid.

[111] Ibid.

[112] Frankel interview, op. cit., p. 27.

[113] J. Presper Eckert, "In the Beginning and to What End" in World Computer Pioneer Converence, Computers and Their Future (Llandudno, Wales: Richard Williams and Partners, Computer Specialists, July, 1970), p. 3/22.

interest of Edmund C. Berkeley, a computer enthusiast since he had seen Stibitz' first machine at Hanover in 1940).[114] Theirs was a hand-to-mouth existence. As the UNIVAC was nearing completion, they were running out of money and approached I.B.M. to see if I.B.M. would buy them out. Watson was not interested.[115] Instead they sold out to Remington Rand in 1950.

I.B.M.--After finishing the Mark I in 1944, I.B.M. maintained a low-profile research effort in computers. They produced a semi-electronic machine in 1948 called the SSEC (Selective Sequence Electronic Calculator).[116] This was not a stored program machine (as were the EDVAC, BINAC, and UNIVAC)--it had its program on paper tape, and also used subroutine readers with continuous loops of tape, as on the augmented Mark I. The machine was installed behind large windows on the ground floor of I.B.M.'s New York headquarters, where the passers-by could be properly impressed. Whenever it broke down, the curtains were closed so that the public could not see it with its panels down.[117]

I.B.M. also gradually added electronic components to its old tabulating machine line. It offered an electronic calculator in 1946 (the Model 603--this machine could add, subtract, and multiply, but not divide; a new model, the 604, replaced the 603 in 1948) and sold

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[114] Auerback interview, op. cit., p. 38.

[115] Rodgers, op. cit., pp. 198-199.

[116] International Business Machines Corp., "IBM Selective Sequence Electronic Calculator" (New York: IBM Corp., 1948).

[117] Robert Patrick, interviewed by Robina Mapstone at Northridge, Calif., February 26, 1973, Smithsonian (CHP), p. 11.

some 5700 such calculators in twelve years.[118] This was the first commercially available electronic calculator. It certainly could not be called a computer, but it was a dramatic improvement over what had been available commercially before. This was followed in 1949 by the CPC (Card-Programmed Electronic Calculator), which had been invented by a group of Northrop researchers using I.B.M. equipment who eventually persuaded I.B.M. to produce it commercially.[119] The CPC allowed longer sequences of calculations to be programmed by punched cards. About 150 of these had been sold by 1953.[120] In 1954, they added the Model 650 Magnetic Drum Calculator. This was much slower than the IAS or Eckert-Mauchly machines, because it stored its program on a magnetic drum rather than in an electronic memory, but it was a true computer at a relatively modest price and sold like hotcakes. I.B.M. expected to sell 20 and would up selling over 2000.[121] In 1953, I.B.M. began deliveries on their first "real" computer, the 701, of which 19 were sold before it was superseded by the 704. It was in connection with this machine that Watson is reputed to have estimated the total market for computers at 15.[122] Within four years, I.B.M. had overtaken

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[118] Phelps, op. cit., pp. 6-7, 11.

[119] Mendelson interview, op. cit., p. 2.

[120] U.S. Office of Naval Research, op. cit., p. 50.

[121] Forman Action, interviewed by Richard R. Mertz, at Princeton, N.J., January 21, 1971, Smithsonian (CHP), p. 39.

[122] Mendelson interview, op. cit., pp. 70-71. For a contrary view, see Dan Mason, interviewed by Robina Mapstone, February 22, 1973, Smithsonian (CHP), p. 3 and p. 10.

Remington Rand's initial lead and secured the 70% market share which it continues to hold today.

Engineering Research Associates.--The group of cryptoanalysts in the Navy referred to above (p. 33) decided after the war to remain involved in working with computer components. This group, led by William C. Norris and including C. B. Tompkins, J. H. Howard, and L. R. Steinhardt, started up a company called Engineering Research Associates.[123] ERA made only computer components at first, especially drum memories, but eventually began making full-scale stored program computers, beginning with the 1101 (= 13 in binary code, cf. p. 33, supra) in 1950 and continuing with the 1102 and 1103 in 1953.[124] ERA was bought out by Remington Rand in 1952, but the ERA people never felt that Remington Rand (later Sperry Rand, after 1955) was marketing their ideas properly. In 1957 several of them left and formed Control Data Corp.[125]

Northrop and its descendents.--On the West Coast, computing developments originated with the work by the Northrop Corp. on the Snark missile project. The Snark was to be an internally guided missile, with an on-board computer. Northrop took two approaches to developing this guidance computer. One was to contract with the newly founded Eckert-Mauchly Corp. for the BINAC; the other was to develop their own computer design shop.

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[123] Engineering Research Associates, "Technical Advisory Committee Minutes, 9 December 1946" (Minneapolis: Engineering Research Associates, December 9, 1946).

[124] U.S. Office of Naval Research, op. cit., pp. 33-35.

[125] T. A. Wise, "Control Data's Magnificent Fumble," Fortune, v. 73, no. 4 (April, 1966), pp. 116, 260.

Their shop was under the guidance of Floyd E. Steele and included Donald Eckdahl, Harold Sarkissian, and Richard E. Sprague.[126] They designed a digital differential analyzer (DIDA), which was conceived by Steele in late 1946 or early 1947 and built under contract by Hewlett-Packard. Before it was finished, Steele got an idea for a better machine using a magnetic drum memory (MADDIDA: MAgnetic Drum DIgital Differential Analyzer) which was finished in 1949. Steele was a singular character, emphasizing in all his designs hardware economy via the precepts of Boolean algebra as applied to computing by Claude Shannon (viz., that only the simplest logical operations need be built into the hardware of the machines--everything else can be accomplished by programming).[127] Many of the machines later developed by West Coast computer companies bear the mark of his influence. Steele and his group became enthusiastic about the commercial prospects for computers, and tried to persuade Jack Northrop to let them develop a commercial version of the MADDIDA. Northrop was experiencing financial difficulties at the time (having just lost money in a number of non-aerospace ventures such as pre-fabricated housing, and having just lost the flying wing contract to Boeing),[128] so he decided against any uncertain ventures in non-aerospace markets. Steele and some of his associates thereupon resigned and started their own firm in 1950, the Computer Research Corp. They

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[126] Mendelson interview, op. cit., p. 5.

[127] Claude E. Shannon, "A Symbolic Analysis of Relay and Switching Circuits," Transactions of the American Institute of Electrical Engineers, v. 57 (1938), pp. 713-723.

[128] Harold H. Sarkissian, interviewed by Robina Mapstone, at Costa Mesa, Calif., December 21, 1972. Smithsonian (CHP), p. 33.

built a copy of the old DIDA for North American Aviation, who wanted to use it for the Navaho missile, a competitor with the Snark.[129] (North American pirated the technology and later built "thousands" of guidance computers based upon the DIDA.[130] North American also built a general purpose computer called the RECOMP. Les Kilpatrick, who ran North American's computer operation, later became President of California Computer Products Co.) CRC also built a couple of other models for a variety of users before running out of money and being bought out by National Cash Register in 1953.

Glen E. Hagen, who remained at Northrop when Steele and his colleagues started CRC, persuaded Mr. Northrop in 1950 to finance a commercial computer program after all.[131] Northrop sold 16 copies of the MADDIDA, and then sold their computer operation to Bendix in late 1951. Hagen left (or was fired by) Bendix three months later and then started a computer company financed by Axel Wenner-Gren, a Swedish industrialist, called the Logistics Research Corp. They built a computer called the ALWAC (Axel Wenner-Gren Automatic Computer) and the company was renamed the Alwac Corp. after Hagen left in 1955. Earlier, Robert Beck, Henry Herold, Vincent Niesius, and Richard Russell had left Logistics Research to start a computer program at J. B. Rea, where they built a machine called the Readix.

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[129] Mendelson interview, op. cit., p. 22.

[130] Donald Eckdahl, interviewed by Henry S. Tropp, September 25, 1972, Dayton, Ohio. Smithsonian (CHP), p. 22.

[131] Glenn E. Hagen, interviewed by Robina Mapstone at New Orleans, November 7, 1973. Smithsonian (CHP), pp. 8-9.

After Hagen left Bendix, Max Palevsky and David Evans joined it and produced a few machines. They later left for Packard-Bell, and then left to start Scientific Data Systems, which they sold to Xerox in 1970 for several hundred million dollars. Palevsky became Xerox's largest stockholder, but Xerox failed to make a success of the business, and sold it in 1975 to Honeywell.

Consolidated Electrodynamics.--Another firm which became interested in computing on the West Coast was Consolidated Electrodynamics. This was a large engineering firm (formerly Consolidated Engineering) which was doing some work in the late 'forties requiring the manipulation of data in matrix form from a mass spectrometer.[132] They hired as a consultant Ernst Selmer, who had spent time at MIT learning about Whirlwind, at the IAS learning about their machine, and at Berkeley learning about the machine being built there by the Electrical Engineering Dept., the CALDIC. They already had on their staff Clifford Berry, wo had assisted Atanasoff in the construction of his machine.[133] Consolidated Electrodynamics was renamed Electrodata and then bought by Burroughs in 1956.[134]

Raytheon.--On the East Coast, another group of people, including Charles West, Richard Bloch, and Louis Fein, built a computer for Raytheon on a Navy contract called the Raydac.[135] Bloch had formerly

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[132] Paul King, interviewed by Robina Mapstone, February 27, 1973, Smithsonian (CHP), p. 68.

[133] Randell, op. cit. (1975), p. 288.

[134] Burroughs Corporation, Annual Report, 1956 (Detroit: Burroughs Corp., 1956).

worked with Aiken at Harvard on the Mark I. Part of this group, led by Louis Fein, started their own firm in 1952 called the Computer Control Co. In 1955, Raytheon formed a jointly owned firm with Honeywell called the Datamatic Corp. (60% owned by Honeywell) which started producing a machine called the Datamatic 1000. Honeywell acquired the remaining 40% in 1957[136]

Other firms.--This completes the list of the most important firms involved in building computers, important largely because of the influence they had on larger firms which later became principal firms in the industry. A number of other firms were also building computers at the time, including Librascope, International Telemeter Corp. (a division of Paramount Pictures which was involved in pay television and which started a computer division using RCA people), Hughes Aircraft (primarily guidance computers), Jacobs Instrument Co., Underwood, Monrobot, Hogan Labs, Inc., and Marchant Research Inc.[137] The demand for computing was expanding so fast that waiting times for delivery were long, so firms often simply built machines for their own use (since the technology was still relatively simple and all the programming and maintenance would have to be done in-house anyway) or contracted with other firms who had little experience in building computers but who might be able to promise faster delivery.

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[136] Honeywell, Inc., Annual Reports, 1955, 1957 (Minneapolis: Honeywell, Inc., 1955, 1957).

[137] U.S. Office of Naval Research, op. cit., passim.

University Computers.--At the same time, a number of computers were being built in universities, sometimes simply as projects for the electrical engineering department, sometimes in response to well-defined requirements for greater computational power by users. Many of these machines were supported by government contracts, and a number of them have already been mentioned as copies of the IAS machine. The Harvard and Moore School programs have already been mentioned.

One of the most important of these was the Whirlwind, which was built at the MIT Servo-Mechanisms Lab (later renamed the Digital Computer Lab). MIT had had a strong orientation toward analogue machines dating from Vannevar Bush's work. The Servo-Mechanisms Lab was involved in building aircraft simulators, and it occurred to Jay Forrester, the director of the Lab, that a computer might be used to make the simulator respond to the actions of the pilot trainee using it.[138] At first they attempted to build an analogue computer to do the job, but by 1946 they had decided that a digital approach would be more successful. The result was a machine called the Whirlwind that was designed to be the state of the art in terms of speed. It was supposed to operate in "real time"; that is, it was to be constantly interacting with the aircraft simulator, calculating the appropriate response of the simulator to the pilot's actions in time for it to appear to be the natural response of a real aircraft.[139] It was finished in 1950 and had a significant

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[138] C. Robert Wieser, interviewed by Richard R. Mertz, at McLean, Va., March 20, 1970. Smithsonian (CHP), pp. 7-11.

[139] Jay W. Forrester, "The Digital Computation Program at M.I.T." Proceedings of a Second Symposium on Large Scale Digital Calculating Machinery, Annals of the Computation Laboratory of Harvard University, v. 26, September 13-16, 1949 (Cambridge: Harvard University Press, 1951), pp. 44-49.

influence on work done elsewhere. It also pioneered a number of innovations, notably core memories, without which the later machines of the late 'fifties and 'sixties would have been impractical.

Other university machines included the IAS machine at Princeton, its copy, ILLIAC, at the University of Illinois, the MINAC (MINimal Automatic Computer) at Cal Tech, the CALDIC at U. C. Berkeley, WISC (Wisconsin Integrally Synchronized Computer) at the University of Wisconsin, and a copy of the MADDIDA built at the Stevens Institute of Technology. As the commercial computer manufacturers started producing computers in substantial numbers, universities began buying from them instead of building their own.

Government Computers.--Nearly all of the computers built during this period had at least partial government funding, mostly military, but only a few were built by the Government itself. Most were built by private industry or in universities for the use of the Government. This was partly for political reasons--private firms lobbied against having the Government forestall their markets by building machines itself.[140] When the Government did build a machine itself, it was usually for the same reason that private firms often did--they could not secure delivery from a private builder soon enough to meet their needs.[141] Some of these Government built machines we have already mentioned as copies of the IAS machine (MANIAC, AVIDAC, AND ORACLE). The National Bureau of Standards, which played the role of contracting agent for a number of

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[140]Derrick H. Lehmer, interviewed by Uta Merzbach at Berkeley, Calif., October 8, 1969. Smithsonian (CHP), pp. 8-9.

[141]Elbourne interview, op. cit.

other Government agencies, also built three machines of its own: SEAC (Standards Eastern Automatic Computer), SWAC (Standards Western Automatic Computer), and DYSEAC (Second Standards Eastern Automatic Computer). Similar to SEAC and DYSEAC were FLAC (FLorida Automatic Computer), built by the Air Force Missile Test Center, Patrick AFB, Cocoa, Fla., and MIDAC (MIchigan Digital Automatic Computer), built at the Willow Run Research Center in Ypsilanti, Michigan, for the Wright Air Development Center. Others included the USAF-Fairchild Computer, built for the Nuclear Energy for Propulsion of Aircraft Project at Oak Ridge, and NAREC (NAval Research Lab Electronic Computer) at the Naval Research Lab in Washington.[142] The Government, like the universities, began buying from commercial suppliers as soon as private computer-building capacity expanded in the late 'fifties.

Computers Abroad.--Other countries were also active in building computers at this time, though their efforts rarely led to the establishment (at least for some time) of commercial firms building several copies of the same computer. The leader abroad was Britain. Goldstine says, "It is remarkable that Great Britain had such vitality that it could immediately after the war embark on so many well-conceived and well-executed projects in the computer field."[143] Goldstine was apparently unaware of their prior experience with Ultra. Several British scientists came over to look at American developments, starting with John R. Womersley in February, 1945. Douglas Hartree and L. J. Comrie also

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[142] U.S. Office of Naval Research, op. cit., passim.

[143] Goldstine, op. cit. (1972), p. 321.

visited. The British were in fact the first to get a stored program machine operating, the EDSAC (Electronic Delay Storage Automatic Computer) in June, 1949, beating Eckert-Mauchly and others by a few months.

A copy of the EDSAC was soon built for J. Lyons & Co., Ltd., the large food processor.[144] Their Deputy Controller, T. R. Thompson, was an enthusiast for automated accounting procedures, having systematized their accounting before the War in a way which would have made it suitable for computerization, had any computers been available. He was unsuccessful at persuading the regular business equipment manufacturers to build a machine, so he contracted with Cambridge University to build a copy of the EDSAC for him, which was called LEO (Lyons Electronic Office). "This computer, LEO I, started live operations on a weekly cost accounting job for the Cadby Hall factories in October, 1951, a job which was done without fail by LEO I every Friday for 12½ years."[145] Another machine had already been built at the National Physical Lab at Teddington by Alan Turing called ACE (Automatic Computing Engine).[146] A machine was built at Manchester University called MADM (Manchester Automatic Digital Machine), of which several copies were made for other universities, industry, and the British military. Another computer was built at Harwell for the Atomic Energy Research Establishment. Two were built at Birkbeck College of the University of London, one financed by the British Rayon Mfrs. Assoc. called APERC (All-Purpose Electronic Rayon

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[144] T. R. Thompson, "Re-Assessment" in World Computer Pioneers Conference, _op_. _cit_., pp. 8/1-8/27.

[145] _Ibid_., p. 8/1.

[146] U.S. Office of Naval Research, _op_. _cit_., p. 2.

Computer), and one financed by a group of foundations and business firms called APEXC (All-Purpose Electronic X-Ray Computer). The Post Office built a machine called MOSAIC (Ministry of Supply Arithmetical Integrator and Calculator). Ellicott Bros., Ltd., built a machine called NICHOLAS (NICkel Delay Line Storage Computer) for itself and a copy for the National Research Development Corp. The Telecommunications Research Establishment also built a machine for itself at Malvern.

Elsewhere, computers were completed or underway by 1953 in Canada, Australia, Norway, Germany (Konrad Zuse had continued building a series of computers after the War, mostly electro-mechanical; four other machines were built at technical institutes), Switzerland, France (two machines), Holland (two machines), Belgium, Sweden (two machines), and Japan (three machines).[147]

Most of these machines had little impact on American developments, being in several cases copies of American machines. The British machines, however, were relatively original, and influenced several Americans, such as Stanley Frankel, Montgomery Phister, and Jerry Mendelson, who noted:

> I think the British were much more advanced than the Americans in the way they approached the machine design in the early days. I was looking for a machine at the time but there was nothing available. One of the machines I looked at was the original Ferranti Manchester Mark I, and that machine was very much better thought out and considered from the programming viewpoint...They were brilliant, but you see, they just never got around to doing it. That's the story of the British life.[148]

Harry Huskey, an Englishman who came to the states after working on ACE,

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[147] *Ibid.*, *passim*.

[148] Mendelson interview, *op. cit.*, pp. 53-54.

influenced the design of the Bendix and other West Coast machines.

This summary history leaves out many details and omits mention of many people who made significant contributions to the technology. We have avoided any significant discussion of post-war technical developments, mostly because these technical developments were incremental improvements for the most part rather than major breakthroughs. Three exceptions deserve mention. Core memories, the invention of which is most commonly attributed to Jay Forrester at MIT but which seem also to have occurred to a number of other people about the same time, solved the most serious problem of the computer builders of the day--how to produce a reasonable amount of memory (5-10,000 words) at reasonable cost and reliability. High-level programming languages, which reduced the amount of highly skilled labor necessary to program the machines (and which depended upon the development of core memories--without the core memories, it would have been too expensive to store the compilers for the programming languages in the memory), were developed in the late 'fifties, primarily by I.B.M. (FORTRAN) and Honeywell (FACT). Transistors, invented in the late 'forties, underwent a ten-year period of development during the 'fifties and began to be used widely on computers about 1960, greatly improving reliability and reducing costs. For the most part, however, improvements in computing after 1945 followed the pattern traced by Gilfillan for the development of the ship--[149] a series of incremental advances which, jointly, produce a larger

[149] S. Colum Gilfillan, Inventing the Ship (Chicago: Follett, 1935).

reduction in cost than the initial breakthroughs themselves (though, of course, the incremental advances would not have been possible, by definition, without the "breakthroughs").

CHAPTER THREE

THE TIMING OF COMPUTATIONAL INNOVATION

The current literature on innovation emphasizes two approaches to explaining the timing of innovation. One approach emphasizes technological determinants or, in economic terms, supply constraints.[1] The other emphasizes "economic" determinants or, in economic terms, demand constraints.[2] We shall consider both of these approaches as they apply to the case of the computer and argue that both, even in combination, offer at best a partial explanation of the timing of computational innovation. We shall argue that, at least for "radical" innovations like the computer, "behaviorist" factors play a crucial role in determining the timing of innovation.

Supply Constraints

I shall argue that the necessary technology for building a computer was available by 1925-30, at the latest, and that therefore the fifteen year gap between this date and the beginning of the first electronic

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[1] I include here variations of the heroic theory of invention, that innovation must await the coming of the brilliant inventor.

[2] Economic determinants would of course include supply constraints as well as demand constraints, but the word "economic" is often used in the literature as if it applied only to demand constraints. See, for example, Jacob Schmookler, Invention and Economic Growth (Cambridge: Harvard University Press, 1966), p. 206.

computer projects cannot be explained on the basis of supply constraints.[3]

Roughly speaking, the technology used in building a computer can be devided into two parts, the "architecture" and the components.[4] By "architecture" I mean the overall logical design of the machine, a design which can be embodied in a variety of different kinds of components. The components are the specific mechanisms used to give form to this design. In discussing the architecture of a calculating machine, one is concerned with questions like: What operations will the machine perform? Will the machine have a memory? Will the program of the machine be embodied in its hardware, or will it be written on some external medium to be operated upon by the machine, or will it be read by the machine from such a medium and stored in memory? What number base will the machine use? What range of problems will it be capable of solving? Will it be analogue or digital? Will it be capable of

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[3]A fifteen year gap between possibility and actuality may seem too small to be worth bothering to explain away. I would argue, however, that in the absence of World War II the development of electronic computers would have been further delayed at least ten years, and perhaps fifteen or twenty (I shall have more to say on this in Chapter Six). Thus the normal processes of the economy (barring a Marxist interpretation of the War) would not have reacted to the possibility of electronic computation for perhaps thirty years after it existed, a gap that seems too large to pass off as a mere friction. By contrast, Marconi's development of commercial radio began only seven years after Hertz' laboratory demonstration of the propogation of electromagnetic waves. See William Rupert Maclaurin, Invention and Innovation in the Radio Industry (New York: Macmillan, 1949).

[4]This is a different distinction from the more commonly made one between "hardware" and "software". Both architecture and componentry are part of the hardware. In the early stage of computer building with which we are primarily concerned, software was in a relatively undeveloped state and was of less significance than it is now.

conditional branching? etc. In discussing components, one is concerned not with whether there will be a memory, but with whether the memory will use mercury delay lines, ferro-magnetic cores, or electrostatic vacuum tubes. One is concerned not with what operations will be built into the arithmetic circuitry, but with whether that circuitry will use electro-mechanical or electronic gates. And so on.

To be sure, the components available restrict one's freedom in designing the architecture (or what I shall sometimes call the "logical" design). The process of designing a computer is necessarily an interative procedure. One might design an "ideal" logical structure, and then consider what components are available to realize it. Given the available components, one makes adjustments in the logical structure to take account of reality. If the reality is too restrictive, one looks again at the available components, asking whether there might really be more options than were considered the first time around. Perhaps with a little inventive effort, one could circumvent some of the constraints found in the original survey. One eventually comes up with a proposed design, and the components and logical structure of that proposal are designed for one another, and yet it is useful and convenient to consider them as separate aspects of the design.

The logical structure of the modern computer was in most respects invented by Charles Babbage. The components which Babbage envisioned using were radically different from those used in computers today (or in the 1940's), but the architecture of his machine was strikingly similar. Both machines envision a memory unit, an arithmetic unit, and a control unit. Both envision having the program fed into the machine on some external medium, so that reprogramming the machine involves merely feeding a new, say, deck of cards. It is not clear whether Babbage envisioned having the program stored in the memory, as is done on modern machines; probably he did not. But he does seem to have planned to have the program tape capable of reversing itself, which, in conjunction with conditional branching, which he also envisioned, would allow the machine to carry out iterative programs. Thus Babbage's machine appears to have been a universal computer, as modern machines are. In most respects, then, with the probable exception of stored programming (and also binary operation, though this would probably not have been useful for Babbage, given the components available to him), Babbage's machine anticipated the architecture of modern machines.

That Babbage anticipated these developments does not imply, however, that later computer builders would pay attention to his designs. Babbage was not very good at explaining the reasons for the various features he wanted to build into the machine. And many twentieth century researchers no doubt assumed that they had nothing to learn from someone writing a century before. Aiken seems to have been affected by Babbage more than any of the other American researchers, yet even he does not appear to have understood the significance of conditional branching. But, in any case, Babbage's work was available to anyone who bothered to study the literature before plunging ahead. While he was known only to aficionados in the U.S., he was generally known in Britain. Computational progress was not held up for lack of a theoretical model of how a machine should be built.

The essential components necessary to build a computer were also available at least by 1925. When we speak of the necessary components, we mean primarily electronic components. Electro-mechanical components were available even earlier than electronic ones, but they are not really relevant for our purposes. While it is possible to build a computer with electro-mechanical components, such a machine is so slow that it offers little or no advantage in cost over conventional adding machines. Its advantage lies primarily in its ability to reduce computational error by automating the procedure.

Three-electrode vacuum tubes (or "triode" tubes), the electronic "gate" in an electronic computer, were invented by de Forest in 1906 and were made sufficiently reliable for extensive use in radios during World War I.[5] That the tubes were available does not, however, necessarily indicate that they were suitable for computational purposes. As stated above (ch. 2, pp. 53-54), the major reason for believing electronic tubes to be unsuitable as gates was their alleged unreliability. Their reliability was progressively improved through the 1920's and 1930's.[6] It is not clear exactly when their reliability became good enough for computational purposes. It is true that they were generally thought to be still unreliable when Eckert demonstrated during the War that they could be made reliable enough. How early this could have been demonstrated had someone with Eckert's imagination attacked the problem is anyone's guess. It does appear that most of the improvement

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[5]Maclaurin, op. cit., pp. 74, 90.

[6]G. Wallace Crawford, interviewed by the author, July 11, 1977, in Englewood, N.J.

in reliability between 1920 and 1940 took place in the early 1920's, so that probably tubes could have been sufficiently reliable by the mid-1920's to perform adequately.[7] As noted above (ch 2, p. 54), even if reliability had been one or two orders of magnitude worse than it was, the ENIAC still would have been superior to conventional technology. Eckert has asserted

> ...there wasn't anything in the ENIAC in the way of components that wasn't available ten and possibly fifteen years before they were used. Knobs, switches, and resistors weren't quite as good and a little more expensive, but they weren't so different. The ENIAC could have been invented ten or fifteen years earlier, and the real questions is, why wasn't it done sooner?[8]

Triodes in the late 'twenties and early 'thirties were somewhat slower, but still in the same order of magnitude of speed so that the same basic advantage of an electronic machine would have existed.[9] Fundamentally, though, we will never know how reliable tubes could have been in the mid-twenties, because we will never know how the tube manufacturing companies would have responded to a demand for an especially high-reliability tube. Different characteristics of a tube are important for different applications. In radio applications, low cost was important, because the radio companies were trying to make radios a widely used consumer good. Frequency response was also important (the accuracy with which the plate circuit could be made to mimic the grid circuit). In digital electronics, cost is not so critical (because the

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[7] Ibid.

[8] J. Presper Eckert, quoted in Harold Bergstein, "An Interview with Eckert and Mauchly," Datamation, v. 8, no. 4 (April, 1962), p. 25.

[9] Herman H. Goldstine, interviewed by the author, July 23, 1975, in Princeton, N.J.

cost effectiveness of the computer depends much more on its downtime than on its capital cost), and frequency response is hardly important at all, because the plate circuit must merely be turned on and off by the grid circuit, not form an accurate replica of it. Obviously tube manufacturers in the 'twenties were devoting most of their effort to making the tubes better for radio applications. They did respond to special requests from scientific users, and tried to design tubes to meet their needs.[10] How well they could have responded to a request from a computer builder, had such a request been made, is an unanswerable question.

Some alterations could have been made to improve reliability with very little effort. For example, when I.B.M. started ordering tubes for their 604 electronic calculation and their 701 computer, they were using RCA 6J6 television tubes. One source of failures was the coating on the cathode, which would come off in specks which shorted out the tube. I.B.M. did not need the cathode coating, which was useful only for the television application. RCA was throwing away perfectly good tubes whose only fault was that their cathode coatings did not meet the specifications for television. These tubes were better for I.B.M.'s purposes than the tubes which did meet specifications, so I.B.M. started taking RCA's rejects, and everybody was happy.[11] I.B.M. eventually developed a tube specifically designed for their computers,

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[10] Crawford, op. cit.

[11] R. G. Canning, interviewed by Robina Mapstone, at Vista, Calif., August 10, 1973, for the Smithsonian Institution National Museum of History and Technology Computer History Project (Washington: Smithsonian Institution, 1973), p. 3. Interviews in this series will hereafter be referred to as Smithsonian (CHP).

and persuaded the tube companies to make it for them, but up until this time computers had been built entirely with tubes built for radios and television. No alteration had been made by the manufacturers; the only changes had been made in voltages which could be effected by the computer builders themselves. So it would seem that even very modest efforts to improve reliability, well short of a "forced draft" demand, might have had a substantial effect. It seems a fair conclusion, at any rate, that, from the point of view of component reliability, an electronic computer could have been built at least by 1930, and probably by 1925.

Our conclusion, then, is that from the point of view both of theoretical development and component availability, the electronic computer could have been built in the late 'twenties. That it was not built until the early nineteen-forties must be attributable to factors other than the lack of a critical technical breakthrough. Antecedent scientific and technical change, in this case, was a permissive factor rather than a precipitative one. It was a necessary precondition from the development of the computer, but it did not induce that development directly.

Demand Constraints

A proposition which commands much more general assent among economists who study technological change is that technological change is fundamentally an economic phenomenon, by which is meant that technological change is "determined" by the market demand for potential inventions.[12] There have been a number of studies of this question, most of which indicate that economic demand is a significant factor in influencing the pattern of technological change.[13] With this general conclusion I would not disagree. These authors vary in the caution with which they interpret their findings, Mansfield and Grilliches being relatively cautions, Schmookler being relatively rash. I do not deny that demand affects what sort of inventions are likely to be undertaken. I do, however, wish to sound a note of caution that the relationship between economic demand and invention is a complicated one, especially in the case of radical inventions like the computer. In particular, where Schmookler argues that the role of antecedent scientific and technical change is merely a permissive one, while the role of economic demand is a precipitative one,[14] I would argue that the two roles are more nearly parallel: both antecedent scientific and technical change and economic demand play an essentially permissive role, making an invention possible, but not inducing or precipitating it.

Assuming a computer could have been built in the 1920's, would anyone have been willing to pay for it? More particularly, would it have been worth anyone's while to pay for it? Would the computer have been cost-effective in the 1920's? This questions resolves into two: First, would the computer have been cost-effective if fully utilized? Second,

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[12] Schmookler, op. cit., p. 165.

[13] Schmookler, op. cit., Zvi Grilliches, "Hybrid Corn: An Exploration in the Economics of Technological Change," Econometrica, v. 25, no. 4 (October, 1957), pp. 501-522, and Edwin Mansfield, Industrial Research and Technological Innovation (New York: Norton, 1968), ch. 3.

[14] Schmookler, op. cit., p. 189.

was there enough computational work to do to have kept it fully utilized (or sufficiently utilized to be cost-effective)?

The cost-effectiveness of a fully occupied computer, even of the most primitive form, such as ENIAC, can be easily demonstrated. (Again, these comments apply only to electronic computers. I do not believe the electro-mechanical computers of the Mark I type were ever cost-effective, unless a high value is placed on the elimination of human error which they promise.) The initial capital cost of the ENIAC was about \$500,000. (This includes all the development costs as part of the capital costs for the first machine, a very conservative procedure. Obviously extra copies could have been built for substantially less than the first one. See above, ch. 2, p. 51.) The annual capital cost, including interest and amortization of the principal at a constant annual cost, would be given by the following formula:

$$C = \frac{Ar}{1 - e^{-rL}}$$

where C is the annual capital cost, A is the initial cost of the capital equipment, r is the interest rate, and L is the expected lifetime of the equipment. If we take the interest rate, conservatively, to be 10% and the expected lifetime to be ten years (the ENIAC was in fact used from 1946 to 1955, when it was shut down because it was obsolete; it probably could have been operated indefinitely, with only negligible annual expenses for replacement of tubes, so any lifetime is arbitrary, but ten years is good enough for our purposes), then the annual capital cost would have been \$79,098.83, or, let us say, \$80,000. The ENIAC required a staff of eleven maintenance people and thirteen mathematicians.[15] Engineers in the late 1920's (1929, to be exact) made about $3500 per year, while college teachers made about $3100.[16] If we assume that mathematicians made about what college teachers did, and that all maintenance people were engineers (a generous assumption), then the total payroll for the ENIAC in 1929 (had it existed then) would have been about 11 x 3500 + 13 x 3100 = $78,800.

The cost of electric power would have been large. We must make some allowance for downtime due to failures and reprogramming. The extent of this would vary with the degree of variety of work (hence frequncy of reprogramming) expected of the machine. The larger the allowance for downtime, obviously, the less cost-effective the machine would be. I think an allowance of 50% for downtime would be generous (this is well in excess of actual downtime in the late 'forties). On this assumption, the machine would have been operating (24 x 365)/2 = 4380 hours per year. Since it drew 120 kilowatts (see above, ch. 2, p. 51), this would mean that its electrical power requirements over a year's time would have been 120 x 4380 = 525,600 kilowatt-hours per year. Electricity in 1929 cost 4.33¢ per kw-hr for large users (though it might have cost half that for an unusually large user such as the ENIAC would have been).[17] Using that figure we get an annual electric bill of

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[15]U.S. Office of Naval Research, A Survey of Automatic Digital Computers, by N. M. Blachman (Washington: Office of Naval Research, 1953), p. 32.

[16]U.S. Bureau of the Census, Historical Statistics of the United States, Colonial Times to 1970, Bicentennial Edition, Part I (Washington: G.P.O., 1975), p. 176.

[17]Ibid., Part 2, p. 827.

.0433 x 525,600 = $22,758. Adding up these basic cost categories, then, we get an annual operating cost (including amortization) of approximately $80,000 + $79,000 + $23,000 = $182,000. (This is in 1929 dollars, except for the capital cost. Most of the capital cost was for labor, which would have been cheaper in 1929 [$289/mo. vs. $334/mo. in 1943].[18] The tubes would have cost more, however, so the total cost probably would have been about the same.)

Estimates of the productivity of the ENIAC vary somewhat, but, in terms of the time required to compute a firing table trajectory, all estimates come out at less than one minute.[19] The time required for a human being with a desk calculator to do the same work was, we calculated, about twelve hours (above, ch. 2, p. 48). Thus the ENIAC could do the work of 720 people with desk calculators. Since these people would only work 40 hours per week, while ENIAC worked round the clock, we would need over four times as many people, (168/40) x 720 = 3024, to produce the same work per week as the ENIAC. However, we assume above that the ENIAC would suffer 50% downtime. This would reduce the man-hour equivalent of the ENIAC to 1512 people. Ignoring the electricity required to operate the desk calculators, the cost of the calculators

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[18] Ibid., Part I, p. 176.

[19] On the basis of Goldstine's calculations, the ENIAC should have been capable of calculating a trajectory in about seven seconds. But Clippinger reports a time of 40 seconds for the same calculation. See Herman H. Goldstine, The Computer from Pascal to von Neumann (Princeton: Princeton University Press, 1972), pp. 136-138, 160, and Richard F. Clippinger, interview with Richard R. Mertz, at Cambridge, Mass., December 17, 1970. Smithsonian (CHP), p. 23.

themselves, the mathematicians required to organize the enterprise, and the larger space required to house this army of people, we can see that the ENIAC would have been competitive with desk calculators as long as the annual wage of desk calculator operators was greater than $116 ($176,000/1512 = $116). Clerical workers in 1926 (no data for 1929) made $2310 per year.[20]

The ENIAC, then, would have paid for itself many times over if fully occupied in the late 1920's. Suppose it were not fully occupied? Was there really enough computational work in the 1920's to justify such a machine? Given the cost figures above, the annual cost of the ENIAC was not more than the cost of 76 clerical workers. The ENIAC would do the work of these 76 people in slightly over four hours per week (assuming they worked forty-hour weeks). So the ENIAC would have been cost-effective even if it had only been employed half a day per week. It would have been worthwhile to build even if there had only been 76 people with desk calculators to replace (to say nothing of the improvement in the accuracy and timeliness of the results). Surely there were may insurance companies, banks, universities, and laboratories with these modest computational requirements. While it is difficult to compare the productivity of tabulators and computers, some impression of the size of the market for large scale computational equipment can be inferred from the fact that I.B.M.'s total revenues from tabulators in 1935 were a

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[20] Historical Statistics, Part 1, p. 168.

little over $15 million.[21] They had, at the time, about 3300 customers using tabulators,[22] so their average revenue per tabulator customer was about $5000. These 3300 customers used about 17,000 tabulators,[23] or about five tabulators per customer, on the average. Each tabulator probably required two or three operators, for a total payroll of perhaps $6000 per tabulator, or $30,000 per customer. Thus the average I.B.M. customer in the mid-1930's was spending perhaps $35,000 on large-scale computation. With 3300 customers in all, it seems not unreasonable that the variance in usage would be large enough that a significant number would have been spending in excess of $176,000, and therefore would have been customers for electronic computers.

Moreover, these computational requirements are enough to pay for the machine; any further computation in the eighty hours per week left over after the machine is paid for is free. The most striking feature of computers is that usage tends to expand to fill the time available (to paraphrase Parkinson). People would inevitably start filling up the extra time with applications which they would soon come to regard as essential. Many people in the 1940's and 1950's underestimated the amount of computing people would want to do. We have already mentioned I.B.M.'s estimates of the total market for computers (above, ch. 2, p. 69). Aiken had opposed

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[21] Calculated from data in Saul Engelbourg, "International Business Machines: A Business History," Columbia University Ph.D. Dissertation, 1954 (Ann Arbor: University Microfilms, 1954). I.B.M.'s total revenues in 1935 were $20,885,000 (p. 372), about 74% of which were attributable to tabulators (p. 125).

[22] _Ibid._, p. 322.

[23] _Ibid._

electronic machines partly because he thought they would spew out data faster than they could be digested by people. In general, the computer reduced the cost of computation by such a large amount that it was impossible to estimate how much computation would be demanded at such a low price. One can only make estimates about the shape of the demand curve in the vicinity of those points with which one has had direct experience. Alston Householder's experience at Oak Ridge was typical:

> Before the machine arrived, I used to have nightmares worrying about whether there would be enough problems to keep such a voracious monster fed, you know? But I didn't need to worry, as soon as it got there, people began to get acquainted with what it could do. The thing is, that until a machine of that sort is available, people don't really think about it, but when it's there and they begin to hear about what it can do, and think about how it might be used for their problems, then all of a sudden, there's a whole slug of them. And so...I don't think... I couldn't really trace the growth rate, but in almost no time at all, we had filled up one eight hour shift, and were ready to go to two.[24]

T. R. Thompson's use of the LEO I in an ordinary commercial environment in 1950 (see above, ch. 2, p. 78) suggests that the market, even for the primitive, early machines, would have been wide had it been recognized. Labor costs and accounting procedures had not changed enough from the nineteen-twenties to the nineteen-fifties to convert a non-existent demand into an overwhelming one.

One problem in using computers for commercial purposes is that commercial purposes generally require a great deal of input/output relative to the amount of computation actually done. Thus making full use of the

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[24]Alston S. Householder, interviewed by Richard R. Mertz, July 20, 1970, at Oak Ridge, Tenn. Smithsonian (CHP).

computational capacity of the machine is relatively easy if it is used for scientific purposes, but more difficult if used for commercial purposes. To some extent, scientific applications became more prominent after World War II than they were before. Very extensive calculations were done for nuclear weapons research, for example, and for design work on conventional Cold War weapons projects. None of these demands, of course, were present before the War.

To some extent the growth in scientific computational demand was generated by other forces which were not present in the late 'twenties. One factor, for example, was the growth in the cost of building prototypes of new airplanes for testing.[25] The old school of designing airplanes was to design a plane intuitively, build a prototype, flight-test it, and then refine the design on the basis of the flight test. This might result in the construction of a series of prototypes. As the cost of building prototypes rose, this approach became increasingly expensive, and a demand for computers developed to allow the computer-aided mathematical analysis of the first design, thus making it less likely that additional prototypes would have to be built. It was not until the cost of building prototypes rose that the demand for computers to aid in their design clearly developed.

On the other hand, many types of scientific computation were the _result_ of the availability of the computer. In many cases, scientists had a refined scientific understanding of a phenomenon, but were unable to

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[25]Almarin Phillips, _Technology and Market Structure: A Study of the Aircraft Industry_ (Lexington: Heath Lexington, 1971).

use their understanding because it required computational capacity beyond their grasp. That there was little scientific cmputation being done before the War was due more to the lack of computers than to the lack of a desire or a need to compute. As we noted in Chapter Two (pp. 46-47), the Ballistics Research Labs were constantly making compromises between their desire to use more sophisticated equations for the motion of a projectile in space and the need to use simpler equations because the computational capacity did not exist to make use of the more sophisticated ones.[26] It is in this respect that organizations behave asymmetrically. They do not respond to opportunities to improve the quality of their work as readily as they do to crises which threaten to reduce the quality of their work. Thus the latent demand for computers existed, even if it was not manifested in much computing being done.

Even insofar as the computer would only be used for commercial purposes, however, I think there was still a market for it as early as 1925-1930. The computer need not have taken the form given to it in the ENIAC. The ENIAC was to a large extent a special purpose machine for calculating firing tables. A machine designed primarily for commerical customers would have taken a different form. One model for the form which a commercially oriented computer could have taken was the model 604 electronic calculating punches which I.B.M. introduced in 1948. This machine could be programmed with plugboards to perform a sequence of up to 60 calculations using the data fed in on a card, and then punch out the result on another field of the same card. It could process 100 such cards

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[26]Goldstine, op. cit. (1972), p. 74.

per minute. I.B.M. sold over 5600 in ten years. Such a machine is certainly not a computer, at least according to the definition which we suggested in Chapter One, but it does indicate the possibilities for intermediate machines between the desk calculator and the tabulator, on the one hand, and the stored program IAS-type computer on the other. Such machines were a great improvement over tabulators for commerical applications, because the tabulator can perform only one arithmetic operation for each pass of the cards through the machine. Thus even a simple commercial calculation like a payroll may require several passes of the cards through the machine, thus multiplying the number of machines required. The 604 required only a single pass for almost any commercial computation, yet it also had the great input/output capacity of the tabulating machine. Such a machine would clearly, I think, have found a commercial market, and the fact that such machines were not introduced until after electronic components had already been pioneered on the ENIAC confirms, I think, our argument that the obstacles to computational innovation were not the lack of a suitable market.

There was, then, not only the technical capability to build computers in the 1920's, but also a market for them. It was not a very visible market-- managers of clerical staffs were not crying out for some solution to their computational problems--but it was a market nevertheless. I think if I.B.M., with its marketing organization, had introduced the Model 650 (their early magnetic drum stored program machine, of which they sold over 2000 copies in the 1950's) in the 1920's, they would have profited handsomely. That computers were not introduced until fifteen years later (commercially, for nearly twenty-five years later) indicates that market demand is not a sufficient condition, even in conjunction with technical feasibility, for invention to take place. Market demand, even in conjunction with technical feasibility, does not "induce" invention. It is, like technical feasibility, a permissive factor, a necessary condition, but the two even jointly are not sufficient.

Neoclassical theory, in its pure form, assumes that the firm has perfect knowledge about its environment (or, in the "economics of information" literature, that it has perfect knowledge about the cost and value of searching for information about its environment).[27] Given this perfect information, the firm merely functions as a clerk, processing this information according to a well-defined profit maximizing algorithm to yield decisions on pricing, output, etc.[28] If we can assume that the firm processes this information correctly (a fairly trivial task), then the actions of the firm are strictly determined by the objective conditions of its environment in a perfectly predictable way. Insofar as technological change is concerned, a thoroughgoing neoclassical analysis would imply that if an invention is technically feasible[29] and

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[27]See, e.g., George J. Stigler, "The Economics of Information," J.P.E., v. 69, no 3 (June, 1961), pp. 213-225.

[28]Herbert A. Simon has a particularly striking statement of this in his "Theories of Bounded Rationality," ch. 8 in C. B. McGuire and R. Radnor (eds.), Decision and Organization (Amsterdam: North-Holland Press), pp. 162-163.

[29]"Technical feasibility", of course, is a slippery concept. With enough research effort, perhaps anything would be technically feasible. What we mean by this, vaguely, is that the inventor must only invent the whole, and not the parts. The components must be available for him to put together in a new arrangement. Perhaps some adjustment of the parts would be necessary (as with Eckert's reduction of voltages on vacuum

economically profitable, then some inventor will invent it (neoclassical theory assumes that businessmen are perfect optimizers, so it might as well assume that inventors are always capable of inventing something if it is technically possible to do so). A thoroughgoing neoclassical analysis would thus imply that the objective technical feasibility and economic viability of a potential technological advance jointly imply that the invention will occur. The invention is thus "determinate" in the same sense that the entrepreneur's decision to cut prices when sales fall is "determinate." Most microeconomically oriented economists do not press the neoclassical analysis that hard, but Nordhaus argues something like this in his analysis of optimal patent lives.[30] As we have seen, however, in the case of the computer its technical feasibility and economic viability did not make its invention in any sense "determinate." It was not invented for some time, and wouldn't have been invented for much longer if it hadn't been for the War.

We could amend the neoclassical analysis and argue that, while the individuals involved <u>sought</u> to optimize--sought to achieve the lowest cost technology possible at each point in time--they were prevented from doing so by faulty perception. While the computer was technologically feasible and economically marketable, it was not perceived as such by the inventors of the time. A behaviorist analysis is in large part concerned with doing just this, with showing how people's subjective

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tubes to improve reliability), but technical feasibility implies that the inventor would not have to invent, as Babbage would have had to, not only the computer, but also the completely new metal-working techniques necessary to build it.

[30] William D. Nordhaus, <u>Invention, Growth and Welfare</u> (Cambridge: MIT Press, 1969), pp. 76-82.

perception of a situation systematically diverges from objective reality.[31] People fail to notice some aspects of reality altogether, or they interpret that reality in a special way due to the nature of their own experience. In particular, in the case of invention, a behaviorist analysis would argue that people systematically underestimate the chances of success for a radical innovation, and systematically overestimate the returns to continued incremental innovation. It is not merely a matter of risk aversity, for there are risks in <u>not</u> pursuing radical innovation as well as in pursuing it too avidly. Looked at another way, a behaviorist analysis would argue that firms will be averse to the risk of pioneering a radical new technology, but not averse to the risk of being creatively destroyed when the new technology is developed by someone else.

But a behaviorist analysis goes beyond merely saying that the attempt to optimize is frustrated by imperfect information. It argues that there is no attempt at all, in a well-defined sense, because there is more than one objective function, that the optimization of one implies less than optimization of the others, and that the firm or inventor involved typically does not weigh the significance of his conflicting goals in a consistent way that would allow the definition of a single objective function. This interplay of conflicting goals is important as we shall show in the following section.

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[31] A vague expression, "objective reality." See Thomas S. Kuhn, <u>The Structure of Scientific Revolutions</u>, 2nd ed. (Chicago: University of Chicago Press, 1970), pp. 144-159. As Kuhn suggests, a fact becomes an objective fact, ultimately, when a large enough number of people subjectively perceive it as such.

However, while a behaviorist analysis is in conflict with a neoclassical analysis in that they make conflicting assumptions about the goals of the entrepreneur (or firm, or inventor), they do play complementary roles. The neoclassical analysis focuses on the environmental constraints within which the inventor operates, and assumes that he does the best he can possibly do within these constraints. The behaviorist analysis does not assume that he does the best he can do within these constraints, but the constraints are still important because he still cannot do better than the constraints allow him to do. He is still constrained by the constraints, so that the analysis of the constraints, of the limitations imposed by technical feasibility and of market viability, is still important because it sets the limits within which a behaviorist analysis operates. The problem with neoclassical analysis is that, while it shows the limits to the inventor's action imposed by the environment, these limits are often not the binding constraints. Other constraints limit his action even beyond the limits imposed by the environment. It is these further limits that a behaviorist analysis is designed to elucidate. We shall now develop the elements of a behaviorist analysis and show how it relates to the development of the computer.

A Behaviorist Analysis

This theory is derived from the work, over the last twenty-five years, of Cyert, March, Simon, Nelson, and Winter.[32] The basic

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[32]Herbert A. Simon, "Theories of Decision Making in Economics and Behavioral Science," A.E.R., v. 44, no. 3 (June, 1959), pp. 253-283; Richard M. Cyert and James G. March, A Behavioralist Theory of the Firm

proposition of behaviorist theory is that man, in his economic activity, acts not on the basis of a constant attempt to optimize his situation, even approximately, but rather acts on the basis of a structure of relatively simple rules of thumb (or standard operating procedures). These rules are stable most of the time, but change from time to time as a result of gross changes in the environment. Man thus achieves a reasonable, satisfactory employment of the economic resources available to him, but not an optimal one, in any sense of the word.

The rules, first of all, can be either broad or narrow. They can be narrow pricing rules like the rules in Cyert and March's department store model (e.g., "Divide each cost by 0.6 and move the result to the nearest $.95."),[33] or they can be broad policy rules like "search for new technology in the field of polymer chemistry" (this is essentially the rule under which Wallace Carothers was operating when his research

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(Englewood Cliffs: Prentice-Hall, 1963); Sidney G. Winter, Jr., "Economic Natural Selection and the Theory of the Firm," Yale Economic Essays, v. 4, no. 1 (Spring, 1964), pp. 224-272, and "Satisficing, Selection, and the Innovating Remnant," Q.J.E., v. 85, no. 2 (May, 1971), pp. 237-261; Richard R. Nelson, "Issues and Suggestions for the Study of Industrial Organization in a Regime of Rapid Technical Change," in Victor R. Fuchs (ed.), Policy Issues and Research Opportunities in Industrial Organization (New York: Columbia University Press and the National Bureau of Economic Research, 1972), pp. 34-58; Richard R. Nelson and Sidney G. Winter, Jr., "Toward an Evolutionary Theory of Economic Capabilities," A.E.R. Papers and Proceedings, v. 63, no. 2 (May, 1973), pp. 440-449, "Neoclassical and Evolutionary Theories of Economic Growth: Critique and Prospectus," E.J., v. 84, no. 336 December, 1974), pp. 886-905, and "Factor Price Changes and Factor Substitution in an Evolutionary Model," Yale University Institution for Social and Policy Studies Working Paper #W5-2 (New Haven: Yale University ISPS, January, 1975); Richard R. Nelson, Sidney G. Winter, Jr., and Herbert L. Schuette, "Technical Change in an Evolutionary Model," Q.J.E., v. 90, no. 1 (February, 1976), pp. 90-118.

[33] Cyert and March, op. cit., p. 138.

group discovered Nylon).[34] We shall have more to say later about the circumstances under which broad and narrow rules are adopted and the consequences of the differences between them. In particular, the use of a certain technology can be considered a "rule" or standard operating procedure.

The process by which these rules change is an important part of the theory. At any given time a person has certain goals (strictly speaking, a firm, as an assemblage of persons, cannot have goals; the different members of the firm may have goals, which may well be in conflict with one another, but the firm does not; nevertheless, we often simplify reality by speaking of the goals of the firm). The owner of a business firm may consider profits, sales, inventory, output, and debt-asset ratio as goals. With respect to each of these goals, he will have aspiration levels. He thinks profits ought to be $10 million, sales ought to be $100 million, etc. "Equilibrium" in a behaviorist world, such as it is, is a situation in which all of a person's aspiration levels are being achieved. If this is the case, the person is satisfied, and there is no tendency for his standard operating procedures to change. (In some versions of the model, however, the aspiration levels can automatically ratchet upwards if they are consistently achieved, so that the firm eventually fails to achieve them and change is induced. Equilibrium is therefore only a short-run phenomenon.) If the firm fails to achieve its aspiration levels, then it searches around

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[34] John Jewkes, David Sawers, and Richard Stillerman, The Sources of Invention, 2nd ed. (New York: Norton, 1969), pp. 275-276.

for a new standard operating procedure which it hopes will allow such achievement. The nature of this search routine is an important part of the theory, since the order in which the person considers alternatives largely determines which alternative he eventually chooses ("choice" might well be put in quotes here, since the winning alternative is not so much "chosen" as determined by the nature of the search routine). If he fails to turn up an alternative procedure which allows him to achieve his aspiration levels, he reduces his aspiration levels until they are low enough that they can be achieved. As noted above, if his aspiration levels are consistently achieved, there may be a tendency for them to inch upwards. Perceptual filters are also an important part of the theory. What a person recognizes as a promising alternative procedure is not objectively given, at least in most cases. The evaluation process itself is a rule-bound procedure.

One important characteristic of the search routine by which new standard operating procedures are developed is the concept of vicinity. Cyert and March argue that the search for new procedures will take place "in the vicinity" of the problem which induced the search and "in the vicinity" of the old, unsatisfactory procedure.[35] The concept of vicinity is intuitively simple--the proposition is simply that firms will first of all seek a solution which is "suggested by" the nature of the problem: if a problem manifests itself in one part of an organization, that part of the organization will be expected to come up with a solution, even if the best solution for the problem lies in some other part of the

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[35] Cyert and March, op. cit., pp. 86, 121-122.

organization. Where the problem cannot be localized to a particular part of the organization, the firm will seek a solution based upon its experience in solving similar problems in the past. If a firm experiences a shortfall in profitability, it may start out by seeking a new pricing policy, because that avenue has been successful in the past, even if, say, a change in product technology would be more successful. Having once decided upon the area in which it will seek a solution, the firm then seeks a new standard operating procedure which is similar to the one it was already using. It sticks to its rules of thumb as long as possible, and, when it changes, it changes as little as possible.

The concept of vicinity is necessarily vague and subjective, however. To say that one thing appears "similar" to another in one person's eyes is not to say that they will appear equally similar in another's. A more extended discussion of this is offered in Appendix A, but suffice it to say for here that things (procedures, technologies, or whatever) are defined along many dimensions, and two things may be similar along one dimension and dissimilar along another. The overall evaluation of similarity depends upon how one weights these different dimensions. The choice of weighting scheme is essentially arbitrary, so that different people will use different weighting schemes, and therefore arrive at different judgments about how similar two different things are.

Behaviorist theory can be contrasted with neoclassical theory in a number of important respects. First, neoclassical theory assumes that firms are constantly responding to changes in the environment, constantly seeking the optimum with respect to their latest perception of the environment, either what it is currently or what it is currently expected to be in the future. In behaviorist theory, on the other hand, response only takes place intermittently, even if the environment is constantly changing, because response only takes place to threshold level stimuli from the environment.

Second, in behaviorist theory the firm searches for solutions only within a narrow choice set of alternatives. In neoclassical theory, the firm is assumed to examine all alternatives, and to choose the global optimum. The limitations on the choice set in behaviorist theory take place in two stages. If the firm has a sales goal, for example, and fails to achieve it, then there are a wide variety of strategies open to it. It might cut prices, or change its marketing strategy, or develop a new product. The firm will first constrain its choice of action by only considering actions which can be taken by that part of the firm "close" to the problem. Thus a shortfall in sales will be regarded as a failure of the sales force, so the first alternative action considered will be a change in marketing strategy. Within that general class of actions, the firm's choice set will be further constrained to alternatives similar to what the firm is doing now. That is, the firm will change its marketing strategy, but by no more than is necessary. If the firm fails to find a satisfactory solution to its problem, it will relax these constraints sequentially. It will first consider more and more dissimilar marketing strategies, and then, if no change in marketing strategies seems to solve the problem, it will consider a solution more distant to the problem--a new product, perhaps. Thus search is constrained on two levels, first by proximity to the problem, and second by proximity to the current operating procedure.

At each level, one can imagine that the constraints work in one of two ways. On the one hand, one can imagine that the firm considers solutions as they occur to it (and they occur to the firm in the order of their subjectively perceived proximity to the problem and to the current solution), and that the firm evaluates these potential solutions one by one, stopping when they come up with one which they, again subjectively, perceive to be a successful solution to their problem. In effect then, the firm never looks ahead of the one potential solution it is considering at the moment. Alternatively, the firm could consider a number of possibilities simultaneously, taking, say, the first five potential solutions which occur to it, and comparing them against one another, choosing the one from the group which appears to be best. In the former conception of the firm's behavior, its choice is completely determined by the nature of the search routine, by the order in which alternatives are considered. In the latter version, while it is still basically behaviorist, the choice is incompletely determined, so that there is a more neoclassical flavor, since the firm at least chooses the local optimum from the narrow choice set, rather than simply choosing the first satisfactory alternative that comes its way.

In any case, technological change fits into a behaviorist analysis in one of two ways. The use of a particular technology, first of all, is a "standard operating procedure" in behaviorist terms. In the simplest version of a behaviorist model, a firm would continue to use a particular technology until it failed to achieve an aspiration level. If it thought that searching for a new technology was the most promising way of solving the problem, it would commence a research and development program (a "search routine," in behaviorist terms) until it found a new technology that allowed it once again to achieve its aspiration levels. Once it had found this new technology, its R&D program would lapse. A simple behaviorist analysis, then, envisions an on-again-off-again R&D program, turned on whenever the firm needs a new technology to achieve its aspiration levels, but dormant whenever the aspiration levels are being achieved.

A more sophisticated version would suggest that a firm could have an on-going R&D program, and that this R&D program would itself be a "standard operating procedure." It would not be induced by failure, but would be operating continuously, whether the firm were achieving its aspiration levels or not (indeed, it might add aspiration levels with respect to the "output" of its R&D program). In this case, a failure to achieve aspiration levels would not induce a search for a new technology, since such a search is already going on. It might, however, induce a search for a new R&D policy--i.e., it might induce a change in the R&D program's budget, or in its directives (e.g., to seek a more fundamental technical advance than it had previously been attempting, or a more immediate advance). At any given time, some individuals and organizations in society will be most appropriately modeled by the first model, changing technologies only in response to failure (typically, firms in technologically stable industries), while others will be best modeled by the second model, changing technologies whenever their R&D department comes up with a better one, even if they are achieving all their aspiration levels already. We shall call technological change of the first type (produced in response to failure) "induced" technological change, and technological change of the second type (produced as part of an on-going R&D program) "programmed" technological change. This distinction is not related to Hicks' distinction between "autonomous" and "induced" technological change.

One common pattern in an organization's historical development is for it to begin with no R&D program, and then to experience a failure to achieve aspiration levels which induces not only a search for a new technology, but also (usually only if this search is successful) a change in R&D policy, viz., the establishment of an on-going R&D program.

What would a behaviorist analysis predict concerning computational research? It would, in general, predict that the onset of new technology would be delayed relative to the rate at which "best possible" technology advances. This would occur partly because technological change occurs "in the vicinity" of immediate problems and of technologies already in use, and partly because searches for new technology often occur only when induced by failures to achieve aspiration levels. Because the concept of vicinity is a subjective one, however, the local nature of search routines has a more conservative impact in some cases than in others. We shall also note that both "programmed" and "induced" research have sometimes limiting and sometimes liberating effects on the breadth of search routines.

In developing a behaviorist analysis, we must to some extent step away from the forms of analysis developed by Cyert and March. They were primarily concerned with analyzing organizations; we shall in several cases be concerned with analyzing the actions of individuals.

But the same basic style of analysis will be retained, emphasizing the roles of goals, aspiration levels, and search routines structured by past experience.

One can find elements of behaviorist phenomena in computational developments in the nineteenth century, but this period is not our primary concern. While most developments in computation involved improved arithmetical methods (rather than improved machanisms), this was certainly not for lack of trying on the part of calculating machine inventors. Advances in calculating machines were, for the most part, held up more for lack of technical feasibility (i.e., the poor quality of machine work) rather than because of the narrowness of search routines. It is true that calculating machines, for the most part (Babbage's work is an exception), did not lead in the development of machine work. They generally lagged behind the development of machines like typewriters and sewing machines. That technical problems had a more inhibitory effect on calculating machine developments suggests that, in the ninettenth century at least, the perceived market demand for such machines was relatively modest. Hollerith's tabulator, on the other hand, was (mechanically) a relatively simple machine which probably could have been invented as early as the telegraph (in the 1840's) forty years before it actually appeared. It probably would have been only slightly less cost-effective in the 1850's than it was in the 1890's. The development of the tabulator is, in fact, an interesting prototype of behaviorist phenomena which demonstrates the same kind of crisis-induced technological development as occurred later with computers.

The goal of the U.S. Census Office which was relevant to the development of the tabulator was the timeliness with which it published the decennial census results.[36] As the size of the population grew, and the number of different kinds of tabulations increased, the Census Office could no longer achieve its aspiration levels with respect to timeliness. While it almost certainly could have profited from a more advanced technology earlier (either by publishing the results sooner, or by publishing them on the same schedule with fewer people), it responded only to a deterioration in its level of achievement, and not to the promise of a higher level of achievement. It commenced a search routine for a new technology, which in this case took the rather formal character of a contest. Three proposals were considered, Hollerith's won, and the use of his technology allowed them to achieve the same aspiration levels that they had formerly been accustomed to achieving. Moreover, the crisis at the Census Office had a double effect. Not only did it induce them to change their tabulating technology, but it also led them to change their policy on research and development. For the first time they introduced an on-going program to improve their tabulating procedures. The crisis led them to change their operating procedures both narrowly and broadly defined.

Computational research during the early twentieth century (before 1945) was characterized first of all by a virtual absence of any effort by the adding machine manufacturers or by the electronics manufacturers to develop an electronic computational technology. None of these firms

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[36]Leon E. Truesdell, The Development of Punch Card Tabulation in the Bureau of the Census, 1890-1940 (Washington: G.P.O., 1965).

seems to have shown any interest at all in the possibility until the late nineteen-thirties, and even then the technology was regarded as a curiosity (see above, ch. 2, pp. 43-44). This is not surprising from a behaviorist point of view. The adding machine manufacturers were making incremental improvements upon their adding machine technology, which precluded any serious attempt at developing an electronic technology in the absence of some crisis (which never occurred). The "electronics" manufacturers were really the radio manufacturers. They were interested in improving the electronics technology, but only as it applied to radios. Both are classic examples of "local" R&D.

The research projects which developed more advanced computational technologies can be classed into two groups, those which arose out of on-going research and development projects ("programmed" research), and those which arose out of a failure to achieve aspiration levels ("induced" research). The former group includes the projects headed by Vannevar Bush, George Stibitz, Howard Aiken, and John Atanasoff. The latter "group" is really just one project, the ENIAC project at the Moore School. We shall consider each group in turn.

The "programmed" research projects in fact show elements of both programmed and induced research. Generally speaking, the research carried out by academic scientists and by industrial researchers, like Stibitz, can be considered programmed research. Research is their full-time activity; it is not done simply in response to some failure to achieve an aspiration level. However, while their research <u>in general</u> was programmed, their research on computers was, for all of these academic researchers (including Stibitz, loosely, as an "academic"), induced by some difficulty in computations in some other area of research. None of these men was, at least initially, a "computer scientist." They were trained as engineers, mathematicians, or physicists. Each was doing research in the field for which he had been trained--Bush in electrical network analysis, Aiken on wave mechanics, Stibitz on filter networks, and Atanasoff on the solutions to systems of simultaneous equations. While these research projects were programmed, their interest in computers was induced by their difficulties in accomplishing the computations incident upon their main areas of research.

The significance of this is twofold. First, because they were trained in particular academic specialties, they were inhibited from considering problems whose solution would have taken them outside of their specialties. While they were willing, in a limited sense, to recombine materials with which they were already familiar--Bush working with integrators, Stibitz working with relays, Aiken working, through I.B.M., with tabulating machine parts--none of them was inclined to make a more general inquiry into the problem of machine calculation, to make a more comprehensive survey of possible components, to consider the problem from a more theoretical point of view (Aiken is to some extent an exception to this generalization with respect to logical design, as is Atanasoff with respect to components). None, in short, was interested in becoming a computer scientist. Babbage, despite never having completed a machine, did at least make this sort of general inquiry into the nature of computing. While he didn't get any computing done, he greatly advanced the art of computing theory and technology. The researchers of the 'thirties, by contrast, were all bound, in some degree, to the professions in which they had been trained. They were interested primarily in solving their own problems, and never addressed the problem of solving computational problems in general. It was only after World War II, when the construction of computers became a full time job, that some of these people became "computer scientists." The fact that their interest in computers was induced, then, limited the breadth and comprehensiveness of their research effort.

Second, because their research in their own academic specialties was programmed, the extent of the computational problem it presented was modest. An important characteristic of programmed research is that it sets its own goals. Its goals are not set by some external problem which demands solution, but rather by an initial estimate of what seems feasible and interesting. Researchers in programmed research projects are largely free to set their own goals, and they are likely to set goals which they think they can achieve, which is to say, modest ones. Insofar as these modest research projects produce computational problems, the computational problems are likely to be modest, so that the advance in computational technology required will be correspondingly modest. A researcher in programmed research has, as his goal, simply the advancement of technology. He is likely to set aspiration levels for this advance at modest enough levels that he can be reasonably sure of achieving them. This almost ensures that technological advance will be, except by accident, slow but sure.[37]

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[37]It is true that it is in these programmed research projects that chance most commonly favors the prepared mind. Fundamental advances do emerge from programmed research projects, but they do so more because

These "programmed research projects thus share the characteristics that, because the interest in computation was induced by particular computational problems, their search for computational technology followed the typical behaviorist pattern of narrow, local search. Moreover, because these researchers were free to set their own goals, the goals were set at modest enough levels so that only this narrow, local search was necessary to turn up a technology which would satisfy their goals. They were never forced to consider more radical possibilities. The "programmed" element in their research, because it led to modest goals, thus accentuated the narrow, local nature of their search routines.

This pattern becomes clear in the individual cases of the people involved. Bush is in many respects the most interesting case, since one might have expected that if anyone were to invent an electronic computer, it would have been he. He was quite familiar with electronic components, having consulted with the radio industry since just after World War I, and having been a founder, in the early 1920's, of Raytheon, a vacuum tube manufacturer. His interest in computing extended over many years, as seen by the series of machines he built for that purpose. He knew of Babbage's work, and he taught at MIT, where one might have expected him to take a more fundamental approach to technical problems. That he did not is partly due to the fact that he was familiar with the integrator technology embodied in the differential analyzer (he had used

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the researchers have the leisure to investigate anomalous phenomena that they stumble upon than because they are aggressively seeking a major advance.

it in inventing a surveying machine for his B.S. degree);[38] partly because his precision requirements did not necessitate a digital approach, partly that, when he began his computational work, vacuum tubes really might have been too unreliable for that purpose, partly that vacuum tubes were used, at that time, exclusively for radio applications, so that it would have taken a leap of imagination to apply them to computational work, but most fundamentally because he was primarily interested in analyzing electrical networks, so that he never made the comprehensive survey of computational technology which might have suggested an electronic approach.

Stibitz' choice of technology was clearly affected by the technologies in use by the Bell System. Lacking any powerful impetus to do otherwise, he naturally chose to develop a calculating machine based upon the electro-mechanical relays in use in telephone switching systems. This design bias on the part of Bell Labs continued into the late 1940's, well after electronic machines had been proven feasible. Stibitz' search for new technology was local both in terms of the components he came up with and in terms of the logical architecture of his machine (it was a narrowly designed, special purpose machine).

Aiken had no particular technological bias himself, but he acquired one by securing his financial and technical assistance from I.B.M. I.B.M. was committed to the use of electro-mechanical components and, notwithstanding some research work in electronic components (see above, ch. 2,

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[38] Vannevar Bush, Pieces of the Action (New York: Morrow, 1970), pp. 155-157.

pp. 43-44), apparently considered them a curiosity. This technological bias also continued long after electronic components had been proved feasible. Aiken seems to have internalized this bias (he stated later than he didn't think electronic components were worthwhile anyway since they would produce output too fast to be digested),[39] and his second machine, the Mark II, which was not built with I.B.M.'s assistance, was also electro-mechanical. I.B.M. continued their research in electronic components, but resisted their use. Their SSEC machine, built in 1947, still used a large number of electro-mechanical parts, and in 1950 they were still refusing to hire electronic engineers because they were not useful in their "fundamental business."[40] While the components Aiken used were the product of a narrow search routine, the architecture of his machine seems the result of a considerably broader one. While he was not the only one of these researchers familiar with Babbage, he seems to be the only one to become genuinely enthusiastic about Babbage's vision. Aiken seems to have failed, however, to have understood the significance of some features of Babbage's plan, such as conditional branching.

Atanasoff's approach was the opposite of Aiken's. Where Aiken adopted a conservative component design but an ambition architectural design, Atanasoff incorporated advanced components in a more narrowly

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[39] Dr. Charles C. Bramble, interviewed by the author, September 7-8, 1974, at Franklin, N.H.

[40] Keith Uncapher interviewed by Robina Mapstone, February 20, 1973, Smithsonian (CHP), p. 1. See also the interview with Derrick H. Lehmer by Uta Merzbach, October 8, 1969, at Berkeley, Calif., Smithsonian (CHP), pp. 3-4.

focused architecture. Atanasoff's willingness to use electronic components was probably due to the fact that he had little experience in computational equipment, and hence little bias one way or another, and that he had probably been exposed, as a physicist, to the use of electronic components in counting circuits (Aiken was also a graduate student in Physics, but he had spent more than ten years as an engineer for Westinghouse before entering graduate school, so he might have been more attuned to conservative engineering practice). In any case, Atanasoff's interest in computing was a derivative one, and he largely abandoned it when the War started, having done little if any computing with his machine.

One might argue that this behavior can also be explained in neoclassical, optimizing terms. One could argue that the risk of developing a technology with which one is already familiar is less than that of developing an unfamiliar technology, and that, if people are risk averse, they will choose the familiar technology even if they think the "mean performance" of the other technology would be higher. This argument founders, however, on the fact that there is no evidence that people like Buch, Stibitz, or Aiken even <u>considered</u> using electronic components. Rather than examining all possibilities and choosing the one with the best cost-performance-risk characteristics, they examined only the technologies akin to their previous experience. They searched no further in considering possible technologies, because the technologies at the beginning of their search routines promised a satisfactory solution to their computational problems. Even afterward, when these researchers became aware of the ENIAC project, it is clear that they did not reject the ENIAC on the basis of higher risk. They either believed that the extra performance was not useful (Aiken) or they believed that it simply would not work. Ir was not that they applied different degrees of risk averseness to the data; it was that they perceived the data differently. It was not that one group saw a ten percent chance of failure as being an unacceptable risk while the other group did not; it was that one group saw the chance of failure as 80% while the other saw it as 5%. This biased perception of reality is a typical behaviorist phenomenon. The direction of the bias is largely predictable on the basis of the past experience of the actors. Neoclassical theory assumes that there are agreed upon objective facts to which the economic actors respond in accordance with their utility surfaces. This is not the case. The data are perceived differently, and different people with different experiences (who might have very similar utility maps) act differently in accordance with their different perceptions of what objective reality really is.

One might similarly argue that this sort of behavior represents optimization of search activity, that the actor conducts a limited search so as to expend time and money on search only so long as the value (in terms of improved technology) of the marginal datum gleaned from search is at least as great as the marginal cost of searching out that datum. It is, first of all, difficult to see how anyone could measure the value of the search until after he had actually undertaken it. Even if he did, how would he optimize the amount of search undertaken to measure the value of the search for new technology? Every optimizing step implies some pre-existing information on which the optimization is based, so the assumption of optimization always begs the question of how the optimizer gathered the pre-existing information in an optimal manner.[41] In any case, there is no evidence that any such evaluation of the likely payoff to more extensive search ever took place. If it did, the fact that the search was not undertaken indicates that the researchers grossly underestimated the value of such search. The search costs involved were quite modest. Atanasoff developed a machine based on electronic components on a research budget of a few hundred dollars,[42] and the payoff to such research was large. The real question then becomes, why was the value of such search systematically underestimated? Only a behaviorist analysis seems to offer an answer.

The ENIAC project, by contrast, was a clear example of induced research. As it evolved into the EDVAC project, however, it took on the characteristics of programmed research, with important consequences. The researchers working on the ENIAC, while they had the same sort of academic backgrounds as the others we have considered, differed in a key respect. They were not trying to solve computational problems incident upon their own research projects; they were contract researchers, trying to solve a computational problem for the Army. The Army, meanwhile, was not really trying to achieve a higher aspiration level than it had previously achieved; it was merely trying to return to a level of performance

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[41] See Winter, "Economic Natural Selection and the Theory of the Firm," op. cit., pp. 229-230.

[42] John V. Atanasoff, "Computing Machines for the Solution of Large Systems of Linear Algebraic Equations" (Ames, Iowa: Iowa State College, August, 1940), reprinted in Brian Randell, The Origins of Digital Computers (New York: Springer-Verlag, 1975), p. 324.

it had achieved previously. The key goal for the Army, in this case, was timeliness in the calculation of firing tables. Before the War, the number of new kinds of ordnance being developed was small enough that timeliness in calculating firing tables was not difficult to achieve. When the War came, this timeliness became much more difficult to achieve. The Army needed a great advance in computational technology just to stand still with respect to timeliness of firing table calculation. If the Army had been trying to do more than it had achieved previously, then its sense of urgency would have been less. Because it was just trying to maintain its accustomed level of performance, its urgency in trying to solve its computational problem was greater (the importance of its accustomed level of performance was, of course, also greatly increased by the war).

The urgency of the problem presented to the ENIAC people had two important effects. First, it led the group to attempt to use a relatively ambitious component design, viz., electronic vacuum tubes. The willingness to use vacuum tubes was encouraged by the fact that Mauchly, as a physicist, had not had sufficient experience with computational equipment to acquire a loyalty to a particular component technology. He could make a relatively open-minded examination of various possible technologies, examining both Stibitz' machine and Atanasoff's. Nor did his patron have a strong commitment to any particular technology (as Aiken's did). The Ballistics Research Labs used both differential analyzers and I.B.M. tabulators, and thus had some bias toward mecahnical technologies, but this bias was somewhat weakened because they were a user, rather than a manufacturer. In any case, its effect was weakened because the ENIAC work took place in Philadelphia rather than in Aberdeen. There was some grumbling at Aberdeen that the ENIAC project was a waste of money,[43] but Aberdeen was concerned with larger controversies, and the physical distance separating the project from its sponsor probably muted what otherwise might have been a substantial obstacle to approval. If Mauchly had any bias, it might have been in favor of electronic components, since, as a physicist, he probably would have been aware of their use in counting circuits. The ENIAC project subsequently acquired a technical bias when Eckert, with his background in electronics, joined the project, but this was a chosen bias, not an inherited one.

The second effect of the urgency caused by the induced nature of the ENIAC project was that the architecture, the logical design, of the ENIAC was quite narrow. The machine was designed for calculating firing tables, which it could do quite well, but it had little flexibility for solving other problems. In this respect it resembled the earlier projects. It was programmed by plugboards, which made programming quite awkward, and had very limited electronic storage, limiting the range of problems it could solve. It also used a sort of "brute force" design, requiring a very large number of tubes for every number stored. Later, more sophisticated designs greatly reduced the number of vacuum tubes required to store each number. The consequences of this rather simple design were reduced reliability and reduced storage capacity. Thus the urgency of the problem which ENIAC was designed to solve led to the

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[43]Goldstine interview (1975), op. cit.

design's being quite ambitious in some respects, where performance was important for solving that problem, but quite primitive in others where it was not. The induced nature of the project accentuated the localness of the search routine where performance was not critical, but relaxed it where performance was critical.

By the time von Neumann joined the project, the ENIAC design was complete, and any further computational research done by the group would not be so oriented toward solving a particular problem. The group had, by this point, become sufficiently enthusiastic about computers that they were beginning to be interested in designing computers for their own sake, not for the assistance that would be provided in solving their own (or the Army's) computational problems (this effect was accentuated by the fact that the War forced several of them to break pre-war ties and to leave behind pre-war interests, an effect which will be considered in greater length in Chapter Six). They were, in short, on the verge of becoming "computer scientists" rather than mathematicians, physicists, engineers, etc. Von Neumann was particularly valuable in this respect. The range of problems which he wished to solve was sufficiently broad that he was more inclined to look at the computational problem in general than at the problem of accomplishing particular calculations (this inclination was reinforced by his exposure to Turing's article on computable numbers and by his interest in the parallels between computing and neural phenomena (see above, ch. 2, p. 61). He was thus more inclined to adopt the comprehensive view of the problem of the "programmed" researcher, but here one whose central interest is computation itself, not the problems which can be solved with computation.

His brilliance allowed this comprehensive view of the problem to be particularly insightful.

What made the ENIAC-EDVAC project particularly significant was the concatenation of two factors. First, the induced nature of the initial research produced a dramatic, but narrow, leap forward in component technology. Second, the appearance of von Neumann on the scene broadened the range of application of that dramatic component advance.

"Programmed" and "induced" research projects thus have mixed advantages and disadvantages. Induced research projects tend to be more intense and more urgent, and thus lead to more ambitious attempts at technological advance (at least when the problem which induces the research is not created by the other research interests of the person ding the research). Programmed research tends to be less urgent, but more broad-guaged. It is concerned with solving a broader range of problems, and thus it attempts to advance technology on a broader front, not just that part of it which will be useful in solving a particular problem. The dramatic advances in computational technology which occurred during the 1940's seem to be due in large part to a fortuitous combination of induced and programmed research projects. This analysis is somewhat at variance, I think, with the conventional views about the likelihood of fundamental advances to emerge from on-going research projects as opposed to ad hoc projects induced by failure. While on-going "programmed" research is more likely to adopt a broader view of the problem, to consider a broader range of alternative solutions, perhaps to come up with a more comprehensive solution, perhaps to stumble upon a radically different technology, it is also more likely to be discouraged if it encounters difficulties. Programmed research addresses chosen problems. If a problem proves difficult, or its solution risky, then the researcher is free to switch to a different problem. In induced research, however, the problem is not of the researcher's own making, so he cannot make it go away. This leads, I think, to a more intense effort to solve the problem, though the solution may have narrower applicability. Necessity is still the mother of invention.

One type of analysis which is closely related to the behaviroist analysis offered here is the work of Nathan Rosenberg on "focusing devices" and the work of Nelson and Winter on "natural trajectories."[44] These writers focus on the process by which problems are posed in technological research. Rosenberg emphasizes the fact that the solution of one technical problem often creates another, leading to a "compulsive sequence" composed of a series of "technical imbalances." The archetypal example is the development of the British textile industry in the eighteenth century. He also emphasizes the role of constraints, such as labor stoppages, wartime raw material cutoffs, etc., on inducing technological advance to circumvent the lack of the resource. The "compulsive sequence" phenomenon is well illustrated in the development of input/output equipment in the computer industry in the late 'forties

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[44] Nathan Rosenberg, "The Direction of Technological Change: Inducement Mechanisms and Focusing Devices," Economic Development and Cultural Change, v. 18, no. 1, pt. 1 (October, 1969), pp. 1-24. Reprinted in idem, Perspectives on Technology (Cambridge: Cambridge University Press, 1976), pp. 108-125, and Richard R. Nelson and Sidney G. Winter, "In Search of Useful Theory of Innovation," Research Policy, v. 6 (1977), pp. 56-60.

and 'fifties. Electronic components had so speeded up the central processors that the input/output equipment now constituted a bottleneck, so that much of the inventive effort went to developing tape drives, magnetic drums, and other peripheral equipment for feeding data into the machine at high speed. The constraints about which Rosenberg speaks are essentially the same as the crises which we have emphasized here.

Nelson and Winter similarly emphasize "natural trajectories," the "inner logic" of technological development within what they call a "technological regime," what Kuhn might have called a technological "paradigm."[45] While these natural trajectories are similar in many ways to Rosenberg's compulsive sequences, Nelson and Winter identify a few specific forms which are of special importance. One of these is the progressive exploitation of scale economies, e.g., in electric power generation, which is a natural outgrowth of a growing market and reduced transportation (and, in the case of electric power, transmission) costs. Another is the progressive mechanization of various operations, which developed as the first item in the technological search routine in response to the secular rise in the wage-rent ratio and the continuing difficulties of capitalists with labor management problems. More recently, natural trajectories have developed in the application of microprocessor technology to an increasingly wide variety of applications,

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[45]Indeed, Kuhn's analysis of scientific revolutions bears some similarity to my analysis of what might be called "technological revolutions." See T. S. Kuhn, The Structure of Scientific Revolutions, 2nd. ed. (Chicago: University of Chicago Press, 1970).

and in the replacement of natural with synthetic materials. Underlying these natural trajectories is the fact that a group of technologists acquire a certain kind of expertise, either the ability to design chemicals to mimic natural materials, or the ability to design mechanisms to mimic human dexterity. They seek opportunities to apply their expertise to a continuing series of problems, until the supply of unsolved problems soluble with that expertise is played out. Then a new trajectory must be launched.

The development of the computer was essentially the launching of a new such natural trajectory. The obstacles to the development of the computer lay in the fact that the various researchers were working on their own natural trajectories--using various mechanical technologies to solve problems. As it happened, however, computing is rather ill-suited to solution with a mechanical technology, at least with the very large-scale mechanical technology embodied in the Mark I. A new natural trajectory was needed, but it was unlikely to be launched in the absence of some critical need. The War provided that crisis, that critical need, and the electronic genre of technology, which had been up until them the means of producing an entertainment gadget, became the foundation for a growing structure of technological solutions. Thus this paper is largely concerned with analyzing the transition from one "natural trajectory" to another.

The Post-War Period

The post-war period was, of course, marked by the proliferation of a great number of computer projects (see above, ch. 2, pp. 62-77). One of the most significant characteristics of this period was that a large number of people became "computer scientists." People who had been trained in a number of fields, and who were employed in various kinds of work, were exposed to computational research and were, in many cases, hooked. Computing was no longer a derivative interest for them; they became committed to advancing computational technology. The intellectual appeal of computers was no doubt largely responsible for this; the promise of financial gain may have had some effect (if the latter was significant, it was probably misplaced; it is my impression that most computer pioneers, like inventors generally, did not become rich). From a behaviorist point of view, one can regard a person's career as "standard operating procedure." He does not flit back and forth from one line of work to another with their changing levels of remuneration--he tends to choose a career and stick with it. The various government-financed war-time computation projects were significant not just because of the technology that they developed. They are also significant because they got people started in careers as computer scientists. Once started, they tended to stick with those careers as long as their aspiration levels with respect to income, prestige, etc., were being satisfied.

Not only did they tend to remain computer scientists, but they tended to attempt to secure the economic stability of their careers by selling business and government on the value of advanced computational technology. Government-financed research on computer technology thus had a double effect. It not only had the direct effect of developing computational technology, but it also had the secondary effect of creating a class of people skilled in that technology who pressed for additional funding from government and business. New technologies do not sell themselves, no matter how advantageous they are. Especially with a radical technology like computers, the selling effort of its practitioners was important. That selling effort was largely a by-product of early government support.

Concluding Comments

In one respect, at least, neoclassical and behaviorist analyses are quite parallel. Both place primary emphasis on the importance of defining the desired characteristics of the invention to be produced. The distinction made by Schumpeter between invention and innovation, and the importance placed upon the innovator rather than the inventor, are reflections of this. Many things are invented, but the crucial role of the innovator is to decide what inventions are really worthwhile and should be put into production. Similarly, the emphasis here has been on the process by which research questions are posed. The implicit assumption has been that, if researchers work hard enough at a problem, they are likely to come up with a solution (assuming one is technically feasible). The critical question is, why do they choose to solve this problem rather than that one? We argued, first of all, that there are problems which are technically feasible which they do not choose to solve (such as the problem of designing an electronic computer in the 1920's or 1930's). We departed from the neoclassical mode of analysis by arguing that they may also choose not to produce solutions which are both feasible and profitable. We argued, instead, that while technical feasibility and economic feasibility are necessary conditions for technical advance, they are not, either singly or jointly, sufficient conditions. Further conditions are necessary. Circumstances must combine so as to present this particular problem as a desirable and feasible problem to solve. In general, the more radical the problem, the less likely it is to present itself as an important problem to solve. As we shall develop in greater length in Chapter Six, the War played an important role in establishing the circumstances that made advanced computing an important problem to solve. A dramatic crisis, like a war, will generally make radical problems more pressing than they will be in peacetime, and this is an important factor in the contribution which war makes to technological change.

But individual factors were also important in posing the problem. Probabbly von Neumann's most important contribution was not stored programming, but rather the idea that it was important to achieve universality in a computing machine. Stored programming was an ingenious solution to the problem of achieving universality, but probably the most important contribution was defining what was the important problem to solve, not actually solving it. Most computer researchers before von Neumann had been concerned with solving their own computational problems. They therefore built machines which were good at solving their individual problems, but less good at solving other problems. As we have mentioned above (ch. 2, pp. 61-62), von Neumann's definition of the problem probably was due partly to his consciousness of the parallels between neural networks and computational networks, and his belief that the th htening of those parallels was desirable. Breadth of vision plays an important role in defining what problems are important to solve. A problem may be of enormous significance, as the computational problem was, but this is often not self-evident. It sometimes takes a peculiar perspective (in particular, one which is not primarily economically motivated) to be aware of the data which neoclassical analysis blithely assumes are known.

CHAPTER FOUR

THE FACTOR-SAVING DIRECTION OF COMPUTATIONAL INNOVATION

The literature on the economics of technological change is partly concerned with the rate of technological change (including the timing of principal inventions), an issue we discussed in the last chapter. It is also concerned with the direction of technological change, in particular with its bias in favor of saving one factor or another. We turn in this chapter to the latter issue.

The literature on the factor-saving bias of technological change has largely emerged from the literature on economic growth theory, and consequently is largely concerned with technological change as a macroeconomic phenomenon, i.e., with the overall rate of productivity growth. This literature derived from the commonplace observation that technological change seems to have been relatively labor-saving over the last two hundred years. Indeed, if Kaldor's "stylized facts" are to be believed, it has been just sufficiently labor-saving to compensate for rising wages and keep the imcome shares of capital and labor equal (i.e., it has been Hicks labor-saving but, due to inelastic factor substitution, Harrod-neutral.[1] It seemed too great a coincidence that this behavior

[1] Nicholas Kaldor, "Capital Accumulation and Economic Growth," in F. A. Lutz and D. C. Hague (eds), The Theory of Capital (London: Macmillan, 1965), p. 178.

of technological change could be purely accidental, so economists for the last forty years have proposed various mechanisms which would make a capitalist economy automaticaly induce just the right degree of labor-saving technological change. This chapter will briefly review this literature and then examine the pattern of factor-saving in the computer industry to see if that pattern is consistent with the various theories of induced innovation.

The current discussion stems from John R. Hicks' Theory of Wages (1932).[2] Hicks first defined as "labor-saving" any invention which increases the marginal product of capital relative to that of labor (at any given capital-output ratio), thus inducing a shift from the use of labor to that of capital. He argued that such invention had in fact been the preponderant type, and that this was not at all surprising. "A change in the relative prices of the factors of production is itself a spur to invention of a particular kind--directed to economising the use of a factor which has become relatively expensive."[3] The secular rise in the capital-labor ratio, leading to a rise in the wage-rent ratio, naturally led to efforts to economize on the factor growing increasingly expensive.[4]

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[2] John R. Hicks, The Theory of Wages (London: Macmillan, 1932).

[3] Ibid., p. 124.

[4] Pigou had also noted the preponderence of labor-saving inventions (according to his own definition), but had not argued that there was any natural tendency for a market economy experiencing secularly rising relative wages to produce such inventions atuomatically. See Arthur C. Pigou, The Economics of Welfare, 3rd Ed. (London: Macmillan, 1929), p. 673. The labor-saving character of invention in the nineteenth century was frequently remarked upon, and the explicit intent that the

Salter.--Hicks' proposition remained the almost unchallenged conventional wisdom until questioned by Salter in 1960.[5] Salter began by suggesting that at any given time technological knowledge exists in what might be called two stages of "readiness." Some technologies have been fully developed, tried, and proven, and are immediately available from their manufacturers, "off the shelf," as it were. Other technologies are known, in a general sort of way, to be technically feasible--they are "within the fold of existing knowledge"[6]--but they have not yet been developed into finished form, because they do not promise any economic advantage over existing technology. However, such feasible but not yet developed technologies will be developed if factor prices change in such a way as to make them economically feasible.

In Figure 1, we represent the various possible technologies as points along the familiar technology isoquant. Curve FF represents the "fold of existing knowledge" of the "fund of knowledge." Technologies along this curve are technically feasible, at least probably, but have not necessarily been reduced to practice. Tangent to this curve are various other curves such as aa, which represent technologies which have been reduced to practice. These technologies are available "off

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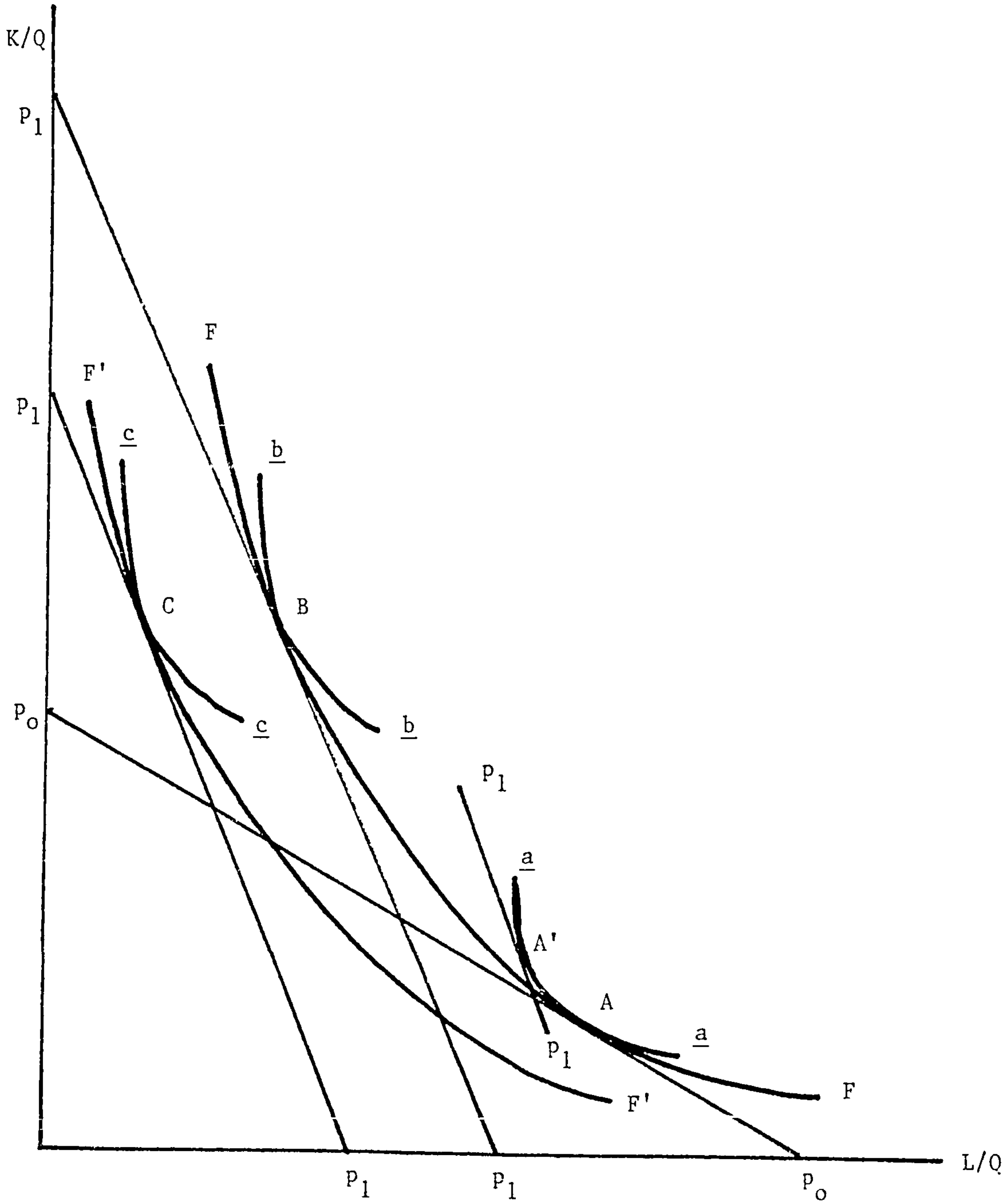
invention should save labor was commonly admitted by the inventors. See H. J. Habakkuk, American and British Technology in the 19th Century (Cambridge: Cambridge University Press, 1967), pp. 100, 120, and 104, but the labor-saving intent seems to have been generated more by the inelasticity in supply of labor than by its high or rising price. See the discussion of Fellner below, pp. 139-140.

[5] W. E. G. Salter, Productivity and Technical Change (Cambridge: Cambridge University Press, 1960).

[6] Ibid., p. 43.

FIGURE 1



the shelf" from their manufacturers. Some degree of substitution is possible within each of these developed technologies, but the range of substitution (its elasticity) is less than is possible among all feasible technologies.

Salter argued that the FF curve advances exogenously toward the origin in a direction determined solely by technological possibilities. The position along the FF curve, however, is determined by factor prices. If factor prices are such that their ratio is represented by the slope of $p_o p_o$, then firms will use technology aa and operate at point A. If factor prices shift so that they are represented by $p_1 p_1$, then the firm will initially substitute within the possibilities allowed by its current technology, moving to A'. Soon, however, the firm will exploit the possibility of bringing the technically feasible technology bb to a commercially usable stage of development. Whether one calls the shift from A to B invention or factor substitution is a semantic question. It would probably be considered invention by the firm responsible for it, and the shift may encounter technical difficulties much greater than originally anticipated. Nevertheless, the degree of inventiveness required is less than that involved in moving from point A to point D, on a more advanced fund of knowledge curve. Thus, while both B and D involve the same unit costs, B can be reached more easily.

Salter stated that if Hicks meant by induced factor-saving technological change solely the movement from A to B, then he had no quarrel with him, but if Hicks meant that the FF curve moves in a way calculated to economize on a particular factor, then Salter could not agree, because the inventors responsible for the shift of FF would not, Salter implied, find one sort of factor-saving technological change systematically more desirable or easier to obtain than another, and therefore the curve would move toward the origin in a random manner. It makes little difference in any case. In Salter's view, concurrently with the factor-price induced shift from A to B, the FF curve can be expected to advance to F'F', bringing us to C. Salter and Hicks agree that the net result will be a movement from a point like A to a point like C, and the exact mechanism does not really matter for the purposes they have in mind.

Fellner.--Fellner proposed two somewhat more specific mechanisms which would induce technology to shift in a particular direction.[7] He argued that if factor prices have been shifting in one direction for a long period of time, innovators will tend to assume that this secular trend will continue and will take this into account in planning what sort of invention to carry out. Since the research and development required to produce an invention is an investment which will be repaid only over future years, the firm must take future factor prices into account in deciding upon the optimal technology to develop. If the firm expects the relative price of one factor to rise, then the nature of invention will be consciously directed to economizing on that factor.

Secondly, Fellner argued that the inelasticity in supply of one factor will cause a firm to expect the price of that factor to rise if the firm expects to expand. This will have the same effect of encouraging

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[7]William Fellner, "Two Propositions in the Theory of Induced Innovations," Economic Journal, v. 71, no 282 (June, 1961), pp. 305-308.

any expansion-minded firm to seek a technology which economizes on the factor in most inelastic supply.

Strictly speaking, Fellner's propositions do not alter the force of Salter's analysis. Both propositions in effect imply that the firm will choose the technology not with respect to current factor prices, but with respect to some weighted average of current and future factor prices. It is not clear whether Fellner's comments refer to Salter's "fund of knowledge" curves (FF) or his "available process"curves (aa). If the former, then the difference between the two essentially amounts to the fact that Salter believes that the FF curves will move forward without regard to the position on the FF curves at which firms are currently producing, whereas Fellner believes that the present and prospective positions on the curves affect the direction in which they will move. There may be good reasons for Fellner's point of view, but if this is it, he does not support it analytically. If Fellner's comments refer to the available process curves (aa), then there is no real difference between their points of view. Fellner in effect is merely saying that firms will anticipate the shift in factor prices from $p_o p_o$ to $p_1 p_1$ and move directly from point A to point C, rather than first, myopically, going to point D and then shifting to point C upon the change in factor prices. The latter interpretation is better supported by Fellner's analysis. If that is the substance of his position, then the differences between his view and Salter's are inconsequential in terms of mapping out the sequence of technologies over time. Nevertheless, the processes which Fellner discusses illuminate more of the micro-structure of the process of technological advance, and will therefore be useful in analyzing technological change in computers.

Kennedy.--The induced innovation mechanisms proposed by Hicks, Salter, and Fellner all tend to make innovation labor-saving in the presence of rising relative wage rates, but they do so to no *a priori* specifiable degree. Innovation may indeed by capital-saving, despite the impact of the inducement mechanisms, if the underlying technical possibilities are strongly enough biased toward capital-saving. None of these theories are of much help to the neoclassical macro-theorist who seeks a more reasonable assumption for his macro-model than the assumption of Harrod-neutral technical change. Kennedy has proposed a model which remedies this problem by producing not only (Hicks) labor-saving technical change but, in conjunction with inelastic capital-labor substitution, just enough Hicks labor-saving technical change to overcome the effect of the rising wage rate and produce Harrod-neutral technical change.

The mechanism which Kennedy uses to induce just the right amount of Hicks labor-saving technical change is the "innovation possibility frontier," which posits a trade-off between labor-saving and capital-saving technological change.[8] Entrepreneurs are presumed to perceive that the more that one factor is saved, the less the other can be, for a given amount of innovational effort. Large savings of one factor may occur only at the cost of increases in the use of the other. Kennedy's conclusion is that, in equilibrium, the marginal rate of substitution between labor-saving and capital-saving technological change will be

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[8]Charles Kennedy, "Induced Bias in Innovation and the Theory of Distribution," *Economic Journal*, v. 74, no. 294 (September, 1964), pp. 541-547.

equal to the relative shares of capital and labor in total costs. For a simple one-good model, Kennedy argues, the rate of increase in total factor productivity will be equal to the rate of increase in labor productivity, and therefore technological change will be purely labor-augmenting, hence Harrod-neutral.

It turns out, however, as Samuelson pointed out, that this is only true if the elasticity of substitution is less than one.[9] It is also true that the equilibrium described can only be reached if the innovation possibility frontier is constant over time. If it is not constant, equilibrium, while tended after, will never be achieved, and the constancy of income shares, which occurs only in equilibrium, will never obtain. The slope of the curve where the capital productivity is zero must remain exactly the same over time--any change in the slope would imply a change in the relative income shares. This is assumed to occur even though the technology to which these trade-offs refer is constantly changing. This constancy of the curve over time seems at least as unsupportable as the existence of precisely Harrod-neutral technological change, so it appears that Kennedy has merely traded in one unrealistic assumption for another. Bowley's Law seems as much a coincidence as ever.

David.--Economic historians have also been concerned with the issue of induced factor-saving technological change. Habakkuk considered fifteen years ago why American technological change had been

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[9] Paul A. Samuelson, "A Theory of Induced Innovation Along Kennedy-Weisacker [sic] Lines," The Review of Economics and Statistics, v. 47, no. 4 (November, 1965), pp. 343-356.

more labor-saving than technological change in Britain during the nineteenth century.[10] Some of his perceptions were formalized by Paul David in a recent essay.[11] David's objection to the usual neoclassical analysis is that it assumes that the nature of the advance of technological possibilities does not depend upon the actual technologies in use at any given time. Salter's "fund of knowledge" curve (what David calls the "fundamental production function") advances without regard to the particular technology being used at the time. David argues instead that that portion of the "fund of knowledge" curve will advance most rapidly which is currently in use. Inventors will tend to improve upon the technology they are currently using. Thus, if the current technology is labor-intensive, firms will tend to improve labor-intensive techniques, but this may well have little impact on improving the state of capital-intensive techniques (this idea has some similarities to the behaviorist notion that technological change takes place in the vicinity of the current technique). As a result, the shape of the fund of knowledge curve is gradually distorted in such a way as to favor the current factor ratio. If we start with labor-intensive techniques, most of our technological change will take the form of improving labor-intensive techniques, and such techniques will gradually advance to the point that they are optimal over a wider and wider range of factor prices. Of

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[10] Habakkuk, op. cit.

[11] Paul A. David, "Labor Scarcity and the Problem of Technological Practice and Progress in Nineteenth-Century America," in Paul A. David, Technical Choice, Innovation and Economic Growth (Cambridge: Cambridge University Press, 1975), pp. 19-91.

course, if factor prices are changing, appropriate substitution will take place, and technological change will then be "locally neutral" from the new point on the fund of knowledge curve. The intrinsic technical possibilities may be more favorable along one capital-labor ratio ray than on another, so that if production is taking place at two different geographical locations (due to different factor prices in the two locations maintained by immobile factors), technological change may occur more rapidly in one location than at the other. Progress may be so rapid at one location that its technology eventually becomes optimal even at the factor prices prevailing at the other location. This, David suggests, is what happened as between Britain and American in the nineteenth century.

The foregoing summarizes a few of the key propositions in the literature on induced innovation. The literature is designed in part to explain the general drift of technology in the (Hicks) labor-saving direction over the years, and in part to explain the more narrow phenomenon of technological change turning out approximately Harrod-neutral over the years. I shall in what follows recapitulate developments in computational technology from the point of view of factor saving and suggest the extent to which the experience of the computer industry is in conformance with the hypotheses about factor-biased innovation proposed above.

The Concept of Capital

Before we can discuss whether or not technological developments in the computer industry were consistent with the hypotheses advanced above, we must be clear what concept of capital we are using.

There has obviously been a great deal of hand-waving by economists whenever discussions of "capital" arise. The old concept of capital as a fund of money is relatively straightforward, but the concept of aggregate physical capital is hopelessly confused. We cannot hope to resolve these confusions here; we shall only catalogue a few of the different ways in which the word capital (in the sense of physical equipment) is used, and suggest one possibly illuminating interpretation for the computer industry.

The simplest way of measuring capital, and the way which is used by default in all empirical studies using the concept of aggregate capital, is in terms of value. Salter in particular uses this approach, since he is not concerned with the marginal products of "old" capital, but only with those of new capital, so he is not concerned with aggregating old and new capital stock.[12] However, all the other theories measure capital in physical units, and this is necessary whenever we wish to measure the marginal product of capital (as with Hicks), or to speak of the effect of changing factor prices on the use of capital (as with Fellner and David), or to talk about the increase in the average productivity of capital (as with Kennedy). In these cases a physical measure of capital is required, but no such physical measure exists. A consideration of the possibility of measuring the "amount" of physical capital embodied in computers might be instructive as to the difficulties with this concept.

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[12]Salter, op. cit., pp. 17-18.

If one wanted to measure the physical capital represented by computers one could, at first glance, simply count up the number of computers. However, computers exist in many different sizes--there are minicomputers, microprocessors, super-computers, etc. Surely these different sizes represent different amounts of capital; they should be weighted differently in arriving at our total. One basis for weighting them is in terms of the output they are capable of. We could define a fundamental operation, say, an addition, and then measure the number of such fundamental operations the machine is capable of per second and use that as a measure of the machine's capacity. The problem with this approach, especially if used inter-temporally, is that it makes the constancy of the capital-output ratio true by definition. Since the amount of capital is <u>defined</u> in terms of the capacity to produce output, the capital-output ratio is definitionally constant and, so long as the interest rate is approximately constant, we get Harrod-neutral technological change by definition.

An alternative approach which is useful for analyzing developments in the computer industry, at least, is to define a quasi-atomic fundamental unit of physical capital. All computers are composed of fundamental electronic components--"gates" and "flip-flops." One can get impression of the amount of physical capital embodied in a machine by simply counting the number of components built into it. If we insist on measuring capital in physical terms, at least, this is probably the best approach.

A second ambiguity about the concept of capital involves the cost of capital. There are basically two ways in which the cost of capital is measured. The simpler of the two involves taking the interest rate as the cost of capital. This is particularly appropriate when we are speaking of capital in the sense of a fund of money. The other approach involves taking the cost of capital to be the rental rate (usually imputed) of capital equipment. This rental rate includes not only the interest rate but also an amortization charge for repaying the principal. This amortization charge in effect represents the depreciation on the equipment (if there were no depreciation, then the scrap value of the equipment would be the same as its original price, so the cost of renting a piece of equipment for a term of years would be simply the interest on the money tied up in the equipment). This rental rate on capital equipment is clearly the relevant "cost" of capital for the businessman trying to decide whether to substitute capital equipment for labor in the production process. The problem with this concept of capital is partly that there are greater difficulties in measuring it, since it depends upon the expected lifetime of the equipment--a highly uncertain datum. The greater difficulty, however, is that if we use the rental rate on capital as the cost of capital, then the <u>cost</u> of capital no longer corresponds to the <u>return</u> on capital, since the latter inevitably refers to capital in the sense of a fund of money. Since most of these theories are motivated by a desire to analyze the determinants of the income shares of capital and labor, this is a very unsatisfactory situation, since the determination that a certain amount of capital will be used and that its cost will be a certain amount no longer tells us what the incomes of capitalists are. Some of the costs of "capital" will in fact be used to pay rents and wages, and the extent to which this is the case depends upon the capital-intensity of the capital goods industry. All of these problems could presumably be solved in a suitable two-sector model, but the fact that they are ignored in the models like Kennedy's rather vitiates their results. We are not here, however, concerned with reconstituting economic growth theory on a sound footing, so we shall leave these problems to others. We shall in what follows treat the cost of capital as being the rental cost of the equipment.

The Pattern of Factor-Saving in the Computer Industry

Our discussion of the pattern of factor-saving must necessarily be impressionistic, since no data exist on the use of capital in the sense defined here. Moreover, the changes in capital-labor ratios, in the sense defined here, have been very large--several orders of magnitude. An adequate sense of technological developments can therefore be achieved simply by discussing the gross changes in factor usage.

If we adopt the measure of capital discussed above in terms of fundamental components, and if we measure labor in terms of number of workers (normalized to a standard work-week), then the initial effect of the introduction of the computer was clearly labor-saving. Components of pre-computer calculating machines are not really comparable with those in computers, but using cost as a rough index of quantity of physical capital (which is not a bad proxy for comparisons within a single time period), the capital-labor ratio rose from perhaps $250 per worker (using desk calculators--about $1500 per worker using tabulators) to about $21,000 per worker with the ENIAC.[13] The introduction of the

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[13] See data in Chapter Three, pp. 91-95.

computer, then, was clearly labor-saving, at least in Hicks' sense of the term (Harrod's definition, since it refers to changes in the aggregate production function, is not really appropriate here). While the computer was relatively labor-saving, it may also have been absolutely capital-saving. It worked so fast that the amount of capital used per computation was very little more than was used with desk calculators. The exact estimate of its capital-savingness depends critically upon the number of hours per week the machine was actually producing output. The estimate we used in Chapter Three (p. 93) would make the ENIAC absolutely capital-using, but somewhat more liberal estimates of its productivity would make it capital-saving.

While the initial introduction of the computer was labor-saving, the next stage in its development was capital-saving. If one compares the ENIAC with the computers of the early 1950's, one prominent difference is that the latter use far fewer tubes than the former to do the same or more work. Where the ENIAC had 18,000 tubes, most of the machines of the early 1950's do more work but use not more than 4000-5000 tubes. Labor requirements varied widely with the applications of the machines, but were roughly the same. The ENIAC was a crude, brute force design; later machines used more sophisticated designs to economize on hardware.

The next major development was again labor-saving: the introduction of higher level programming languages like FORTRAN, ALGOL, and COBOL. These languages essentially allowed programming man-hours to be stored up and used again and again at no incremental cost. They in effect created a new form of capital which was highly productive and reduced labor requirements. Programming languages resulted in the substitution of capital for labor and were thus labor-saving, though the capital so substituted is not manifested in the form of "hardware." The development of programming languages illustrates the difficulty of devising any sort of physical measure of capital. Our use of components as a measure of capital, useful though it is for some purposes, takes no account of the increase in capital in the form of compilers and packaged programs. The use of such programs does have the subsidiary effect of increasing the amount of memory required in the machine, and this increasing the "hardware" capital in use, but this does not measure the full increase in capital.

The fourth major change in computational technology, speaking in very broad terms, was the reduction in the cost of components due to transistorization, development of new memory components, and increases in speed of all components. These innovations play a somewhat anomalous role since they fit rather awkwardly with our usual concepts of factor-saving technological change. They are all, in effect, innovations which reduce the cost of capital equipment, speaking of capital equipment still in the sense of so many components. Such reductions in the cost of capital equipment are, as Salter (who is one of the few to analyze them in detail) emphasized, labor-saving. By reducing the cost of capital goods, they induce the substitution of these inexpensive capital goods for the now relatively more expensive labor, so that labor is saved. The peculiar aspect of this is that, while the <u>effect</u> of the innovation is to save labor, that is not the intent of the innovators. The intent of the innovators is to reduce capital costs, but that has the effect, via factor substitution, of reducing labor costs and <u>increasing</u> capital measured in physical terms. Depending upon the elasticity of substitution, actual capital costs may either rise, fall, or remain unchanged. The reason why this seems peculiar is that, again, we have two concepts of capital--capital as fund of money and capital as equipment. We have a situation here where the innovator seeks to economize on capital (in the sense of a fund of money), yet the result is that he increases his use of capital (this is definitely true of capital measured in physical units, and is true of capital as a fund of money--at least that portion of it used to rent capital as equipment--if the elasticity of substitution is greater than one). The concept of capital as equipment is again a troublesome one because the old concept of capital as a fund of money always seems to intrude itself, and in this case the two concepts give opposing signals about whether capital or labor is being saved.

One need not introduce capital-as-fund-of-money to find perplexities in the characterization of this innovation as labor-saving. If one used a different measure of physical capital, one could also find this innovation to be capital-saving rather than labor-saving. It seems faintly bizarre to consider an engineer working with a modern mini-computer with 16,000 words of memory packed into a shoebox, and to compare him with a team of operators running an I.B.M. 701 back in the 1950's with 4000 words of memory housed in great cabinets costing fifty times what the mini-computer costs, and to conclude that the capital-labor ratio has gone up! Modern computers, while much larger in terms of the number of components used and in terms of their capacity for output, are much smaller in terms of volume, weight, power requirements, and cost. If we used either volume or weight as our index of the amount of physical capital embodied in a computer (neither of them being much more arbitrary than using the number of components), then we would conclude that the innovations in components had saved capital rather than labor.

In any case, the elasticity of substitution, defined in terms of capital-measured-in-components, has been approximately unitary, or perhaps a little less. The cost of components has fallen to about one one-thousandth its value in the late 1940's, while labor costs have perhaps quintupled. The wage-rent ratio has therefore increased by about five thousand times, and in consequence the capital-labor ratio, measured in components per worker, has increased about the same amount, perhaps somewhat less. Here again we must emphasize that there are no hard statistics on these quantities, so we rely here on general impressions of "typical" installations. Thus the relative shares of capital and labor costs have remained remarkably steady at about 50% each.[14]

At the same time the speed of components has increased by perhaps one hundred times. However, this increase in speed has been partly vitiated by the increase in programming complexity brought on by programming languages, packaged programs, operating systems, etc. To execute a given computation now requires less programmer time but more "housekeeping" and translation steps on the part of the machine. If we take this reduction in capital efficiency as being perhaps a factor of ten, then the rise in output per worker has been perhaps 50,000 times, though this figure is highly approximate and would vary greatly with the nature of the application and the extent to which packaged programs are

[14] For some recent statistics, see Richard A. McLaughlin, "1976 DP Budget," Datamation, v. 22, no. 2 (February, 1976), pp. 54-57.

used (probably more installations now run the same program repeatedly than was the case in the early days of the industry--this would increase apparent labor productivity because the nature of the product had changed, not because of any change in the technique of production). The capital-output ratio has thus fallen by about ten times. The cost of output has fallen perhaps ten thousand times in line with the reduction in capital costs and the increase in capital productivity, which together about equalled the rise in labor costs and the increase in labor productivity. Thus, technological change since 1950 has been approximately neutral in cost terms, but strongly labor-saving in physical terms.

Evaluating the Hypotheses of Factor-Saving Bias

We are now in a position to consider how suitable the hypotheses discussed above are to analyzing the factor-saving bias of innovation in the computer industry. Considering first Hicks' proposition that factor-saving bias is induced by the gradual shifting of the wage-rent ratio, it is difficult to believe that such an abrupt jump in the capital-labor ratio could be induced by a gradual shifting of the wage-rent ratio. A gradual shifting of the wage-rent ratio suggests a gradual shift in the capital-labor ratio, not the abrupt shift which took place. In any case, there is no evidence that the motivation for the development of the computer involved the gradual rise in wage rates. This does not seem to have constituted a problem that any of the computer builders were trying to solve. The precipitating change was much more the change in technical possibilities rather than the change in factor prices. The change in technical possibilities was so powerful as to be profitable at a wide range of factor prices. Hicks does, of course, envision that not all invention will be of this induced nature. Some "autonomous" invention will also take place due to what he called technical opportunity, not the desire to save on any particular factor. It seems most reasonable to classify the computer, insofar as Hicks' analysis is concerned, with this "autonomous" group.

Salter's analysis is, for our purposes, essentially similar to Hicks'. Salter envisions that the fund of knowledge advances in a not prodictably biased way, but that the point at which we are located within the fund of knowledge at any given time is determined by factor prices. Insofar as technical change in Salter's model is biased in one direction or the other, the bias is due to this gradual increase in the wage-rent ratio, just as with Hicks. Thus Salter's analysis, any more than Hicks', cannot be used to explain the factor-saving direction of computational innovation. It is exogenous to his model.

Fellner's first proposition concerning induced innovation, that labor-saving innovation is induced by the expectation that labor costs will rise in the future, seems almost as inappropriate as Hicks' induced factor-saving mechanism. Fellner's proposition assumes that the labor-saving innovation is not optimal at today's factor prices, but that it is expected to be optimal at tomorrow's factor prices. Therefore the innovator carries out an innovation which is not defensible in terms of current factor prices because he recognizes that inventive effort is of the nature of investment, and its appropriateness can only be assessed in terms of the factor prices ruling over the expected lifetime of the invention.

In the case of the computer, however, we have an invention which was already the minimum cost technology, even at current factor prices. It was not necessary to take future prices into account, except to assume that current price trends would not be wildly reversed. The new technology had become opitmal because of a change in the technology, not primarily because of a change in factor prices. Fellner assumes that inventors are working right at the edge of economic viability, developing technology which has only just become appropriate due to changing factor prices. In the case of the computer, inventors were well behind the frontier of viable capital intensity.

Even for other cases where invention is right up to the factor intensity frontier, Fellner's first proposition seems unlikely to have much effect. Most inventors invent with a view toward a short-term pay-off to their inventive efforts (the fact that the pay-off is often quite long in coming is only testimony to the perennial optimism of inventors). If inventors typically invent with the intent of recouping their investment in five years, then the potential rise in the wage-rent ratio over this period is so modest as to produce only slightly more capital-intensive invention than would occur if inventors designed with respect solely to current factor prices. Moreover, while the long-term rise in the wage-rent ratio is a safe bet, the short-term rise is much more uncertain. The upward phase of the business cycle can cause the wage-rent ratio to fall (since capital costs are more volatile than labor costs), so that an inventor seeking a short-term pay-off to invention would be taking a significant risk that the wage-rent ratio would fall rather than rise during the expected payoff period of the invention.

Fellner's first proposition, then, seems an unlikely mechanism for induced factor-saving, either in the case of the computer or in general.

Fellner's second proposition, on the other hand, seems to have much broader applicability. His second proposition is that the perceived inelasticity of supply of one factor would make an inventor expecting to expand the scale of his operation expect a rise in the cost of the factor, and economize on its use in his invention. Whatever the real inelasticity of the supply of labor, the perceived inelasticity seems to be substantial. In Habakkuk's discussion of American technological change in the nineteenth century, he emphasizes inelasticity in the local supply of labor as a major factor inducing labor-saving technological change. Rosenberg makes a similar point.[15] Entrepreneurs notice the difficulty in attracting labor at the going wage rate and, correctly or not, estimate that a large wage increase will be necessary to attract the required labor force for the current technology. The high estimate may well be correct in the short run, particularly if the present technology requires special skills. In any case, inelasticity in the supply of labor was a major inducing factor in the development of the computer. The Ballistics Research Labs felt that they had exhausted not only the local labor supply, but also the East Coast labor supply. The inelasticity in the labor supply was obviously in large part due to the War, but, in view of the size of

[15] Nathan Rosenberg, "The Direction of Technological Change: Inducement Mechanisms and Focussing Divices," Economic Development and Cultural Change, v. 18, no. 1, pt. 1 (October, 1969).

the computing burden facing the BRL, they probably would have faced significant inelasticity in labor supply even in the absence of special wartime conditions.

Thus one important characteristic of Fellner's second proposition is that it is more likely to obtain the greater the increase in output which is contemplated by the potential innovator. In this sense, the factor-saving bias is in part the result of an increase in scale. In fact, one can generalize that, in advanced industrialized countries, the supply of capital is more elastic than the supply of labor. An increase in scale is thus likely to generate pressures to economize on labor. The more rapidly growing the economy, the greater the labor-saving bias. This is not really very different from the conventional analysis, except that it suggests that rising capital-labor ratios (which will rise faster the faster the rate of economic growth) will manifest themselves more in a growing relative inelasticity of labor supply than in a rising wage-rent ratio, at least initially. The wage-rent ratio will rise eventually, but the initial effect of increasing the inelasticity of the labor supply may have a more powerful effect on labor-saving innovation than the eventual rise in relative wage rates.

The usefulness of Kennedy's proposition about the innovation possibility frontier largely rests, as we have seen, on the intertemporal stability of that frontier. We can therefore consider whether this frontier, as it applies to computational technology, really has been stable.

As we have seen, the initial development of the computer was strongly labor-saving. In this respect it continued the pattern that had previously existed; i.e., previous computational developments, like the desk calculator and the tabulator, had also been labor-saving. That the share of capital was continuing to rise in computational work might be taken either, I suppose, as evidence that Kennedy's theory is wrong because it shows that the innovation possibility frontier is unstable, or as evidence that Kennedy's theory is right, but it takes time to reach equilibrium. In any case, it is clear that the innovation possibility frontier had shifted since the nineteenth century. Computational technology in the early nineteenth century was surely in "equilibrium"; if the equilibrium capital-labor cost shares had shifted, then the curves must have shifted.

Subsequent technological change was, as we have seen, at first capital-saving and then neutral (in cost terms).[16] One might argue, from Kennedy's point of view, that the initial development of the computer "overshot the mark", making capital costs too high, thus inducing an effort to reduce capital costs back to an equilibrium level. But to describe the resulting share of capital costs as an equilibrium share because ensuing technological change was neutral is to ignore what was really happening beneath the bland label of "neutral technical change." While capital costs remained constant during this period as a proportion

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[16]While Kennedy's analysis is formally in terms of capital measured in physical units, it only makes sense if capital is measured in terms of its cost to the entrepreneur. He is presumed to seek to economize on the use of capital as the amount in use becomes large, and this would make sense if the "amount" of capital were measured in value, rather than in physical terms. If he were using more physical capital because the cost of physical capital were plummeting, then there would be no reason for him to wish to economize on its use.

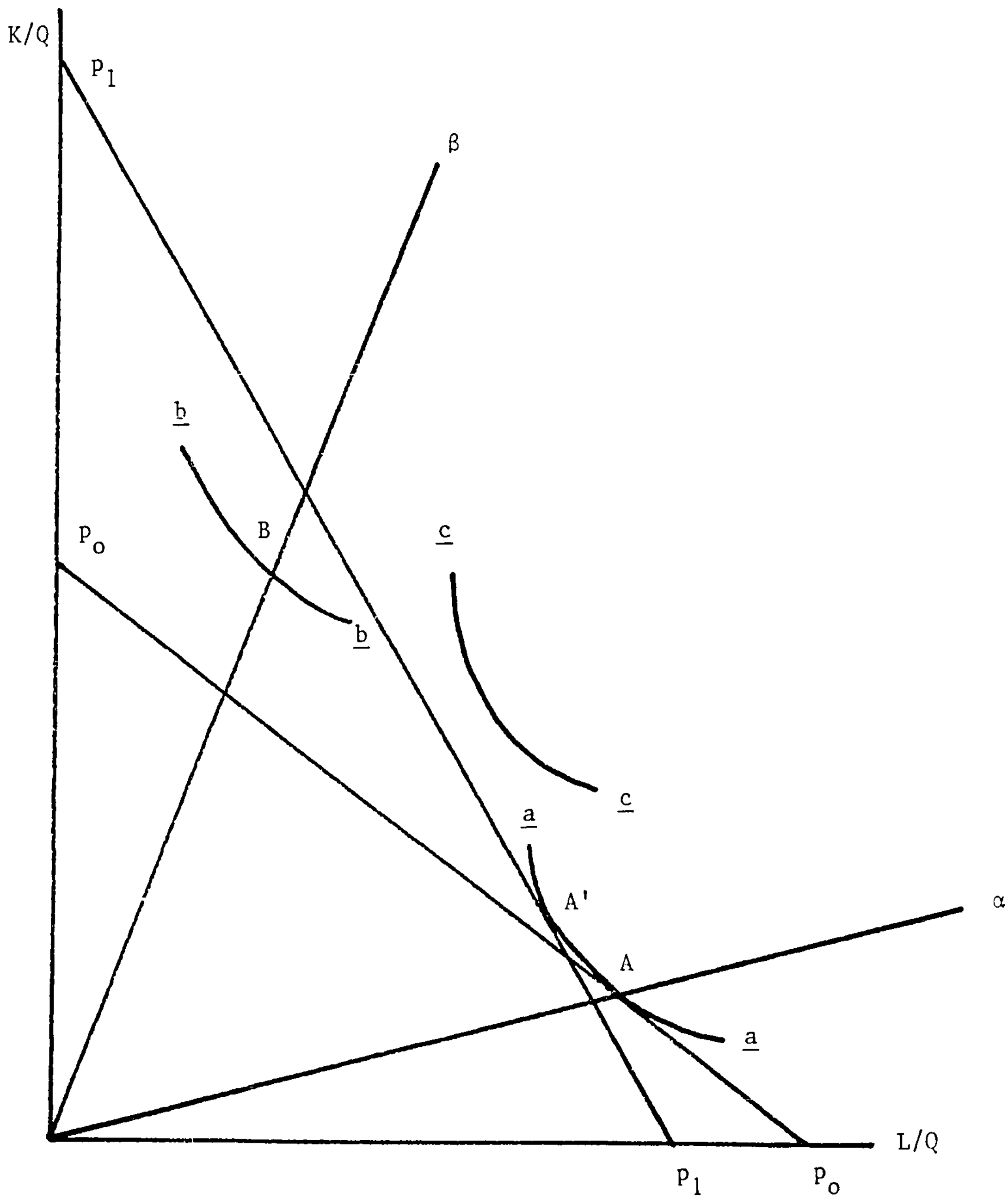
of total costs, this occurred despite a constant effort on the part of innovators to reduce capital costs. Their efforts were technically successful, in that costs per unit of physical capital (as measured here) fell dramatically. Since, however, the elasticity of substitution was approximately unitary, the fall in capital costs (per unit of capital) led to so much substitution of capital for labor that the total share of capital costs remained constant. If the share of capital costs was regarded as "too high," then the effort to reduce it, while technically successful, was an economic failure, so no equilibrium was ever reached. It is for this reason that Kennedy's theory must assume inelastic factor substitution--only then does a technically successful effort to reduce costs of one factor actually succeed in reducing that factor's share of costs. If elasticity of substitution is greater than one, then the effort to reduce one factor's costs backfires--that factor's share in total costs actually rises. Kennedy's analysis thus seems a poor model, in general, for computational innovation.

Paul David's analysis is useful, I think in understanding technological developments in computing, though its essentially neoclassical flavor somewhat overstates the role of rational calculation in computational innovation. First of all, David's analysis is suggestive of an assumption which he does not make explicitly but which seems implicit in much of his presentation, and which seems appropriate in the case of the computer. We ordinarily assume that the range of the technologies, in terms of the capital-labor ratios embodied in them, is continuous--that we can find a unique technology to correspond to any capital-labor ratio that we choose. Thus, as wage-rent ratios rise, there is a gradual, continuous shift in the capital-labor ratio in response. David also makes this assumption, but assumes that most of these technologies are only theoretically possible--they are not yet reduced to practice. At any given time, only a few are reduced to practice, so that the isoquant reflecting available ("off-the-shelf") technologies has the segmented, non-continuously differentiable look of a linear programming isoquant.

It seems odd to assume that technologies with "distant" capital-labor ratios which, given a secularly changing wage-rent ratio, have never been economically optimal, would nevertheless have been reduced to practice. Any distant technology would exist, if at all, only as a possibility within Salter's "fold of Knowledge," not as on off-the-shelf possibility. However, David's assumption is suggestive because it turns out that the set of potential technical possibilities in fact resembles David's assumption about the set of presently available technical possibilities. It is in fact not generally the case that a unique technology exists at any given capital-labor ratio. There may be (at least at any given time) only three or four possible techniques which are optimal at some wage-rent ratio. There may be other techniques using intermediate capital-labor ratios, but they may be inferior to some other technique regardless of the wage-rent ratio. If we stick with unique technologies, the technology isoquant is not convex throughout. It becomes convex only if one deletes the intermediate technologies which are always inferior and fills in the gaps with line segments which represent mixtures of separate technologies. Thus, the true shape of the "fund of knowledge" curve is not the usual continuously twice differentiable one, but rather a segmented linear programming type isoquant.

The significance of there being only a few basic technologies available (even potentially) is that the response to gradually shifting wage-rent ratios will not be a gradual shifting of technologies, but rather a series of abrupt jumps from one technology to another. If we start (in Figure 2) with technology aa and factor prices $p_o p_o$, producing at point A, the initial result of a change in factor prices to $p_1 p_1$ is to make marginal adjustments to technology aa, changing slightly to production at A'. Despite the fact that technology bb exists as a technical possibility which would be superior to aa at the new factor prices, the feasibility of bb is highly uncertain. Its uncertainty is largely due to the fact that moving to it requires such a large jump in the capital-labor ratio (which ordinarily involves a substantial change in the underlying technology used--one point commonly ignored by economists is that, while technological change must often be embodied in new capital, capital intensity is conversely often possible only as the result of technological change). The firm producing A' is standing at the edge of a cliff looking out into the fog. It is not sure if there is another technology waiting out there to be discovered. It may be able to see, dimly, technologies like cc, which appears to be uneconomic at current factor prices. It may investigate these technologies anyway, hoping that it can improve them anough to make them economical (Aiken's development of the Mark I falls into this category, though I'm sure he considered it economical to begin with). But, in any case, the fact that technologies exist as discrete possibilities and not continuous possibilities means that firms cannot slide gradually and relatively risklessly up the production isoquant as factor prices change. They must move in jumps (a

FIGURE 2



firm may achieve an intermediate average capital-labor ratio by using bb in combination with aa, but it must first make the technical jump to bb in order to do this--its overall capital-labor ratio can change gradually, but its technology must change abruptly).

This is in fact what happened in the case of the computer. The wage-rent ratio had changed enough by the mid-twenties to make computers feasible, but the size of the technological jump involved was too great for firms which preferred to feel their way in the dark along the aa curve on which they were currently operating. The effect of the War was suddenly to twist the effective wage-rent ratio much further around than it was either before or after the War. Capital was practically free to approved projects--no formal rate-of-return criterion was applied to war projects--and labor was inordinately expensive in the sense that expansions in the labor force became practically impossible. This abrupt shift in the effective wage-rent ratio induced a willingness to investigate an area of the technological terrain previously considered too chancy. No technologies between A and B were found which were enough superior to A to bother with. It was necessary to shift abruptly to B (the ENIAC) to effect any significant improvement at all. (The discussion here implies a much more neoclassical kind of behavior than I think really occurred, but I am interpreting the developments in neo-classical terms so as to fit them into the neoclassical construct of the production isoquant.)

Having once arrived at B, however, the second part of David's analysis becomes relevant. Progress occurs at different rates along different technology rays. The learning curve (Schmookler notwithstanding)[17] for mechanical calculating equipment was largely played out. The possibilities for advance using an electronic technology were great. Progress occurred rapidly down the beta ray, so that even at the more usual factor price ratios which prevailed after the War, the new electronic technology had become optimal for many more applications than had originally been anticipated. Eventually the technology advanced so far that it dominated the old mechanical technology even for the sort of small scale desk calculators where the mechanical technology had its greatest relative advantage. Essentially the same technology is now replacing everything from wristwatches to carburetors, thus demonstrating the much greater potential along the ray than along other technological rays (this analysis is rather awkward as long as we persist in describing technologies in terms of capital-labor ratios, a characteristic of them which is quite unimportant for many purposes--a more suitable basis for description would be in terms of Lancastrian performance attributes of the technology, but that would lead us too far afield of our present purpose).[18]

David assumed that, in the absence of further changes in the wage-rent ratio, further technological advances would be "locally neutral." As stated above, computational advances continued to be strongly labor-saving, at least in terms of physical quantities of capital as measured

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[17]Jacob Schmookler, Invention and Economic Growth (Cambridge: Harvard University Press, 1966), pp. 87-94.

[18]See Kelvin Lancaster, Consumer Demand: A New Approach (New York: Columbia University Press, 1971).

above. In cost terms, technical advance was approximately neutral, but this aspect of David's theory is certainly not critical. One might criticize the appropriateness of David's theory on the same grounds as we criticized Hicks and Salter. The development of the computer was not fundamentally induced by any change in factor prices--by the early 1930's, the potential computer technology had advanced to the point that it was optimal over a wide range of factor prices. The development of the computer was fundamentally an autonomous invention. The developments in vacuum tubes took place for reasons having nothing to do with factor prices, and these propelled the potential technology into a position where it was optimal even in the absence of any factor-price changes.

Thus, in a fundamental sense, factor-price based inducement mechanisms did not cause the development of the computer. Nevertheless, the radical nature of the computer would have delayed its introduction for quite some time in the absence of factor-price inducement mechanisms which focused attention in a labor-saving direction. Thus these theories are not useful for showing the underlying causes of computational advance, but they do illuminate some significant aspects of these developments. Fellner's second proposition, concerning inelastic factor supplies, is useful for delineating an important focusing device, and David's analysis is useful for showing the significance that linearities in the production isoquant can have in producing abrupt jumps in technology, and for showing the significance of differential rates of advance along different capital-labor ratio rays. But it is probably safe to say that radical innovations are rarely induced by incremental changes in the wage-rent ratio. Incremental changes in factor prices generally induce incremental changes in technology. Radical innovations flow from other sources.

CHAPTER FIVE

THE DIFFUSION OF COMPUTATIONAL INNOVATION

One point which has justifiably been emphasized in the literature on technological change has been that the factors which determine the occurrence of an innovation are of only modest significance if that innovation does not diffuse into widespread use. The factors which determine the diffusion of innovations are thus of comparable significance with those which determine the timing and direction of the innovation itself.

In the case of the computer, the word "diffusion" may refer to two different phenomena. First, there is the diffusion of particular computational technologies amoung computer manufacturers--the spread of the use of core memories, for example, or of multi-processing. Second, there is the spread in the use of computers by computer users. In the earliest stages of computer development, this distinction is muted, because the manufacturer was in many cases also the user (this also mutes the distinction between invention and innovation--if the inventor invents for his own use, then the acts of inventing and innovating are one).

The Mechanisms of Diffusion

One of the basic propositions of the behaviorist theory which we developed in Chapter Three was that a person's perceptions of the environment, particularly, for our purposes, of the technical feasibility and economic viability of an innovation, are distorted by the nature of the information which he receives. The information which he receives is always partial, and is always interpreted in the light of his own experience, which is only a small part of the total relevant experience which he would need to make an "optimal" decision about the innovation. Thus an important factor in understanding the nature of the decisions which the potential innovator makes is understanding the nature not only of his own experience--what we stressed in Chapter Three--but also the nature of the information which he receives. A behaviorist analysis of the innovative process would assume that the nature of the channels through which he receives information will color that information. Thus the institutions by which information is disseminated play a critical role in determining what information is disseminated, how the information is interpreted (people discount the value of information, depending upon its source), and therefore what actions people take on the basis of that information. We shall therefore start with a discussion of what channels for the dissemination of information, what mechanisms of diffusion, existed and developed in the early years of the computer industry.

One of the distinctive features of a radical innovation like the computer, as compared with, say, the agricultural innovations studied by Grilliches,[1] is that the radical innovation creates its own

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[1] Zvi Grilliches, "Hybrid Corn: An Exploration in the Economics of Technological Change," Econometrica, v. 25, no. 4 (October, 1957), pp. 501-522.

industry, and therefore the institutions of that industry are not in existence initially to aid in the process of diffusion. The seed companies, Agricultural Extension Service, and farmers' organizations which acted as institutions for diffusion of hybrid corn did not have corresponding institutions in the early days of the computing industry. There were, of course, business firms involved with computing, but they were largely concerned with other products and only later played a prominent role in encouraging the diffusion of computational technology. Professional organizations existed, but they were organized around older technologies and were largely concerned with other matters. The professional organization for electronic engineers was still called the Institute of Radio Engineers, and while they gradually became active in spreading computational technology, their role at the beginning was quite modest. A new industry like the computer industry must create its own institutions for the diffusion of technology.

In the case of the computer, these institutions took a variety of forms. One important form was the conference. We have already mentioned the meetings of the American Mathematical Society in 1940 at which Stibitz exhibited his machine (supra, p. 37) and of the American Association for the Advancement of Science in Philadelphia in the same year at which Mauchly met Atanasoff and found out about his machine (supra, p. 45). Several other important conferences were held in the late 1940's which were concerned primarily with computers, and which helped in spreading information about computational techniques. The first of these was held at MIT in October, 1945, sponsored by the Mathematical Tables and Other Aids to Computation Committee of the

National Research Council.[2] This was primarily designed to celebrate MIT's electronic differential analyzer, which had been completed several years earlier but had been kept secret during the War. This conference had a limited impact, since few machines had been completed, and their developers were hesitant to discuss them until they had been finished and officially announced. The list of participants was intentionally kept narrow and the proceedings were not published. The Moore School work was discussed along with the Bell Labs and Harvard work.

A more important gathering took place at the Moore School of Electrical Engineering in the Summer of 1946. This was actually not a conference but a series of lectures given by people connected with the ENIAC and EDVAC projects. These lectures had an important impact, since they made the details of the construction of the ENIAC available to a wide variety of people. By demonstrating the feasibility of the technology and its suitability to solving a wide variety of problems, the lectures greatly encouraged people who were thinking about improving the computational technology in their own organizations. The lectures were published in mimeographed form in four volumes in 1947 and 1948 by the University of Pennsylvania.[3]

A third important conference was held at Harvard in January of 1947. This conference was jointly sponsored by Harvard and the Navy

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[2]Herman H. Goldstine, The Computer from Pascal to von Neumann (Princeton: Princeton University Press, 1972), p. 219.

[3]University of Pennsylvania, Moore School of Electrical Engineering, Theory and Techniques for Design of Electronic Digital Computers: Lectures Given at the Moore School, 8 July 1946 - 31 August 1946, 4 volumes (Philadelphia: University of Pennsylvania, 1947, 1948).

Department Bureau of Ordnance, which had supported the work at Harvard on the Mark I since 1944, as well as the development of the Mark II. The Harvard meeting was a conference _per se_, with talks given by people working on various computer projects, including not only the ENIAC and Mark I projects but also people working with I.B.M. tabulators and analogue machines. It was a useful conference partly because it offered some comparison of the capabilities of various kinds of technologies for those who had not yet committed themselves to any one. The conference also allowed participants to see and play with the Mark I and to tour the newly completed Harvard computing center. The Harvard Conference also published the proceedings of the conference, making the discussion available to a wider audience.[4]

A second conference was held at Harvard in September, 1949, also co-sponsored by the Navy Bureau of Ordnance. It had a similarly comprehensive selection of speakers representing a wide variety of technologies. The basic outline of computational technology had not yet settled down, and a wide variety of approaches was still being considered by partisans of various designs. This meeting again allowed a better opportunity for a comparison of these various designs than would have been possible otherwise.[5]

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[4] _Proceedings of a Symposium on Large-Scale Digital Calculating Machinery_, jointly sponsored by the Navy Department Bureau of Ordnance and Harvard University, at the Computation Laboratory, 7-10 January 1947 (Cambridge: Harvard University Press, 1948), vol. 16 in the Annals of the Computation Laboratory of Harvard University.

[5] _Proceedings of a Second Symposium on Large-Scale Digital Calculating Machinery_, jointly sponsored by the Navy Department Bureau of Ordnance and Harvard University Laboratory, 13-16 September 1949 (Cambridge: Harvard University Press, 1951), vol. 26 of the Annals of the Computation Laboratory of Harvard University.

In the early 1950's, the dissemination of information about computing became much better organized and institutionalized. Beginning in December, 1951, a series of Joint Computer Conferences was held by the American Institute of Electrical Engineers and the Institute of Radio Engineers. The proceedings of these conferences were published and did much to spread information about new machines and technologies.

The AIEE and the IRE had already been playing a significant role by publishing a number of articles in their respective proceedings about computers (the Transactions of the AIEE and the Proceedings of the IRE). Articles also appeared in a number of other periodicals in the field, such as Electrical Engineering and Electronic Industries. One important early contribution to the literature was the journal Mathematical Tables and Other Aids to Computation. This was founded in 1943 by Raymond C. Archibald of Brown University as a project of the National Research Council. In its first couple of years it devoted itself largely to publishing errors in existing mathematical tables, but after the end of the War it was often the first source of published information about new computing machines.

In the 1950's a number of new periodicals were started. The Association of Computing Machinery was founded and began publishing its Journal in 1954. It subsequently added other periodicals. Several private consultants such as Edmund Berkeley and John Diebold, as well as regular publishing firms, began publishing newsletters and magazines such as Computers and Automation, Computerworld, and Datamation. By the late 1950's, the industry had emerged as a full-fledged industry, complete with its own professional associations and trade press, so that subsequent technical developments were publicized through ordinary channels of communications.

Another important source of information was technical reports sent directly to users. The most important source of these reports was the Institute for Advance Study. The work which von Neumann and Goldstine did on the IAS machine was publicized extensively to anyone who asked to be put on the mailing list, including private firms as well as government agencies. The long list of IAS inspired machines has already been discussed in Chapter Two (p. 63). This series of reports really began with von Neumann's original report on the EDVAC in 1945[6] and with the subsequent report by Burks, Goldstine, and von Neumann in 1946.[7] These reports were never formally published until much later, but circulated extensively in samizdat, as it were, and there is general agreement that they were very influential, even if there is dispute about the originality of the ideas there expressed.

Perhaps the most interesting source of information, however, was simply the movement of people from one firm or organization to another during this period. Computer designers were generally very mobile during this period. Believing strongly in the rightness of their ideas and in the glowing future of computers, they often grew dissatisfied

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[6] John von Neumann, "First Draft of a Report on the EDVAC," Contract No. W-670-ORD-4926 (Philadelphia: Moore School of Electrical Engineering, University of Pennsylvania, June 30, 1945). Reprinted in Brian Randell (ed.), The Origins of Digital Computers: Selected Papers (New York: Springer-Verlag, 1975), pp. 355-364.

[7] Arthur W. Burks, Herman H. Goldstine, and John von Neumann, "Preliminary Discussion of the Logical Design of an Electronic Computing Instrument" (Princeton: Institute for Advanced Study, June 28, 1946). Reprinted in Randell, op. cit., pp. 371-386.

with the conservatism of the organizations for which they worked, and the unwillingness of those organizations to support their ideas. As a result, they often quit to start their own firms or to work for some other organization more sympathetic to their ideas. Because the market was growing, particularly during the 1950's, there was a sellers' market in computer personnel. A firm seeking to start up a computer program felt completely lost in the new technology and felt the need to import experienced people from the outside. Those experienced people could therefore often change jobs at substantial increases in pay. Thus both financial and professional motivations combined to encourage a high rate of mobility. The marginal product of an experienced person always seemed higher to a firm just starting out in computers than to a more experienced firm.

Forces Affecting Diffusion of Technology Among Manufacturers

Most analyses of technical diffusion by economists naturally emphasize the role of economic forces in influencing diffusion. Thus Grilliches, in his study of hybrid corn,[8] emphasized the relative profitability of hybrid corn in various parts of the country as a determinant of the rate of diffusion of the use of such corn to those areas. Similarly Mansfield found in his studies of innovation in the coal, steel, brewing, and railroad industries that the potential profitability of an innovation was a major determinant (along with the size of the

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[8]Zvi Grilliches, *op. cit.*

investment required) of the rate of diffusion.[9] There can be little doubt that profitability is a major factor affecting the rate at which most innovations are adopted by imitating firms. I shall argue, however, that, for a variety of reasons, the factors affecting the diffusion of a radical innovation like the computer are likely to be more non-market, more behaviorist in nature.

We have already argued that, because the computer was a radical innovation, there were no well-established media of communications through which information about technical developments could be communicated. One special aspect of this problem was that the computer required for its development various kinds of expertise. It required the technical abilities of electronic engineers and the logical abilities of mathematicians. While there were certainly periodicals in existence in which articles about computers could appear, they were periodicals which appealed only to subsets of the total group interested in computers (e.g., only engineers, not mathematicians, would be likely to read Electrical Engineering). Thus the periodicals which existed were not fully satisfactory as media for communication of information about computers.

Secondly, there was relatively little recourse even to those communications media which did exist. Virtually from the beginning, people were interested in patenting their inventions in computational technology, and therefore were only interested in publicizing their work after it had been completed and patent applications had been filed.

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[9] Edwin Mansfield, Industrial Research and Technological Innovation (New York: Norton, 1968), pp. 133-194.

By this time, because the pace of technological change was so fast, it was too late to be helpful to anyone.

> In the early days of the development of the computer, we didn't publish papers, you see. Everyone was frightened that disclosure would lose patent position. So if you go back to the early periods, ENIAC was published, but not widely. Mark I was published, but not widely. Some of the really interesting ideas of those machines were not surfaced. Von Neumann published, and his publishing was possibly some of the best, with the group at IAS.[10]

Because published information lacked detail and usually came too late to be helpful, computer manufacturers were forced to rely on their own abilities and on the information and ideas they picked up from personal contacts. While personal contact took place at conferences, this probably led to little more communication of up-to-date information than did the papers presented at the same conferences. People would be unlikely to communicate information that would sacrifice the competitive advantage of the firm for which they were working. The only full sharing of information would occur within the firm (and, to some extent, with consultants brought in from the outside). Thus the only communication of current information from firm to firm would occur when someone moved from one firm to another. Thus the last diffusion mechanism mentioned above, the movement of people from firm to firm, is likely to have been the only one in which the information communicated would really be current, useful information.

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[10] Isaac Auerbach, interviewed by Henry S. Tropp at Philadelphia, Pa., February 17, 1972, Smithsonian Institution, The National Museum of History and Technology, Computer History Project (Washington: Smithsonian Institution, 1972), p. 10. Other such interviews will hereafter be cited as Smithsonian (CHP).

Thus, the paucity of published information forced computer designers to rely largely on their own imagination and on the ideas brought to them by people hired away from other firms. The set of technical possibilities which designers were able to consider was thus restricted to the narrow set of those with which they or their new colleagues had had experience. The set of possibilities through which they might search was narrowed by the lack of published alternatives, and this sort of narrow search is, as we have emphasized in Chapter Three, typical of a behaviorist model of problem solving. But what we are arguing here is that this narrow search routine was forced upon the researchers by the limited media for diffusion of new ideas; we need not simply assume it on a priori grounds. I would argue that this is likely to occur generally in the case of radical innovation. Radical innovation implies the absence of established communications media and suggests the likelihood of rapid technological change (on the theory that change is likely to occur more rapidly the closer to the beginning of the learning curve we are for any particular technology). Both of these factors imply the importance of the movement of people as the vehicle for the transmission of ideas, and the consequently narrow range of ideas open to the imitator. He cannot make a universal survey of the relevant technology and choose the best one; he must choose the best one he knows about. His search routine is a constrained one. Informational constraints and imperfections enter as a characteristic feature of imitation in a radically new technology.

Isaac Auerbach has emphasized this aspect of the movement of ideas in computing, that ideas move through the movement of people, rather than, as in an established academic field, through formal publication of articles. This phenomenon was so marked that he claimed he could identify where a computer was designed just by looking at it, like Henry Higgins identifying the block where a flower seller was born by her accent:

> By 1964 I was able to identify with over eighty percent accuracy where every new computer was developed, and name the individuals who conceptualized the machine. That idea came from this group, and so-and-so moved from this company to that company, and he was the driving force behind that or that system or that design concept.[11]

One example which illustrates this phenomenon is the question of hardware versus software economy. Any computer designer faces a basic choice concerning how much of the arithmetic functions of the machine should be "hardwired" and how much should be programmed via software. Claude Shannon's article of 1938 had shown, using Boolean algebra, that all the arithmetic operations could be programmed as long as a couple of basic logical operations were wired into the circuitry of the machine.[12] In practice, some were wired in and others were handled with programmed subroutines. In most cases the three simplist operations--addition, subtraction, and multiplication --were hardwired. Division and square rooting were sometimes hardwired and sometimes programmed. The extent to which arithmetic operations were wired in was thus one well-defined characteristic of the machine.

Each method had its advantages and disadvantages. Wiring more

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[11] Ibid., p. 8.

[12] Claude E. Shannon, "Symbolic Analysis of Relay and Switching Circuits," Transactions of the American Institute of Electrical Engineers, v. 57 (1938), pp. 713-723.

operations into the machine increased the amount of hardwire required, of course, thus increasing initial cost and size and reducing reliabity (the more tubes there were, the more chances that one would blow out). On the other hand, hardwiring the arithmetic operations increased speed and reduced programming complexity. Generally speaking, the advantages of programming the arithmetic operations were probably greater in the early days of computers, when there was greater stress upon reducing the number of tubes, speed was not as critical, and users were more tolerant of programming complexity. As the cost of components dropped, especially after they were replaced by transistors, the value of hardware economy dropped. As Norm Kreuder put, "Silicon is cheap. It's all over the beach."[13] But in the early days, it was not clear which method was superior, and different firms were free (from the point of view of the market) to follow their own intuitions and prejudices.

The figure who was most identified with the idea of hardware economy was Floyd Steele, originally of Northrop. His influence was felt in a number of companies whose computer operations were staffed by Northrop alumni--Computer Research Corporation, National Cash Register, Bendix, Logistics Research, and J. B. Rea. All of the computers manufactured by these firms reflected to varying extents the emphasis on hardware economy stressed by Steele. Other firms on the West Coast, whose personnel did not come from Northrop, were more inclined to economize on programming (e.g., Consolidated Electrodynamics and Burroughs).

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[13]Quoted by Paul King, interviewed by Robina Mapstone, February 27, 1973, Smithsonian (CHP), p. 39.

One might carry the appreciative understanding of this theoretical proposition forward by suggesting a metaphor for various kinds of diffusion. What we are suggesting here is that the form of diffusion which was most important in the early computer industry was a form of convection--just as in convection heat is transferred from one place to another by the actual movement of the hot air from one place to another, so in the case of the computer industry, the movement of technical knowledge required the actual movement of people from one firm to another.

Analogously, we might define a diffusion process characterized by "conduction" as one in which people do not move from one firm to another, but in which person-to-person contact is critical for the spread of technical knowledge. This sort of diffusion was significant in computers. The conferences mentioned above involved this sort of person-to-person contact; there was also a great deal of visiting of computing centers by interested outsiders. This window-shopping typically involved potential users who were considering buying a machine. In many cases, however, such people eventually were unable to buy a machine in the time they needed one, so they built one themselves.[14]

The third sort of diffusion process, carrying out the metaphor, would be the analogue of radiation. This would be diffusion through impersonal media such as journal articles, technical reports, proceedings of conferences, etc. These media played some role in the early

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[14]See, for example, Alston Householder's account of his search for an appropriate machine for the Oak Ridge National Laboratory in his interview with Richard R. Mertz at Oak Ridge, Tenn., July 20, 1970, Smithsonian (CHP), pp. I/26-I/30 and II/1-II/16.

computer industry, but, as I have emphasized, reports were often out-of-date by the time they appeared, and the firms publishing reports of their work often did so to demonstrate their technical competence to potential users, not to share their secrets with potential competitors. Those who did publish useful reports were the non-profit researchers such as the Institute for Advanced Study, but these researchers by no means had a monopoly on the good ideas.

At the same time that rapid technical change forced innovators to rely on a narrow circle of sources of technical information, market forces gave little direction to technical innovation. This was partly because computer manufacturers were uncertain about the best characteristics to build into their machines, partly because users were equally uncertain, and partly because demand was so strong that awkwardly designed machines could often find a market anyway.

The organizers of most of the computer firms of the late 1940's and early 1950's were engineers who had some familiarity with certain applications in their engineering work, but who knew little about the broader range of computational applications, especially "data processing" (as opposed to scientific computation). Since the organizers of these firms, while very enthusiastic about the general usefulness of computers, had little sense of how they would actually be used by most of their customers, they were largely in the dark about what features would be most valuable to build into the machines. Many features were obviously desirable--more memory, faster calculating times, etc.--but it was not clear which of these desirable features were most valued by customers, and therefore should be added first. Donald Eckdahl, for example, recalls, "My viewpoint...is that the original people that designed computers (and I class myself as one of them) simply did not, in any sense, foresee what the user would do with the machine."[15] It was only later that others came along, more user-oriented, more software-oriented, to develop applications. Eckdahl had no idea that software would become as significant as it did.[16]

In the 1950's, the established business machine manufacturers began entering the market, and one would have expected them to have closer ties to users, and thus have a clearer idea of what machine characteristics would be most useful to customers. This was to some extent true--I.B.M.'s success was partly due to a sharp sense of customer needs--but sometimes the established manufacturers also failed to produce the best machine characteristics. If the failing of the organizers of the new firms was to sell overly sophisticated equipment that customers didn't know how to use and that was not clearly related to their established needs, the failure of the established firms was to fail to recognize the possibilities in the new technology and the fact that customers would respond to the lower cost of computing by changing the kinds of computing they would so.

Established firms also often failed to recognize that the much greater pace of technological change meant that they would have to change their style of doing business--e.g., new products would have to be

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[15]Danald Eckdahl, interviewed by Henry S. Tropp, September 25, 1972, Smithsonian (CHP), p. 42.

[16]Ibid.

brought to market sooner in order to avoid being obsolete before they were introduced.[17]

Finally, the customers themselves were often ill aware of their own needs. They knew what computations they wanted computers to do that were already being done, but the great significance of the computer was not simply that it reduced the cost of computations already being done, but that it made feasible a wide range of applications previously impossible. A great deal of innovation was required not only in developing new technology, but also in dreaming up new applications for that technology. Users were unsure of what additional work they might have the computer do because they did not know what the capabilities of the machine were, and, even when they did, it took time to reorganize their operations with the possibilities of the computer in mind.

All of the actors, then--new firms, old firms, and users--had only a part of the relevant information. None had enough information to forecast how computers would be used and thus to plan rationally what characteristics the machines should have.

The result of this was that each computer designer was forced to conceive his own idea of what constituted a good computer design, and there was, in the early days (and this really continued into the 'sixties at least, because new applications were constantly being developed, so it was never really clear what the characteristics of a "good" computer were), little rational basis for gainsaying him. There was thus

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[17] Jerry Mendelson, interviewed by Robina Mapstone at Sherman Oaks, Calif., September 6, 1972, Smithsonian (CHP), pp. 73-74.

a strongly evolutionary character to the early computer market--different entrepreneurs would have different ideas about what constituted a great idea in computing. Each would build a computer reflecting his beliefs, and the market would choose among these various designs.[18] The market was not, of course, an untutored arbiter. Firms which were major factors in the market, such as I.B.M. and Remington Rand, had much greater freedom in deciding what characteristics their machines would have than did newly created firms. To some extent they set (arbitrary) standards which smaller firms were forced to accept.

The harshness of the evolutionary environment was meliorated by the fact that demand was very strong. Since firms found it easy to sell all the machines they could produce, there was relatively little pressure on them to bend their designs to the demand of the market. These two factors--the difficulty in determining the best configuration for suiting user needs, and the market strength which weakened the selective function of the market--combined to strengthen the influence of non-market, "behaviorist" factors like those discussed above. Computer designers tended to design according to their intuitions, and there was little either in rational argument or market pressure that could determine their intuition to be wrong. The scope for behaviorist factors was wide, and therefore the diffusion of new technological ideas among manufacturers was determined more by where designers had worked before

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[18]The early computer industry is therefore well-modeled by the sort of "evolutionary" theory developed by Richard Nelson and Sidney G. Winter, Jr., in "Neoclassical and Evolutionary Theories of Economic Growth: Critique and Prospectus," E.J., v. 84, no. 336 (December, 1974), pp. 886-905, and (with Herbert L. Schuette) "Technical Change in an Evolutionary Model," Q.J.E., v. 9, no. 1 (February, 1976), pp. 90-118.

and whom they had worked with than by traditional market forces.

There were, of course, limits to the extent to which computer designers could indulge their personal preferences against the clear trend of scientific advance and market preference. Howard Aiken, despite his oft repeated preference for electro-mechanical components, eventually designed electronic parts into the Mark III and IV. I.B.M. also eventually introduced electronic equipment despite its apparent preference for electro-mechanical units. Some developments like core memories were so clearly superior to anything which had previously been available that they quickly took the industry by storm, with old machines being reconstructed to take advantage of the new technology.

Diffusion Among Users

We have been speaking primarily of diffusion of computer technologies among computer manufacturers. Another important form of diffusion was diffusion among users. Diffusion among users took three important forms. First, there was diffusion among organizations, as organizations without experience in computing imitated the applications pioneered elsewhere. Second, there was diffusion within an organization, as new applications were developed for computers already installed, and as different parts of an organization imitated the applications pioneered in other parts of the organization. Third, there were organizations without computing experience using the computer for completely new applications. We shall focus here on the latter two forms of diffusion, though some evidence of the first kind can be found in the table in Appendix B.

Actually, the first application of the ENIAC was not in calculating firing tables but in doing feasibility calculations for the hydrogen bonb, the "super." Since the War was over (this was in December, 1945), the pressure for firing table calculations abated and new priorities emerged. The analysis of nuclear weapons was from the beginning a highly methematicized proceeding, so that several of the early machines were used for this purpose, including machines at Army Ordnance (ORDVAC), Oak Ridge (ORACLE), Argonne (AVIDAC), Los Alamos (MANIAC), Rand (JOHNNIAC), and the University of California Radiation Laboratory (UNIVAC).

Another important early application that we have already mentioned was the use of computers as guidance systems for missiles. This application did not work out very well until the size and weight of the machines had been dramatically reduced, but by the late 'fifties computers were being used for this purpose. Early contracts for this purpose including the BINAC and DIDA and MADDIDA machines, the latter of which was picked up by North American to become the foundation for their machines. Hughes Aircraft also built an early machine for this purpose.

The aircraft manufacturers were also early users of computers for mathematical analysis. Konrad Zuse, we will remember (ch. 2, p. 38), had become frustrated with the difficulty of doing calculations needed for aircraft design as early as 1934. As we noted in Chapter Three, the increase in the cost of building prototypes of aircraft and conducting wind tunnel tests greatly increased the advantages of calculating more carefully in the first place, which required more extensive calculations. This application was a widely recognized one by computer builders in the early days, as aircraft manufacturers were already using I.B.M. tabulating equipment extensively in their calculations. Mauchly, who had no special association with the aircraft industry, recalls of his early anticipations of the applications of computers, "We said it would revolutionize airplane design--you wouldn't need all those wind tunnels."[19] In 1946 the National Advisory Committee on Aeronautics was interested in buying a machine for this purpose.[20] Northrop made an important contribution as a result of its calculations using I.B.M. tabulators and electronic calculators when two of its people, Greg Toben and Bill Woodbury, showed how to hook together a 603 electronic calculator with a 405 tabulator to produce a card-programmed calculator. I.B.M. was at first resistant to producing this machine, but Jack Northrop made a direct appeal to Tom Watson, and Watson had the machine made.[21] In 1948, when I.B.M. produced their 604 electronic calculator, Toben and Woodbury were loaned to I.B.M.'s San Jose lab to produce a 604-405 card-programmed calculator (the 604, as we noted in

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[19] John W. Mauchly, quoted in John Costello, "The Little Known Creators of the Computer," Nation's Business, v. 59, no. 12 (December, 1971), p. 62.

[20] J. H. Curtiss, "A Review of Government Requirements and Activities in the Field of Automatic Digital Computing Machinery," in University of Pennsylvania, Moore School of Electrical Engineering, Theory and Techniques for Design of Electronic Digital Computers, Report No. 48-9 (June 30, 1948), Vol. III, p. 29/4. NACA bought an electromechanical Bell Labs Model V. See U.S. Office of Naval Research, A Survey of Automatic Digital Computers, by N. M. Blachman (Washington: O.N.R., 1953), p. 9

[21] Jerry Mendelson, interviewed by Henry S. Tropp, January 3, 1972, Smithsonian (CHP), pp. 1-4.

Chapter 3, was programmed with plugboards, which made reprogramming a nuisance; programming with cards made it possible to change the program simply by loading in a new stack of cards). The card-programmed calculator was then offered commercially.[22] I.B.M. sold almost 700 of them.[23] I.B.M. had a certain advantage over other computer firms in that nearly everyone used I.B.M. card-processing equipment as peripherals, so I.B.M. had a good opportunity to find out what everyone else in the industry was doing.

Another closely related application involved the "reduction" of flight test data. Flight test data had to be "reduced" by fitting the data to a series of linear equations, so that the motion of the missile could be accurately described. The Air Force built FLAC for processing data from missile tests. The Wright Air Development Center sponsored several computers for this purpose, including OAREC, MIDAC, three ERA 1102's, and a Datatron. The Rome Air Development Center similarly sponsored a Telerigister computer, an ELECOM 120A, and a Bendix D-12. The Army Ordnance White Sands Proving Ground for missiles at Las Cruces, N.M., ordered a CRC 107, two CRC 102A's, two ERA 1103's, and a CRC 106. The Naval Air Missile Test Station at Point Mugu, Calif., bought a CRC 107, a RAYDAC, and an ELECOM 100. The Naval Ordance Test Station at China Like, Calif., bought an I.B.M. 701, a CRC 102A, and a CRC 105. The National Advisory Committee on Aeronautics, along with its initial

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[22] _Ibid_.

[23] Byron E. Phelps, The Beginnings of Electronic Computation," I.B.M. Systems Development Division Technical Report Tr-00.2259 (Poughkeepsie: IBM-SDD, December 9, 1971), p. 15.

purchase of a Bell Labs Model V, later bought an I.B.M. 650 and a Datatron. The aircraft companies were also buying computers. Northrop used its own MADDIDA's and also sold one to North American Aviation. Lockheed bought two I.B.M. 701's, an I.B.M. 650, and a CRC 105. Convair bought an ERA 1103, an I.B.M. 650, and an LGP-30. Boeing, Douglas, and Martin all bought I.B.M. 701's, and Republic got an I.B.M. 650.

The testing of ship hulls involved somewhat similar computations, though using "water tunnels"--towing tanks--instead of wind tunnels. The Navy's David Taylor Model Basin in Carderock, Md., bought an ALWAC and a UNIVAC, and the Stevens Institute Towing Tank in Hoboken, N.J., built its own copy of Northrop's MADDIDA and also bought an ELECOM 100. The ordinary ordnance proving grounds were also well equiped, with the ENIAC, EDVAC, ORDVAC, and an ELECOM 100 at the Army's Aberdeen Proving Ground, and the Harvard Mark III, a CRC 105, and the NORC at the Navy's Dahlgren Proving Ground.

Another early military application was flight simulators and gun control. The Whirlwind project at MIT was originally intended to build a computer for use in a flight simulator. Bell Labs did extensive work during and after the War with gun controls. These were essentially computers used for what would be called in industry "process control," where the output of the computer is not in printed or digital form, but is in the form of direct control of a mechanism.

Another early military application was the general field of air traffic control, air defense, and tracking. These are, mathematically, closely linked fields. As Weiser points out, air traffic control involves tracking planes to keep them apart; air defense involves tracking planes to bring them together.[24] This work began at MIT as an outgrowth of the Whirlwind project, and also involved submarine tracking. In late 1949, MIT received a contract to figure out how to land jet aircraft at a rate of one every thirty seconds. In 1950, the USSR exploded its first nuclear bomb, and the Air Force became much more concerned with air defense. It was decided about 1951 that the Whirlwind was inadequate for this purpose, so they began designing what eventually became known as the SAGE system. I.B.M. won the contract to develop it, which was a surprise, considering their relative inexperience with computers at the time. The 701 emerged partly as a byproduct of this system (the 701 was often referred to as the "Defense Calculator"). The development of magnetic cores as memory components also emerged from this work at MIT.

The military also had a variety of miscellaneous needs, such as the construction of high latitude (i.e., for polar regions) navigation tables, studies of shock wages in underwater explosions, studies of naval antenna designs, processing of data from ballistics tests for the improvement of mathematical ballistics models, etc.

The military also became increasingly interested in using computers for what would be called "commercial applications." Marshall Wood, in the Air Force, supported Leontief input-output work because he thought it would be useful for resource planning for Air Force procurement.[25]

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[24] C. Robert Weiser, interviewed by Richard R. Mertz at McLean, Va., March 20, 1970, Smithsonian (CHP), p. 18

[25] Murray A. Geisler, interviewed by Richard R. Mertz at Santa Monica, Calif., February 22, 1971, Smithsonian (CHP), p. 4.

The Air Force also bought the second UNIVAC and used it for inventory management and logistical problems. The techniques developed were useful in managing logistics during the Korean War. An I.B.M. 701 was also used at the Naval Supply Office in Philadelphia for inventory management. A special purpose machine was built by Engineering Research Associates called the Logistics Computer. This was used at the Logistics Research Project at George Washington University, which was sponsored by the Office of Naval Research. The Air Force Operations Research Office at Eglin Air Force Base used an ERA 1103 to develop operations research models, and the Air Force Air Material Command at Wright-Patterson AFB used a UNIVAC for inventory management. The Army commissioned a special machine called the ORDFIAC (ORDnance Fiscal and Inventory Automatic Computer, also called the ELECOM 200) to be built by Underwood for the Army's Letterkenny Ordnance Depot in Chambersburg, Pa., for inventory control. The first I.B.M. 702 (the commercial version of the 701) when to the Naval Aviation Supply Office in Philadelphia. Military computers were increasingly turned to "commercial" applications during the 1950's, as Table 1, indicating Navy applications, shows. By 1958, computers used for commercial purposes outnumbered those used for scientific purposes.

Civilian Government agencies were also active computer users. The most important early user was the Census Bureau, which contracted with the National Bureau of Standards as early as the Spring of 1946 for a computer.[26] The National Bureau of Standards, which was acting as a

[26] U.S. Bureau of the Census, "Role of the National Bureau of Standards in the UNIVAC Program" (U.S. Bureau of the Census, mimeo, June 14, 1951), p. 2.

TABLE 1

USE OF COMPUTERS BY U.S. NAVY, 1954-1959

At end of Fiscal year	Number of Systems			Number of People			Dollar Cost ($000)		
	Total	Commercial	Scientific	Total	Commercial	Scientific	Total	Commercial	Scientific
1954	5	1	4	102	49	53	849	234	615
1955	10	2	8	166	64	102	4720	605	4115
1956	20	8	12	262	94	168	3891	1160	2731
1957	29	13	16	586	368	218	9184	4234	4950
1958	48	26	22	988	718	270	12299	7272	5027
1959	72	46	26	1242	930	312	23057	15961	7096

Source: Navy Management Review, data reported in Datamation, v. 4, no. 5 (September/October, 1958), p. 3. Data for 1958 and 1959 are contemporary estimates. Cost data exclude peripheral equipment.

consulting and contracting agent for the Census Bureau, in turn contracted with the new Eckert-Mauchly Computer Corp. (still called Electronic Control Co.) in the Fall of 1946 to design a computer for the Census Bureau. In 1948, the Eckert-Mauchly design was evaluated by NBS and a contract was awarded to build a machine, which was delivered in 1951. The Census Bureau had fairly prosaic computational work to do in tabulating their complete counts, but it also had more advanced computations to do in connection with sample surveys.

Another early civilian Government application was in meteorology. This was a special interest of von Neumann's and thus was an important application for the IAS machine. Numerical meteorology (the use of numerical approximations of differential equations to model atmospheric flows for purposes of weather prediction) was first developed by Lewis F. Richardson during World War I,[27] but the theory of numerical approximation and the computational capacity necessary to solve the equations were not sufficiently advanced to make the techniques workable. By the late 1920's, Courant had solved the theoretical problems in numerical analysis, so that the only remaining problem was that of speeding up the computations. A twenty-four hour forecast (a prediction based on data available twenty-four hours prior to the time for which the weather is forecast) is not of much use if the computations take more than twenty-four hours to do.

Von Neumann was interested in the meteorological work because the equations involved much the same sort of fluid dynamics modeling as was

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[27] Lewis F. Richardson, Weather Prediction by Numerical Process (New York: Dover, 1966). Original edition, 1922.

involved in much of his other work (see Chapter Two, pp. 57-58). He also probably felt that it would provide a striking demonstration of the power and usefulness of the computer because it would allow the solution of a problem in which virtually the entire population was strongly interested. The early numerical forecasts were rather crude, because memory limitations forced the use of relatively simple models, but in the years since numerical forecasting has become the major technique for weather forecasting.[28]

Other computational work was sponsored by the Bureau of the Mines and the Coast and Geodetic Survey, which in its map-making work often solved quite complex systems of linear equations (e.g., 400 equations with 400 unknowns).[29] Some work was done by civilian agencies under military sponsorship, such as the work done by the NBS Mathematical Tables project in analyzing and tabulating various kinds of functions, such as Bessel functions, spherical scattering functions, Jacobi elliptic functions, Mathieu functions, hyper-geometric functions, and gamma and error functions for complex arguments. This work was originally supported by the Office of Naval Research, but was supported by the Commerce Department after 1948.

Non-governmental applications developed more cautiously, but fairly wide experimentation with advanced computing had taken place by 1955. One of the first areas where civilian interest manifested itself was in the insurance business. This was one of the first businesses to use

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[28]Goldstine, _op_. _cit_., pp. 304-305.

[29]Curtiss, _op_. _cit_., p. 29/11.

I.B.M. tabulating equipment in the early years of the century, and its great data processing requirements made it a natural market for computers. Edmund C. Berkeley of Prudential had long been interested in more advanced computational techniques, beginning at least with his viewing of Stibitz' Complex Number Calculator at the American Mathematical Society meetings in 1940. After the War, he arranged for Prudential to finance some of the work at Eckert-Mauchly, though Prudential did not take delivery of any of the early UNIVACs. They did buy a CRC 102A from Computer Research Corp., in California. UNIVAC #8 went to the Metropolitan Life Insurance Company, #14 went to Franklin Life Insurance Company, and #15 to Pacific Mutual Life Insurance Company. Two of the early Datatron Computers went to Allstate Insurance and General Insurance.

Another important early commercial application was, broadly speaking, inventory control. This included not only inventory control in the usual sense, but also automated reservations systems for airlines and hotels, and stockbroker quotation systems. This was an important early military application, as well, given the enormous logistical problem of the military. This led to the construction of the Logistics Computer by Engineering Research Associates for the Logistics Research Project at George Washington University, to the construction of the ORDFIAC, and to the use of the Air Force UNIVAC and the Navy's 701 for inventory management. Several of the general purpose machines ordered by large corporations were used for inventory management, and some special purpose machines were also designed for this purpose. B. Altman, the New York department store, ordered a machine called the Magnefile from the Electronics Corp. of America in Cambridge, Mass., that was specially designed for inventory control. A special purpose machine was also built by the Teleregister Corp. for American Airlines called the "Reservisor" that was designed to keep track of airline reservations. I.B.M. designed software to allow their general purpose machines to be used for airline reservations as well. A variation of the Reservisor was sold to the Toronto Stock Exchange as a stock quotation system. A similar system was later designed by Scientific Data Systems.

A variety of other applications was also conceived. One sponsor of Eckert and Mauchly's work was the American Totalizator Company, which wanted to use computers to keep track of betting odds on racetrack tote boards. One company developing its own machine was the International Telemeter Corp., a division of Paramount Pictures, which was interested in pay television and hired several people from RCA to work on the computing componentry which they assumed would be necessary. They built a machine called the TC-1 modeled after the IAS machine.

General Motors used computers for a variety of purposes. Partly they were used for conventional tasks such as design analysis of suspention systems and gas turbines and inventory management, but one unusual application was computer controlled machine tools to reduce die-casting time:

> It took thirty or forty weeks to build sets of these dies, so essentially you had to release the next year's model for manufacture to get the dies built, before you could see what the competition did this year. That was a very important competitive edge. If we could just reduce the time, to hell with the cost,

> it was the time that was very important so that we could see what Ford announced, say in 1958, before we had to release the tooling for the 1959 General Motors cars.[30]

Another unusual early application was Axel Wenner-Gren's plan to use a computer to control a monorail. The application was unsuccessful, but it did lead to the establishment of a new company, the Alwac Corp. Another interesting early effort was General Electric's early effort to automate an appliance factory in Louisville, Ky.[31] This effort was largely unsuccessful, and reflected badly on UNIVAC, whose machine (#7) had been used. There were, of course, also the more prosaic uses of computers for processing payrolls, doing accounting, etc. These applications were largely pioneered by T. R. Thompson at Lyons.

Another important commercial application was in process control, where the form which the output of the computer takes is the direct control of a mechanism. Ramo-Wooldridge pioneered in this area, with Eldridge Nelson, Ralph Conn, and Montgomery Phister being the key people. Their first contract was with the Riverside Cement Co., a small Los Angeles firm run by a former Harvard Business School professor. That experience helped them to get their first major contract with Texaco for its Port Arthur, Texas, refinery in 1958, an application which was widely publicized (it made the cover of Business Week).[32]

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[30]Bob Patrick, interviewed by Robina Mapstone at Northbridge, Calif., February 26, 1973, Smithsonian (CHP), pp. 44-45.

[31]See Ibid., p. 67, Martin H. Weik, A Survey of Domestic Electronic Digital Computing Systems, U.S. Proving Ground, Aberdeen, Md., Ballistic Research Laboratories, Report No. 971 (December, 1955), p. 177, and George Schussel, "IBM vs. Remrand," Datamation, v. 11 (May, June, 1965).

[32]Montgomery Phister, interviewed by Robina Mapstone at Los Angeles, Calif., February 21, 1973, Smithsonian (CHP), pp. 40-45.

Some impression of the development of applications can be derived from Appendix B, which consists of a table listing all of the computers which had been developed by 1955. A summary of the data in Appendix B appears in Table 2, showing how many machines had been finished by each year, broken down according to whether their users were military (including corporate military contractors), civilian Federal Government, or non-Federal Government (including state and local government as well as private users). The discrepancy between the data for 1955 and 1955a is due partially to differences in the definition of a "computer" and partly to differences in definition about when a computer is "installed," but given the rapid rate at which computers were being installed at the time, the discrepancy amounts essentially to a quibble over whether thirty or so computers were installed in December or January. It is difficult to say how many of the computers in the list were used for scientific and how many for commercial purposes. Many were no doubt used for both. Some impression of the general pattern of use can be gleaned from a perusal of the data in Appendix B.

The significance of market considerations was often muted in the pattern of diffusion among users. To begin with, many users, particularly at the beginning, were government organizations and non-profit firms whose objective functions were unclear. If the objective function is unclear, then it is naturally difficult for the firm to determine if the computer is helpful in maximizing that objective function. Even for the business firms which acquired computers, for which the objective function was presumably profits, the impact of the computer on that objective function was often unclear. The firms involved were typically large, so that the impact of the computer on the firm's earnings would

TABLE 2

COMPUTERS INSTALLED, 1946-1960, BY YEAR

Year	Each Year				Cumulative Totals			
	Federal Government Military	Federal Government Civilian	Private	Total	Federal Government Military	Federal Government Civilian	Private	Total
1946	1			1	1			1
1947				0	1			1
1948				0	1			1
1949	1			1	2			2
1950	2	1		3	4	1		5
1951	4	1	1	6	8	2	1	11
1952	9	1	7	17	17	3	8	28
1953	32	1	12	45	45	4	20	73
1954	31	6	47	84	80	10	67	157
1955	38	1	135	174	118	11	202	331
1955a								363
1956				520				883
1957				746				1629
1958				986				2615
1959				1261				3876
1960				1010				4886

Source: 1946-1955, Appendix B. 1955a-1960, "Appraisal of the Market for the Bendix G-20, G-20 [*sic*, they mean "G-15, G-20"], and G-25 Computers," Booz, Allen & Hamilton, Management Consultants (January, 1961), Appendix A, p. 7. Reprinted in U.S. Senate, Subcommittee on Antitrust and Monopoly, *The Industrial Reorganization Act, Part 7, The Computer Industry* (Washington: G.P.O., 1974), p. 5191.

be hard to gauge. In some cases, especially where the computer was used to do work that the firm was already doing, but which the computer could do without disrupting the operations of the firm, the cost-saving impact of the computer was quite clear. This was often the case, for example, with aerospace firms, where the ability of the computer to perform design analyses and save the firm the expense of building and testing prototypes led to substantial, immediate, and obvious savings.

In the case of firms using computers for more business-oriented data processing, on the other hand, the full impact of the computer could often come only after the reorganization of, say, the firm's accounting system. To some extent, the computer produced a different product in terms of accounting statistics than conventional methods did, and comparisons of productivity were therefore ambiguous. Moreover, there were always difficulties with the system at first which gave a more negative impression of its profitability that would come with greater experience.

Thus, in the case of both for-profit and non-profit organizations, there was room for great uncertainty about the usefulness of the computer in achieving the organization's objectives. As a result, decisions about using computers were often made for the same reasons as decisions about the use of a particular technologies by manufacturers--on the basis of personal prejudices and intuitions of key people in various firms. Firms who harbored computer enthusiasts tended to acquire computers, regardless of the potential profitability of the machines. As it happened, though, firms which harbored computer enthusiasts often did make profitable use of their machines where otherwise similar firms did not. People became enthusiastic about computers because they imagined all the wonderful things computers were going to do for them. As I have stated before, the significance of the computer lies less in its ability to do work more cheaply that was already being done than in its ability to do at a reasonable cost work that was impossible before. An important part of making profitable use of a computer was in dreaming up new applications for it. Computer enthusiasts were more likely to come up with these applications, and more likely to press for the changes in the operations of their firms that would make the computer useful.

Thus, the profitability of the computer was not objectively defined for any firm. It largely depended upon how well the firm used the machine, and that largely depended upon personal characteristics of the firm's personnel--their enthusiasm for the new technology. Thus, people like E. C. Berkeley at Prudential and T. R. Thompson at J. Lyons & Co. pioneered in the use of computers and made them contribute to the profitability of their firms. For non-profit firms and government organizations, the role of enthusiasts could be even greater, for the non-profit organization has the option of redefining its mission in such a way as to make its mission more suitable for achievement with computers.

The Role of Radical Innovation

We have argued, in general, that non-market, "behaviorist" forces were important in determining diffusion patterns in the early computer industry. This was true both for diffusion of new types of technology among computer manufacturers and for diffusion in the use of computers among customers of these manufacturers. We suggested that "convection" was the chief mechanism for the diffusion of technological ideas--the actual movement of people from one firm to another. This was partly true of necessity--there were few ways of getting detailed, up-to-date information about what was being done at another firm other than to hire someone from that firm (this remains, incidentally, a prominent feature of the computer industry today). It was also true because no one really knew what characteristics of a computer would be optimal for various users, and, insofar as users did know, the selective force of the market was weak because demand was so strong (many computer firms went out of business, of course, but they usually did so becasue of bad management and undercapitalization in a very capital-intensive industry--one could not push anything on the market, of course, but sales generally remained well above forecasts throughout the 'fifties and early 'sixties).

I would argue that these phenomena would tend to be characteristic of the early stages of most industries founded upon a radical innovation. Technical change would tend to be rapid in the early stages of the learning curve, so that published sources of information would be scarce and out-of-date. Demand might well be strong (though this less certainly), and the best configuration of the innovation might well be unclear to users and manufacturers alike. These factors would make market signals weak and confusing, so that the scope for non-market, behaviorist factors would be wide.

CHAPTER SIX

THE IMPACT OF FEDERAL GOVERNMENT SPENDING ON COMPUTATIONAL INNOVATION

Government spending had a major impact on the development of the computer. In its absence, the development of the computer would almost certainly have been delayed by twenty years or more. There were two major stages in this Federal support. The first was the support of the ENIAC project (and, to a lesser extent, the Mark I and cryptoanalytic projects) during World War II; the second was the support for the variety of projects financed by various Government agencies after the War. We shall here briefly discuss the significance of the wartime support for the ENIAC project, and the significance of war in general for technological change, and then consider in somewhat greater detail some of the peculiar features of the postwar support for computational projects.

The Impact of the War

We have from time to time noted that the use of electronic components was a critical step in the development of the computer. It is possible to build a computer with electro-mechanical parts, but such a machine has so little advantage over conventional technology that the continued use of electro-mechanical technology would have consigned computers to an existence as university curiosities, without sufficient advantage to have warranted continued technological development.

The significance of the War, essentially, was that it introduced the use of electronic components for the first time on a large scale. I would argue that the War was the only event which would have caused this to happen, at least until the development of cheaper, more reliable semiconductor electronic components in the late 1950's. In the absence of the War, therefore (and the postwar military spending resulting from it), the development of the computer would have been at best slow and painful. Perhaps Atanasoff's machine would have inspired a few small-scale electronic imitators, but these would not have been large enough to convince the doubters of the usefulness of electronic components.

There were, as we have seen in Chapter Two, two major computer projects in progress prior to World War II, one at Harvard and one at Bell Labs.[1] Both of these projects used electro-mechanical parts for

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[1]See Chapter Two, pp. 33-38. I omit Atanasoff's project from the "major" category partly because of its meagre funding, partly because it is not clear that it ever produced a working machine, and partly because it is not clear that the project ever would have had much significance aside from the influence it had on Mauchly. The Atanasoff project is one example of the negative impact of war on technological change--Atanasoff was drawn off from working on his machine, on the brink of making it operational, by the need to do war work at the Naval Ordnance Laboratory. He evidently made little or no effort to interest the NOL in supporting the development of his ideas, which almost certainly would have been useful to them. In fact, he does not even seem to have communicated the nature of his computational work to his colleagues. Atanasoff's behavior is unusual in this respect. Most inventors have great enthusiasm for their own ideas and seek all opportunities to develop them. Atanasoff seems to have lacked this drive, and there is therefore some doubt as to what the significance of his project would have been even in the absence of the War. See Isaac Auerbach, interviewed by Henry S. Tropp, in Philadelphia, February 17, 1972, Smithsonian Institution, The National Museum of History and Technology, Computer History Project (Washington: Smithsonian Institution, 1972), pp. 6,

their machines, for reasons which we have discussed at length in Chapter Three. Because of the strong commitment of the designers of these machines to electromechanical parts, it is doubtful if they ever would have switched to electronic parts in the absence of pressure from the outside. Even in the presence of clear evidence of the success of electronic parts in the ENIAC, they continued to use electro-mechanical parts well into the late 'forties on the machines designed during that period. It seems safe to say, then, that in the absence of the ENIAC project or some other comparably successful project using electronic parts, the use of such parts would not have occurred either at Bell Labs or at Harvard.

Nor does it seem likely that the use of electronic parts would have been pioneered elsewhere. Various firms, as stated above, had experimented with electronic parts, but it seems unlikely that any of them would have pioneered in their use. The firms known to have experimented were I.B.M., NCR, and RCA. I.B.M. and NCR were both the leading firms in their industries who had established a pattern of not taking the lead in innovation (that they did research on electronics simply indicates that they were keeping themselves in a position to respond to a technical challenge by another firm, but since they had little market share to gain by taking the technical lead, they would have been unlikely to be innovators themselves--in fact both firms, despite their lead in research, lagged in computational innovation). RCA was unlikely to have

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33 [Interviews in this series will hereafter be referred to as Smithsonian (CHP)], and Robert D. Elbourne, interviewed by Richard R. Mertz, in Gaithersburg, Md., March 23, 1971, Smithsonian (CHP), p. 9.

launched an expensive effort to enter another industry at the same time that they were developing color television. No other firm would have been likely to pioneer in the development of electronic computing equipment in the absence of either the pull of a well-defined demand or the push of experience in that particular field. No well defined demand was likely to develop in the absence either of a crisis like the War or the availability of a developed technology.

In the absence of the military demand spawned by the War and its aftermath, then, it is unlikely that the civilian sector would have developed computers on their own until after transistor prices became low enough to appear attractive as computational components, around 1960. Thus the War probably advanced the development of the computer by about twenty years. This increases the significance of the behaviorist considerations discussed in Chapter Three, because, in the absence of the War, they probably would have delayed the development of the computer not for the fifteen years or so that they actually did, but for nearly thirty-five years.

The impact of war on technological change has occasioned a fair amount of argument among historians and other social scientists,[2] but little has been written about it by economists, perhaps because the impact of war seemed obvious. In a way, its impact is obvious. War constitutes a dramatic increase in the overall level of demand accompanied by a dramatic shift in the pattern of demand which makes the

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[2]Notably Werner Sombart, Krieg und Kapitalismus (Munich, 1913), and John U. Nef, War and Human Progress (Cambridge: Harvard University Press, 1950), particularly ch. 10.

increase in demand for war-related goods particularly intense. Yet the nature of wartime demand is significantly different from that of a peacetime demand, and that difference deserves separate treatment.

One major difference is that the way in which a firm responds to an increase in demand in wartime is fundamentally different from the way in which it responds to such an increase in peacetime. In peacetime, a firm has several options in responding to an increase in demand (indeed, one of the weaknesses of neoclassical theory is that it is quite vague as to the way in which a firm actually will respond). A firm can raise prices so as to clear the market; it can stretch out delivery dates, keeping prices constant (this possibility is ruled out by the usual neoclassical analysis, though it is, of course, quite commonly resorted to); it can increase real output, using the same technology, hence using more productive factors; or it can introduce a new technology allowing it to increase production without an increase in the use of productive factors. Or, of course, it can use some combination of the four.

The wartime economy is different from the peacetime economy first of all because the first response which we have listed is inadmissable during wartime. Firms cannot get away with exploiting a situation of excess demand by raising prices. The firm is no longer free to maximize profits as it sees fit, because it has in large part become a creature of the government. The whole economy, in fact, becomes an extension of the government administrative apparatus. Labor is largely allocated by the government, either directly or indirectly via selective draft calls. Capital investment is largely controlled by the government, with free capital largely absorbed in government borrowing. War production boards govern production of most goods, both war-related and consumer goods. Prices and wages are controlled, and profits are controlled by excess profits commissions. It would therefore in any case be pointless for a firm to raise prices in response to an increase in demand, since the excess profits would be specifically taxed away. In peacetime, the firm can treat an increase in demand as, in effect, a demand for higher prices. In wartime, the demand is for real output, and must be treated as such. The firm's goals might have changed so that profits are no longer the primary goal anyway, but whether this happens or not, the constraints within which it now operates makes profit maximization pointless.

This leaves either a stretching out of delivery dates or an increase in real output. An increase in real output with the existing technology may be impossible because the requisite capital and labor may be unavailable from the greatly tightened factor markets prevailing during wartime. If the demand is great enough, of course, the resources will be found to satisfy it, but if the demand is of the second order of criticalness, then the firm must do the best it can with the resources it has available already. In peacetime, an increase in demand means either that resources can be bid away from other sectors where demand is decreasing, or that an inflationary aggregate excess demand exists which justifies an increase in prices. In wartime, the firm is faced by the novel situation of being called upon to increase output without an increase in resources, so that there is a special pressure in favor of new process technologies. (The incentive to develop process technologies during peacetime is that of increasing profits by cutting costs; it seems likely that the wartime pressure to boost output will be more intense than the peacetime pressure to boost profits, since the wartime demand will face fewer conflicting goals interfering with it, such as maintaining employment.)

The increased pressure for technological change occurs not simply because of the increase in demand, but because of the change in the environment within which the firm operates which determines its response to that demand. The primary goals of the firm have changed. Profit is no longer the primary goal--high profits will, in any case, be taxed away. Output replaces profit as the primary goal (though profit may remain a subsidiary goal, operating as a constraint--the firm still wants to avoid losing money, though in the wartime context that should not be very difficult), and the development of more productive processes of production has a more certain impact on output than it does on profits. A technically successful new production process is certain to increase output, even if its effect on profits is uncertain. Once the firm has set up output as its primary goal, it no longer has to worry as much about whether a potential new process is cost effective. It need only worry about whether it will increase output without increasing use of factors.[3] A shift in the goals of the firm from an

[3] One might argue that any process which increases output without increasing consumption of <u>any</u> factors can't help but be profitable. Whether this is true or not may depend upon the level of output which the firm can expect. It may only be worthwhile to introduce innovations if the demand for the firm's product is not only high but is expected to remain high for a long enough period of time to recoup the cost of developing the innovation. War-induced innovation is often only successful if there is an implicit commitment on the part of the government

emphasis on profits to an emphasis on output will therefore naturally make the firm more receptive to process innovations. A similar effect occurs with respect to product innovations. War increases the importance of product quality (at least along some dimensions, where product quality is critical to military success), so that firms will shift their goals away from maximizing profits in favor of maximizing product quality, if necessary at the expense of profits. Quality-improving innovations will not face the same obstacles as they do in peacetime of proving cost-effectiveness. The shift in goals away from profits will therefore encourage innovation generally (there will be exceptions, of course--a product innovation which disrupts production of a needed product will be discouraged).[4]

The administrative control of the economy by the government brings us to the second major way in which a wartime economy is different from a peacetime economy, namely, that the market allocation of goods and services breaks down, leading to important consequences for technological

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to protect the firm from bankruptcy in the event that military orders suddenly cease if peace should "break out." See, for example, Roy and Kay MacLeod, "War and Economic Development: Government and the Optical Industry in Britain, 1914-18," in J. M. Winter (ed.), War and Economic Development (Cambridge: Cambridge University Press, 1975), pp. 188-190.

[4]One valuable feature of a behaviorist analysis is that it allows us to analyze these trade-offs among multiple, conflicting goals directly, rather than by trying to figure out a way in which attention to one goal (e.g., output) to the detriment of another (e.g., profits) somehow maximizes the latter in the long run. A behaviorist analysis allows us to use the same basic analysis in wartime as we do in peacetime, with only the priorities of the goals shifted, while the neoclassical analysis must be mostly abandoned when war breaks out.

change. In the peacetime economy, the market assigns prices to capital and labor to indicate their relative scarcity. In the wartime economy, these prices no longer reflect relative scarcity, since they are arbitrarily controlled. As between capital and labor, the government's control is probably greater for capital than it is for labor. Capital, in the sense of a fund of money, takes on a new form during wartime that is practically unrecognizable from its peacetime nature. For a government approved project, capital becomes practically free during wartime. The government can print all the money it needs. It can, by raising the prices of consumers goods, in effect enforce whatever savings rate it desires, subject only to the constraints of the people's tolerance of its actions, which are likely to be quite generous during wartime. The government can create capital in a way in which it cannot create labor, so that its control of the one factor is greater than its control of the other. Because the ordinary restraints imposed by the difficulty of raising capital disappear, the economic problem for the government becomes amazingly fundamental. It has a stock of physical resources over which it has virtually complete control, and it must decide how best to use those resources to advance the ends of the war.

The implication of this for technological change during wartime is that the usual issues of factor-saving bias in invention lose much of their meaning. Since capital has no "price" in any definite sense, there cannot be any technology which has an optimal degree of capital-intensity given that price. Rather, the relevant question, in choosing between the current technology and possible new technologies, is their use of various kinds of labor, raw materials, and existing capital equipment (i.e., existing capital equipment should be treated as a gift of nature--that it was bestowed by past generations of humans rather than by nature in the more usual sense is irrelevant).

For an innovation like the computer, raw materials are not very significant, so the optimality of computers (from the point of view of maximizing the achievement of the government's war aims) amounts to the question of how various kinds of labor and capital equipment should be used. What alternative uses are there for those women punching desk calculators for which they might be freed if there were a computer? What other gadgets might those engineers and mathematicians be working on if they weren't working on a computer? How important are the thousand or so radios which will not be built because those vacuum tubes and electrical parts have been pre-empted for use in a computer? The economic problem continues to exist in its traditional, most fundamental form, but the economizing of "capital" has lost most of its meaning. There are, of course, still trade-offs involving time. Indeed, these trade-offs have, if anything, become more critical. A development which will make an impact on the war effort in one year rather than in two is not only valuable but possibly invaluable, in that the failure to win battles with output produced now may make output produced next year worthless. So the value of time, which capital and interest ordinarily mediate, is still great, but those trade-offs are now made directly rather than being represented by rates of return on capital. What are the relative values of radios available now versus a computer available (possibly) two years from now? What are the relative values of a modest increase in computational output available now (by using Ph.D. mathematicians to punch desk calculators or, more likely, to develop simpler mathematical techniques) versus a much greater increase in output available (possibly) two years from now? Since there is no market to measure these relative values, they must be traded off directly.

The suspension of the price system as the framework for determining the desirability of new technologies means that the usual constrained procedure for evaluating a potential technology is shaken loose from its narrow focus. So long as prices provide a definite standard for evaluating the economic feasibility of a technology, it is easy to reject many possibilities. Their costs are easy to determine, and their benefits are usually uncertain, so that it is easy to make a case on economic grounds against them. When prices clearly lose their economic rationale, so that they are merely arbitrary administrative labels, then economic arguments framed in terms of such prices no longer have any persuasiveness. When capital is available more or less for free from the government for approved projects, the real cost of an innovation becomes as uncertain as its benefits, and perhaps more so. When the benefits are seen as an apparent military necessity, the costs seem fuzzy by comparison. In particular, if the price of capital is a certain amount, then capital-intensive projects can easily be dismissed because their rate of return is not reliably in excess of the interest rate. But if there are no meaningful interest rates to be used as standards for capital productivity, then it is easy simply to ignore the capital productivity of the proposed innovation, and concentrate instead on measures of its productivity in terms of factors which are more obviously scarce, such as labor. The breakdown of the price system thus breaks down the constraints imposed by economic justification, so that innovations which either are not economically justifiable, or cannot be shown to be economically justifiable, nevertheless can be approved because they solve immediate problems. Projects can be approved if they are efficient within a narrow context, even if they are inefficient within a broader context.

Even this statement of the situation understates the significance of the change which has occurred, because we are still implying that there is a well-defined, albeit unmeasurable, concept of "efficiency" at hand. Yet clearly this is not the case. The usual concepts of value are so altered by a war that it is hard to say anymore how the real value of anything would be defined, much less measured. One could, in theory, assign shadow prices to various factors of production, but it is not clear how one would define an appropriate procedure for defining them. A war radically changes the goals of society, with consumers' "utility" shunted aside and social survival thrust to the fore, so that it is unclear how these two broad criteria of value should be weighted in assessing the value of something which contributes partly to them both. A war also brings with it so much uncertainty about the future that it is almost impossible to say how much value something will have even to a well-defined part of the war effort (the ENIAC being a good example of this). Thus, the true costs and benefits of a potential innovation become virtually indefinable, much less measurable, so that practically anything will seem a reasonable use of society's resources if it has a sufficiently glib proponent. War thus eliminates much of the framework of economic constraints which inhibit technological change in peacetime. Objective standards of rationality become less significant, and the sorts of behaviorist factors which we emphasized in Chapter Three are given freer rein. The identities of the sponsors of a project and their personal backgrounds become more important in determining whether or not a project will be proposed, and similar personal data about their supervisors will largely determine whether or not it is approved. This will mean that the chances of gaining support for a radical technological change will be better. The benefits of radical technological change are generally more uncertain than those of more incremental change, so that it is more difficult to justify in the ordinary peacetime climate. In the wartime climate, where the usual constraints have been relaxed, gaining support for a radical innovation is likely to be easier.

It also seems likely that war will encourage a more radical approach to solving problems because the circumstances in some sense seem to demand it. In peacetime, the environment is perceived as changing slowly, and the technology appropriate to that environment will be perceived as changing slowly as well. People are unaware of changes which fundamentally alter which technology is "best"--e.g., that developments in vacuum tubes had made them the best technology for large-scale computation. The obviously radical change in the environment brought about by war suggests that the incremental approach normally taken to technological change in peacetime is no longer appropriate, that radical changes in the environment demand the consideration of radical approaches to dealing with it. For all of these reasons, war will have a liberating effect on people's conceptions of how to solve problems. It will partly force and partly allow them to consider more radical approaches to solving problems. The effect of war on technological change is thus two-fold. It first of all increases the pressure for technological change, and secondly it stimulates an interest in more radical approaches to technological change. Both effects were important in bringing the computer into existence.

One major impact of the War was simply that it conditioned the nature of the postwar period, whose unique characteristics in turn affected the further development of computers. We therefore now turn to a consideration of the nature of government support for computing during the postwar period.

Postwar Government Support

Wartime support for computational developments would have been largely inconsequential if that support had not been continued into the postwar period. All the major postwar computer programs received Government (usually military) support. All the enthusiasm in the world would not have sustained Eckert and Mauchly and Goldstine and the others in the absence of continuing Government contracts. Of 73 computers completed by 1953, 53 had been paid for primarily by the Federal Government. Of these, 49 had been paid for by the military.[5] This extensive

[5]See Ch. 5, Table 2. I include here under military those computers built for aerospace firms, hence paid for directly or indirectly by the military, as well as those built for the AEC and its associated laboratories, both public and private.

Government program of buying computers was not due solely to the fact that computers were now more easily available. Real military spending (using as a deflation index what seems appropriate in this instance, viz., salaries of engineers) had increased tenfold from 1934 to 1948 (after postwar reconversion and demobilization were essentially complete) and increased by another two-thirds from 1948 to 1957.[6] There was thus much more money around to spend on computers than had been the case before the War. This high spending level after the war, in marked contrast with the experience of the military after World War I,[7] is of course explained by the continued military competition associated with the Cold War. The Cold War, in turn, was a result of the emergence of the U.S. and the U.S.S.R. as dominant powers during World War II. The effects of the Cold War, then, are secondary effects of the War itself, and are probably at least as important in explaining the development of computers as the direct effects of the War itself. But there are some characteristics of this postwar spending that make the Cold War doubly significant.

What is striking about Government spending on computers during the

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[6]United States Bureau of the Census, Historical Statistics of the United States, Colonial Times to 1970, Bicentennial Edition (Washington: G.P.O., 1975), pp. 175-176, 1120.

[7]Real military spending rose after World War I, but not nearly so dramatically (it approximately doubled from 1913 to 1922, depending upon what inflator is used). And even this more modest increase was from a much lower base. Moreover, military spending thereafter fell during the rest of the 'twenties for a total reduction of thirty or forty percent (again depending upon the deflator index) by 1934. Thus, from 1913 to 1934, real military spending at best increased 64% (using the consumer price index as a deflator) and possibly even fell 6% (using average hourly earnings as a deflator). See Ibid., pp. 1120, 211, and 172.

postwar period is how undemanding the Government was. Not only were deliveries often far behind schedule, not only did the machines often not conform to the specifications given in the contract, but in many cases it does not appear that the Government agency funding the contract could have had any reasonable expectation that the contract could be fulfilled. In many cases it does not appear that the primary motivation of the Government was to receive a useful device, but rather was to support the development of a technology, most of the benefits of which would accrue to private businessmen and consumers and other Government agencies rather than to the agency financing the development.

One of the best examples of this phenomenon was the BINAC project (see Ch. 2, p. 67), a subcontract let by Northrop Aircraft to the Eckert-Mauchly Corp. to develop an airborne guidance computer for the Snark missile. No one who was involved with the project seems to have ever had any confidence that the specifications of the project could be fulfilled. The idea of taking the computer technology that at that time required a large room to house a computer and cramming it into a package small enough to fly in a missile and, at the same time, making the machine rugged enough to withstand the vibrations of flight seemed utterly preposterous to those working on the project. Isaac Auerbach recalls:

> I'll never forget my annoyance when I was told that the machine had to be built small enough to fit through a bomb bay of an airplane. I said: "For God's sake, we're never going to fly this machine. The mercury's not going to fly [tubes of liquid mercury were used as a memory component]. The vibrations are just

> absolutely going to tear this computer apart. We're crazy to try to build this machine to fly."[8]

Jerry Mendelson notes, "The original BINAC was to have been an airborne computer, at least by contract, although I don't think Eckert and Mauchly ever had any intention of making it so. Clearly the machine they built never had any potential for flying at all."[9] Harold Sarkissian adds that the BINAC specifications

> ...were absolutely out-of-sight as far as the computer field was concerned, because it was supposed to be an airborne computer. I believe it weighed several tons by the time it arrived [at Northrop]...It was a best effort thing. Perhaps they had other reasons for asking for this; it may have been a way for the government to sponsor development which really was what was going on.[10]

The BINAC, in fact, never did fly. Eckert and Mauchly stalled on delivering it to Northrop, assigning their best people to the concurrent development of the UNIVAC and keeping the BINAC in Philadelphia so that they would have something to show to prospective UNIVAC customers.[11] The BINAC was supposed to be self-checking by having two identical halves, each of which would solve the problem independently, thus checking on the other. But apparently the two "identical" halves matched neither each

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[8] Auerback, op. cit., pp. 25-26.

[9] Jerry Mendelson, interviewed by Henry S. Tropp, January 3, 1972, Smithsonian (CHP), p. 12.

[10] Harold H. Sarkissian, interviewed by Robina Mapstone, at Costa Mesa, Calif., December 21, 1972, Smithsonian (CHP), p. 2.

[11] "Findings of Fact, Conclusions of Law and Order for Judgment," Honeywell vs. Sperry Rand, 4-67 Civ. 138, District of Minnesota (October, 1973), pp. 138-141. Reprinted in U.S. Senate Subcommittee on Antitrust and Monopoly, The Industrial Reorganization Act--S. 1167 (Part 7--Computers) (Washington: G.P.O., 1974), pp. 5857-5858.

other nor the specifications, and it took quite a lot of work to get it running at Northrop. Only one problem was ever run on it.[12] The other guidance computer designed for the Snark project, the MADDIDA, designed by Northrop itself, also was much too large to fly.[13]

One general problem with machines built by universities and other laboratories (both private and governmental) for their own use (usually with military funding) was that there was usually a conflict between the engineers building the machine and the mathematicians and others waiting to use it. We have already mentioned (Ch 2, pp. 64-65) the fact that the construction of the IAS machine was so regularly delayed that the completion date always was announced as eighteen months away, the "von Neumann constant." The National Bureau of Standards got so tired of waiting for the EDVAC to be delivered that they decided to build their own "quick and dirty" version of the machine, the SEAC. But this machine also became embroiled in conflict between Sam Lubkin, representing the mathematicians waiting to use the machine, who wanted a machine sooner, even if it were less sophisticated, and Ralph Slutz, representing the engineers building the machine, who wanted to approach the state-of-the-art, even if it meant delaying its completion.[14] It was always difficult to say exactly when these machines were completed, because the engineers kept tinkering with them, disrupting the running of problems. Forman Acton, a mathematician working on the NBS' Los Angeles machine, the SWAC,

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[12]Mendelson, op. cit., pp. 14-15.

[13]Donald Eckdahl, interviewed by Henry S. Tropp, September 25, 1972, Smithsonian (CHP), p. 22.

[14]Elbourne, op. cit., pp. I/II/3 - I/II/15.

complained, "I never computed anything useful on the SWAC for the three years I was there. It was always being modified...."[15] The usefulness of Government-built machines was also inhibited by political considerations. Derrick Lehmer noted that most of the work done by the SWAC was relatively theoretical:

> One reason for it was that we got instructions from Washington that we should not compete in any way with private enterprise. I guess this came from Nixon originally. Some small person in the computing, what computing that was being done in the Los Angeles area with what we called CPC's [a primitive IBM computer], some person with a CPC installation, and he heard some work was being done for the Army or for the Navy at our establishment and this was bread out of his mouth, you know. So he went to the Little Business Bureau [sic] and he stormed to, I guess it was Homer Fergueson in those days. Fergueson did some phoning and we finally got the word that this was socialism. So we couldn't work on any practical problems....[16]

In effect many of these early computer projects became not so much projects to build computers to do useful work as projects to develop computational technology, without any serious expectation that any useful work would immediately result.

In some cases the support for development of computational technology, without its necessarily being embodied in a useful machine, was explicit rather than implicit. The development of the UNIVAC was sponsored jointly by the National Bureau of Standards (acting as contracting agent for several Government agencies) and Prudential Insurance. The contracts were explicitly for technique development--NBS supporting

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[15]Forman Acton, interviewed by Richard R. Mertz, at Princeton, N.J., January 21, 1971, Smithsonian (CHP), p. 14.

[16]Derrick H. Lehmer, interviewed by Uta Merzbach, at Berkeley, Calif., October 8, 1969, Smithsonian (CHP), pp. 8-9.

development of the memory while Prudential supported development of the arithmetic unit.[17] The Air Force's support of work done by the Computer Research Corporation in the early 'fifties also seems to have been motivated more by a desire to develop techniques than by the desire actually to have hardware delivered.[18] In 1946 J. H. Curtiss told prospective computer manufacturers that, while the Army emphasized "specific problems and *ad hoc* research" in its computer projects, the Navy was willing to provide "broad support to long-range, basic research, as well as to the shorter-range type of project."[19] Nevertheless, the Army transferred half a million dollars to the National Bureau of Standards to finance long-range component research--input/output devices, memories, etc.[20] If technique development was not the sole motivation for the contract, it was often a subsidiary motivation: "Government contracts not only called for hardware, but also required a look at the state-of-the-art at the same time."[21] As a result, machines designed for military customers tended to be loaded down with

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[17] Auerbach, *op. cit.*, p. 38.

[18] Sarkissian, *op. cit.*, pp. 44-45.

[19] J. H. Curtiss, "A Review of Government Requirements and Activities in the Field of Automatic Digital Computing Machinery," in Pennsylvania University, Moore School of Electrical Engineering, *Theory and Techniques for Design of Electronic Digital Computers*, v. III, Lecture 29, August 1, 1946 (Philadelphia: University of Pennsylvania, Moore School of Electrical Engineering, Report No. 48-9, June 30, 1948), p. 29-5.

[20] *Ibid.*, p. 29-27.

[21] Sarkassian, *op. cit.*, p. 49.

"every bell and whistle you could think of"--peripheral equipment ordered not so much to use as to see what was possible.[22]

Thus, the large Cold War military budgets allowed the military to go far beyond the normal procurement that it would ordinarily undertake in peacetime. It allowed the military not only to buy the computers it needed, but also to finance experimentation on a whole technology, to support the development of an entire area of information processing technology where there was some expectation of finding military applications. In effect, the Defense Department was able to do for the nascent computer industry what NACA had done for the fledgling aircraft industry--develop the underlying technology without regard to the development of particular products which would fulfill a specific Government need. But NACA had acted with a specific Congressional mandate, while the Cold War allowed the Defense Department to act on its own.

That this was possible is due to the fact that the Cold War economy is an interestingly semi-war economy. There is the same showering of money on projects which can rationalize themselves in terms of the national defense, no matter how distant or uncertain the purported application. But it is unlike a war economy in that there is not the same sense of urgency that we emphasized in discussing the ENIAC. The Cold War allowed the financing of projects which could not have been financed either in wartime (because their payoffs were too distant) or in normal peacetime (because their military usefulness was too uncertain,

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[22]Ibid., p. 48. Peripheral equipment came to be referred to as "bells and whistles" because some of the early machines actually had bells on them to ring whenever the machine malfunctioned.

and Congress was unwilling to finance them on the basis of their civilian payoffs). The distinction between "hot" war supported research and "cold" war supported research is parallel to the distinction we made in Chapter Three between "induced" research and "programmed" research (see Chapter Three, pp. 111ff, especially p. 125). The urgency of a "hot" war "induces" support for research with a relatively immediate payoff, but which has a narrow focus (like the ENIAC project, which pioneered a revolutionary new component technology, but did so within the context of a logical design that was so narrowly focused that it was difficult to use the machine for more than one application). The more relaxed circumstances of "cold" war research allow researchers to establish a "programmed" research project, where progress will be slower but along a broader front, where the full possibilities of the project are developed as it moves along. The IAS project was a classic example. It is always difficult to gain support for programmed research projects, because the payoffs seem so distant. What is remarkable about the Cold War was that it permitted such generous support for such projects over such a long period of time. The organizational slack which develops during wartime (in the sense of the relaxation of narrow constraints imposed by the need for economic justification; see above, pp. 210-214) carries over into the Cold War period, but without the urgency of wartime.[23] The emphasis during the Cold War

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[23]Mauchly indicates the kind of organizational slack which led to the development of the ENIAC in describing Goldstine's reaction to the idea: "At Aberdeen, he told us, they might spend a million bucks to build a tank and, if they found it no good, scrap it. His attitude was: Why couldn't we do that with this? If it didn't work, scrap it. And if it did, we were in." (John W. Mauchly, quoted in John Costello, "The Little Known Creators of the Computer," Nation's Business, v. 59, no. 12 [December, 1971], p. 60).

was less on finding something which would work and more on expanding the general body of knowledge in the area. Workable solutions were ultimately expected to follow, but they were not expected to be an immediate consequence of the project. In particular, the Cold War allowed projects without definite practical results to be carried out under Defense Department sponsorship which could not be carried out by civilian agencies. Murray Geisler recalls:

> The fortunate thing about the Defense Department was that it had such a large budget that it was able to undertake these kinds of fundamental research efforts... the whole political tone of domestic research is so much stronger. Military research could take on a non-political aspect, particularly during the period when the Cold War was going on.[24]

In some cases a civilian agency was unable to get Congressional support for doing research it wanted to do, but was able to get the work supported indirectly through a military agency. For example, the National Bureau of Standards was forced to go to the Office of Naval Research to get support for its Mathematical Tables Project in the late 1940's, and to Army Ordnance for support of development of computer components. It eventually won support for the Mathematical Tables Project in the regular Commerce Department appropriation in 1948, presumably because the military usefulness of the work had been demonstrated.[25] The Cold War economy also allowed the financing of other essentially civilian endeavors on the basis of a thin military rationale (e.g., the national interstate "defense" highway system.

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[24]Murray A. Geisler, interviewed by Richard R. Mertz, at Santa Monica, Calif., February 22, 1971, Smithsonian (CHP), p. II/25.

[25]Curtiss, op. cit., pp. 29/8-9, 29/27-28.

Cold war and hot war play complementary roles. As we suggested in Chapter 3 (p.126), the breakthroughs which are made possible by the urgency of hot war can be developed and have their applications broadened during the more relaxed research that takes place during cold war. The programmed research supported during cold war is also effective in expanding the range of technical possibilities. Innovations like lasers, transistors, vacuum tubes, etc., which can be used in a wide variety of applications, are often turned up during the more fundamental research pursued during cold war. But the application of these new components to the solution of old problems is often inhibited during cold war because the urgency for a major technological breakthrough is lacking. Hot war is likely to overcome these inhibitions and induce researchers to consider technical possibilities which had lain dormant for some time. Thus, the jet engine had been invented some time before World War II, but it took the crisis atmosphere of the War actually to induce anyone to try to put it in an airplane.[26] Cold war thus provides both the underlying scientific developments and the later technological refinement, while hot war provides the essential intermediary breakthrough which connects a scientific development associated with one field with an application in another. The impact of war on technological change is significant in both its hot and cold versions.[27]

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[26] John Jewkes, David Sawers, and Richard Stillerman, The Sources of Invention, 2nd ed. (New York: Norton, 1969), pp. 262-266.

[27] Cold war, incidentally, is not a twentieth century novelty. While the term is a neologism, the phenomenon has been prominent in many periods of history, e.g., Europe during much of the sixteenth and eighteenth centuries.

CHAPTER SEVEN

THE ROLE OF PATENTS IN COMPUTATIONAL INNOVATION

One does not find, in general, that patents have played a very important role in the development of the computer industry. Brock, for example, states, "Patents play a far less important role in the computer industry than one might expect...[they] have had little overall effect on competition in the industry."[1] One finds no patent monopolies in the computer industry, no cases of firms holding key patents which allow them either to earn high royalties from other firms as licensees or to sell a product with a clear and established technical edge over the competition. Firms in the computer industry patent extensively,[2] but the patenting does not seem to have the effects that one expects patents to have. In this chapter we shall consider first what role patents actually play in the computer industry. Second, we shall consider why patents play the modest role that they do. Third, we shall consider, given the modest role which they play, why firms bother to take out patents at all. And fourth, we shall consider whether patents,

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[1]Gerald W. Brock, The U.S. Computer Industry: A Study in Market Power (Cambridge: Ballinger, 1975), p. 64.

[2]For a survey, see William V. Schelbert, "Development of the Computer Industry and Patents," I.A.P.I.P. - M.I.E. Conference, Correlation between the Protection of Industrial Property and Industrial Development, 28 September - 2 October, 1970, Endre Takats, editor (Budapest: OMKDK Technoinform, 1970), pp. 201-207.

despite not having created any monopolies, nevertheless have played their assigned role of encouraging invention and innovation.

While a large number of patents have been issued in the general field of computer technology, they seem to be either ignored by other firms in the industry or licensed freely on very moderate terms.[3] Sperry Rand offered to license firms to use the ENIAC patent, which made very broad claims--in effect covering all automatic digital electronic computers--at a royalty rate of 1½% of sales, far lower than what one would have expected if Sperry Rand were really seeking to maximize revenues from the patent.[4] T. Kite Sharpless' patent on magnetic disk and drum menory systems was also licensed on very moderate terms, with I.B.M.'s license agreement, for example, stipulating that the total royalties over the life of the patent could not exceed $400,000.[5] Taylor and Silberston, in a study of the electronics industry in Britain, found that patents are either licensed very freely or simply ignored:

> ...a large computer contains thousands of different patented components. It would be quite beyond the resources of licensing departments of equipment manufacturers to take licenses and check for possible infringements on each component patent separately, and for that reason it is common practice in electronics, more than in any other field, to license patents in large clusters, or to grant patent clearance in a defined field without specifying patents

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[3]See, e.g., Brock, op. cit., pp. 64-65.

[4]"Findings of Fact, Conclusions of Law and Order for Judgment," Honeywell vs. Sperry Rand, et al., 4-67 Civ. 183, District of Minnesota (October, 1973), p. 230.

[5]"Computer History Haunts Manufacturers," Datamation, v. 14 (August, 1968), pp. 17, 19.

> individually. This practice inevitably tends to reduce the number of license agreements concluded, and does much to explain the low rate of licensing per unit of R & D expenditure in electronics... informal (sometimes purely tacit) agreements between companies to permit mutual infringement of each other's patents...--described to us as positions of 'armed neutrality'--are not uncommon in electronics.[6]

The Taylor and Silberston remarks refer to the electronics industry as a whole, and not just to computers, but their comments suggest that computers are no exception. Indeed, the only exceptions they mention are in the television sector of the industry, where some significant disputes have developed.[7] Their study is of particular interest because it is based on extensive interviews and responses to questionnaires about the role of patents in the industry. The general impression of Taylor and Silberston concerning royalty rates is that they are set at "fair" levels, i.e., high enough to compensate a research-intensive firm for the additional cost of its research and development, but not high enough to extract any sort of monopolistic return on its patents. The general assumption seems to be that firms with large patent portfolios have invested more in research and development, and a moderate royalty charged on each patent, largely regardless of its economic significance, compensates such a firm for its extra contribution to the technological base of the industry.[8]

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[6]Christopher T. Taylor and Z. A. Silberston, The Economic Impact of the Patent System: A Study of the British Experience (Cambridge: Cambridge University Press, 1973), pp. 291-292.

[7]Ibid., pp. 298-299.

[8]Ibid., pp. 292-294.

The informality with which patents seem to be treated in the computer industry, and the diffidence which firms exhibit about enforcing their patent rights, raise the question of why patent rights seem so much less significant here than they do in, say, the copying machine industry. There are a number of reasons for this.

First, while patent infringement cases are not common in the computer industry, insofar as they are taken to court they seem to result in a higher rate of declarations of patent invalidity than is true for patents generally. The leading case, of course, is that of the ENIAC patent (#3,120,606), for which Eckert and Mauchly applied on June 26, 1947, and which, after many years of interference litigation, was finally granted on February 4, 1964.[9] The ENIAC patent, as stated above, has extremely broad claims, including such fundamental characteristics of all modern computers as program branching, storage of data in memory electronically, etc.[10] After the patent was granted, Sperry Rand, to which the patent had been assigned, asked for royalties from all major computer firms except for I.B.M., with whom Sperry Rand had signed a patent cross-licensing agreement several years before. All the firms refused, and Sperry Rand finally sued Honeywell for infringement in 1967. Honeywell counter-sued for antitrust violations and for a declaratory judgment that the patent was invalid. In October, 1973, the case was decided, and the patent was ruled invalid because the ENIAC had been in public use for more than one year prior to the date of the

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[9] Honeywell vs. Sperry Rand.

[10] Ibid., pp. 238-245.

patent application. The court also ruled that the most fundamental claims of the patent had been derived from Atanasoff's work.[11]

Nor is the ENIAC case an isolated example. There are, as I mentioned, very few patent infringement cases in the computer industry which have been litigated to a final court decision, but of those which have been, not a single one has resulted in the patent being declared valid. This statement is made on the basis of a review of all cases reported in the United States Patent Quarterly, the standard case reporter for patent cases, involving the top eight firms in the computer industry (or their predecessor companies), for the years 1950-1976.[12] In all, I found five cases, including Honeywell vs. Sperry Rand, which might be considered computer industry cases, although a couple involve basic components which might be used in a variety of electronic products.

The first case was Pierce vs. Hewlett-Packard, et al.[13] This case involved patent infringement on Patent #2,133,642 issued October 18, 1938, to George Washington Pierce for a piezo-electric crystal. The court ruled the patent invalid for double patenting material covered in an earlier patent, #1,789,496, issued in 1931.

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[11] Ibid., pp. 12-49.

[12] The top eight firms, as listed in Datamation for 1976, were IBM, Burroughs, Sperry Rand, Honeywell, Control Data, NCR, Digital Equipment, and Hewlett-Packard. See Oscar H. Rothenbuecher, "The Top 50 U.S. Companies in the Data Processing Industry," Datamation, v. 23, no. 6 (June, 1977), p. 64. I also considered cases involving smaller firms, such as Technitrol and Texas Instruments, when I noticed them.

[13] 103 USPQ 234, District of Massachusetts (1954), affirmed in 105 USPQ 50, First Circuit (1955).

The second case was Sperry Rand Corp. vs. Texas Instruments Inc.[14] This case involved patent infringement on Patent #2,530,110, issued to John R. Woodyard on November 14, 1950, and assigned to Sperry Gyroscope, which later became part of Sperry Rand, for a "nonlinear circuit device utilizing germanium"--a germanium rectifier, basically. The court ruled that the patent was invalid because the invention was the product of a group effort during World War II, and that Woodyard could not claim sole credit. The court also ruled that Texas Instruments had not infringed the patent, even if it were valid.

The third case was Technitrol Inc. vs. Aladdin Industries Inc.[15] This case involved infringement on Patent #3,155,766, issued November 3, 1964, to Technitrol for an "electrical component assemblage." The patent was ruled invalid on grounds of obviousness, the Patent Office having been unaware, in issuing the patent, of part of the prior art.

The fourth case was Honeywell vs. Sperry Rand (1973), which we have already discussed.[16] The fifth case was Technitrol Inc. vs. Control Data Corp., et al.[17] This case involved infringement of Patent #2,611,813, issued September 23, 1952, to T. Kite Sharpless and E. Stuart Eickert, Jr., and assigned to Technitrol for a "magnetic data

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[14] 133 USPQ 680, Northern District of Texas (1962), affirmed in 142 USPQ 411, Fifth Circuit (1964).

[15] 166 USPQ 4, Northern District of Illinois (1970).

[16] It is reported in 180 USPQ 673, District of Minnesota (1973), and also reprinted in U.S. Senate Subcommittee on Antitrust and Monopoly, The Industrial Reorganization Act--S. 1167 (Part 7--Computers) (Washington: G.P.O., 1974), pp. 5794-5910.

[17] 185 USPQ 801, District of Maryland (1975).

storage system"--essentially an airline seat reservation system using magnetic discs and drums. This case illustrates the difficulties encountered in seeking to patent work done on Government grants. Eickert and Sharpless had both worked on the EDVAC project at the Moore School (which, of course, was supported by a U.S. Army Ordnance contract) until 1947, when they resigned to found Technitrol. In their subsequent patent applications they sought to expand the scope of their claims to emphasize ideas that were developed after they stopped working for the Government, thus reducing the extent to which the Government could claim royalty-free licenses on their patents. The reasoning of the court was quite complex, but basically they ruled that Eickert and Sharpless, in seeking to expand the range of their claims, had made claims which they could not support, and therefore the patent was invalid.

These cases imply nothing, of course, about what the fate would be of other computer patents if they were litigated, but the fact that not a single computer patent has been vindicated in court suggests that firms must view the prospects of defending the validity of their own patents with some wariness. This general impression of an unusually high rate of invalidity in computer patents is supported by Taylor and Silberston's study of patents in the electronics industry generally in Britain:

> One explanation for the lack of impact [of patents] is that an expremely high proportion of patents in the electronics field, as elsewhere in electrical engineering, is nowadays thought to be of very suspect validity. (Estimates as high as 90 percent were cited to us.) The general level of doubtfulness

> appears to be very much higher than in the chemical field....[18]

There are a number of reasons why computer patents would have less than the usual degree of validity. First, a large number of people began doing work in the industry more or less simultaneously. We have reviewed the large number of projects which got started during and after the War in Chapter Two. With so many highly qualified people working at solving the same problems, it would be very difficult for one person to demonstrate clearly that he came up with an idea before anyone else did. In the case of core memories, for example, three different groups claim some credit for them and hold patents on them--F. W. Viehe and An Wang at Harvard, Jan Rajchman at RCA, and Jay Forrester at MIT.[19] Presper Eckert has also claimed credit for the idea.[20] Probably none of these people could successfully collect royalties on their patents, since each is probably infringing on the others. It would be very difficult to prove who had priority. The technical problems

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[18] Taylor and Silberston, _op. cit._, p. 294. This suspected 90% rate of invalidity applies to _all patents issued_ in electronics. By contrast, only 60% of all patents in industry generally which are contested in court are found to be invalid, and this rate applies only to those contested, which may be assumed to be of more suspect validity to begin with than the population of patents generally. See U.S. Senate, Subcommittee on Patents, Trademarks, and Copyrights, _American Patent System Hearings_ (Washington: G.P.O., 1956), pp. 175ff, and _idem._, _Patents, Trademarks, and Copyrights Report_ (Washington: G.P.O., 1957), p. 9.

[19] Schelbert, _op. cit._, p. 204.

[20] J. Presper Eckert, "In the Beginning and to What End," in World Computer Pioneers Conference, _Computers and Their Future_ (Llandudno, Wales: Richard Williams and Partners, Computer Specialists, July, 1970), p. 3/9.

involved in developing computers have not been so difficult that several people could not solve problems simultaneously.

A second reason for the invalidity of at least some computer patents is that computational research has often involved groups of people, and it is often difficult to say who exactly was responsible for which ideas. This was the crucial issue which led to the finding of invalidity in Sperry Rand vs. Texas Instruments, cited above (p. 232). The issue was raised (unsuccessfully) by Honeywell in attacking the ENIAC patent, claiming that some of the ideas were really the work of others on the project, such as T. Kite Sharpless, Arthur Burkes, and Robert Shaw.[21] It has also been a perennial issue in connection with who really invented stored programming--von Neumann or Eckert and Mauchly.[22] The issue also arose when the newly formed Computer Research Corporation began building its first computer for North American Aviation. Northrop, which the founders of CRC had just left, had never made much of an effort to patent its inventions,[23] but when its best computer engineers left, Northrop sought to take out patents on the MADDIDA naming as inventors only people who hadn't left.[24]

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[21]Honeywell vs. Sperry Rand, pp. 50-51.

[22]Herman H. Goldstine, The Computer from Pascal to von Neumann (Princeton: Princeton University Press, 1972), pp. 223-224, 198.

[23]Jerry Mendelson, interviewed by Henry S. Tropp, January 3, 1972, Smithsonian Institution, The National Museum of History and Technology (Computer History Project), p. 23. Interviews in this series will hereinafter be referred to as Smithsonian (CHP).

[24]Harold H. Sarkissiam, interviewed by Robina Mapstone, at Costa Mesa, Calif., December 21, 1972, Smithsonian (CHP), p. 31.

CRC successfully opposed the patent.[25]

When an invention is developed by a group of people all of whom work for the same company, to which their patent rights are assigned, it is of course not important to make an exact assignment of credit to individuals. Since the same firm receives the rights regardless of who is given credit for the invention, the assignment of credit is not an obstacle to the issuance of the patent. But if the inventors are working together on a Government project or for an organization that does not require assignments of patent rights, then assigning credit within the group becomes a more crucial issue. Even if patent rights are assigned to the firm, problems can arise if a researcher leaves his firm for another or to start on his own. It may be difficult to say whether the researcher conceived of the invention before he left, when he was still under contract to assign inventions, or after. Given the high degree of mobility among computer researchers, this could cause a significant degree of uncertainty about who has the rights to any given patent.

Third, Taylor and Silberston suggest that the very nature of the technology involved in electronic innovation makes it more difficult to prove novelty and non-obviousness than is the case with, say, chemicals.[26] Their argument is that since innovation in electronics often involves merely rearranging an unchanged set of components, it is more difficult to prove that the new arrangement is really novel and non-

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[25] Ibid.

[26] Op. cit., pp. 294-295.

obvious than is the case in other technologies where the components themselves are more likely to be new. Even where the components themselves seem unambiguously new, there remains some doubt about the validity of patents. Pye, for example, threatened to attack the validity of Western Electric's transistor patents and induced Western Electric to settle out of court for much less than they were originally asking.[27]

The lack of confidence in the validity of most computer patents is one factor, then, which accounts for the minor role which patents have played in the industry. A second important factor is that, while an imitator could not avoid taking out a license on a patent which covers the fundamental characteristics of the machine (as the ENIAC did), it is not difficult to invent around patents which cover only technical details. There are several different kinds of memory systems, for example, and a patent on any one could be easily circumvented by using another.[28] Taylor and Silberston suggest, for electronics generally, that "inventing around" is usually possible "at minimal cost and inconvenience."[29]

A third cause of the general ineffectiveness of patenting is that a rigid attempt to enforce one's patents would usually encounter retaliation. Computers are complex machines, any one of which, as noted above, uses a large number of components covered by a large number of patents

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[27] Ibid., p. 295n.

[28] Douglas J. Theis, "An Overview of Memory Technologies," Datamation, v. 24, no. 1 (January, 1978), pp. 113-131.

[29] Op. cit., p. 295.

held by a variety of different firms. No one can build an efficiently designed computer without infringing on someone's patents. If a firm sought to enforce its patents, either by charging high royalties or by withholding licenses, the injured firm could probably find some patent in its portfolio which the enforcing firm was infringing. If each firm were successful at enforcing its patent rights, and neither was willing to pay high royalties to the other, then the two would become deadlocked, with neither able to build the best computer possibly by using the patents of both firms. The possibilities of this sort of patent deadlock are illustrated by the radio industry in the 1920's. In the United States, no one could manufacture radios in the early 'twneties without infringing on someone's patents, and the result was the creation of the Radio Corporation of America, primarily as a vehicle for merging the patents held by General Electric, Westinghouse, A.T.&T., and Marconi.[30] In Britain, consolidation was not so successful, so that in the early 1930's two patent pools were still fighting it out for control of the industry, with independent manufacturers forced to choose between two sets of patents, each of which had some deficiencies in comparison with the other.[31] Taylor and Silberston suggest that the memory of this sort of stalemate has discouraged firms from pressing their patent rights too vigorously.[32] Firms recognize

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[30]William R. MacLaurin, Invention and Innovation in the Radio Industry (New York: Macmillan, 1949), ch. 6.

[31]S. G. Sturmey, "Patents and Progress in Radio," Manchester School of Economics and Social Studies, v. 28, no. 1 (January, 1960), p. 34.

[32]*Op. cit.*, p. 299.

that their technical interdependence in much the same way that classical oligopolists recognize their pricing interdependence. And because they recognize that unrestricted patent rivalry can have destructive effects analogous to those of unrestricted pricing rivalry, they adopt a "live and let live" attitude toward patent rights. Mutual tolerance in both cases depends upon some degree of equality among the major firms in market or technical "power," a condition which clearly obtains in the computer industry.[33]

A fourth factor is that imitation is in any case difficult due to the incomplete disclosure of what is usually called "know-how"--the detailed technical knowledge that can only be achieved either by doing one's own research or by having a full-scale technical assistance agreement with the innovating firm. Taylor and Silberston note:

> Almost all the electronics firms in our inquiry stressed the importance of unpatented technical information, and stated that, by comparison, patents were usually a minor protective device, whereas secret technical expertise can sometimes be an important barrier to competition in new developments.[34]

A particularly striking example of the importance of this sort of know-how in the computer industry can be found in Honeywell's efforts to develop printers and other peripheral equipment during the late 'fifties and early 'sixties. This episode is recounted in detail in the Honeywell vs. Sperry Rand case because it bore upon Honeywell's contention that it had been injured by the illegal (under the antitrust

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[33]See Brock, *op. cit.*, ch. 11.

[34]Taylor and Silberston, *op. cit.*, p. 296.

laws) 1956 agreement between Sperry Rand and I.B.M. to share technical information.[35] I.B.M. had been required, under the 1956 consent decree resolving the Justice Department's 1952 antitrust suit, to license any firm that applied (at "reasonable" royalties) to use any of I.B.M.'s tabulating machine and computer patents and applications filed prior to 1961. Thus, patent licenses were freely available to any firm in the industry on any I.B.M. equipment developed prior to 1961. I.B.M. was also required to provide all the technical information required for manufacture, but only on its old tabulating machine equipment, not on the new printers and other peripheral equipment used with its computer line. I.B.M. and Sperry Rand had been litigating a series of patent interferences for several years, primarily involving Sperry Rand's ENIAC patent and I.B.M.'s SSEC patent. They agreed to a resolution of these disputes in 1956, and also agreed to exchange detailed technical information on everything which they had developed up through September, 1956. Thus, while all firms had access to I.B.M. patent licenses, only Sperry Rand had access to the technical information behind those licenses. Honeywell claimed that this agreement amounted to an unreasonable restraint of trade against the other firms in the industry.[36]

The Honeywell vs. Sperry Rand case describes in detail the difficulties which Honeywell encountered due to its lack of expertise in peripheral equipment. Briefly, it was forced either to use equipment of its own design and manufacture (albeit making free use of patented

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[35]Honeywell vs. Sperry Rand, pp. 184-207.

[36]Ibid., pp. 158-165.

I.B.M. features), thus suffering from having equipment demonstrably inferior to that of I.B.M. and Sperry Rand, or to lease equipment from I.B.M. for re-lease to its own customers. This had the disadvantage that customers preferred to have all equipment from one manufacturer. Even this option was eliminated after Honeywell and I.B.M. signed a patent cross-licensing agreement in 1964. Eight months after the agreement was signed, I.B.M. informed Honeywell that it would no longer allow leased machines to be re-leased. Honeywell was free to buy the machines and lease them, but this imposed a greater burden on Honeywell of raising capital to purchase the machines. Moreover, I.B.M. would charge the full retail price, with no quantity discount. Even at this, I.B.M. was relatively generous. Sperry Rand refused to deal with Honeywell at all.[37]

The court decision explains why Honeywell could not use the patent specifications alone to manufacture duplicates of I.B.M. peripheral equipment:

> 15.40.105 Honeywell could not have "reversed-engineered" the IBM and SR electronic and electromechanical devices as to which IBM and SR shared information in the technological merger of 1956 and which could have been helpful to it in the period 1957 to date [1973] for the following reasons:
>
> .1 It is impossible on small parts to determine what type of metallic material was used, whether steel, brass, bronze, or some alloy;
>
> .2 It is impossible to tell from examination of parts which parts were made by the peripheral device manufacturer and which parts were purchased from vendors and, if some were purchased, it is impossible to tell the sources, purchase specifications, and inspection criteria;

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[37] *Ibid.*, pp. 186-189.

.3 It is impossible to tell by examination of the device what kinds of tooling, or machine tools, jigs and dies were used by the manufacturer of the device to produce the desired shapes and tolerances;

.4 It is impossible to decipher tolerances and adjustments of intricate electromecanical and electronic parts since the particular machine which is torn down may be in the middle, low or high position within the permissible tolerance range with respect to each part; and competitive tolerance ranges are not known and are impossible to decipher by inspection;

.5 It is impossible to determine by inspection the order of manufacture and assembly and the procedures and finishes used in the manufacture and assembly processes; and

.6 Most important, it is impossible to reproduce any device without making some unintended or intended modifications or adjustments and as soon as any such modifications or adjustment is made a part may be moved outside the permissable tolerance range since there is no way of knowing which adjustments are incidental and which are fundamental.[38]

It was only after more than a decade of effort that Honeywell developed enough of its own expertise in the manufacture of peripherals that it could compete on an equal basis (on that account) with I.B.M. and Sperry Rand. The significance of know-how is accentuated by the high rate of technological change in the industry. Honeywell had free access to full technical information on all the old tabulating machine equipment, but this equipment had been completely replaced by new peripheral equipment designed to work with high-speed computers. More generally, if technological change is rapid enough that equipment typically

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[38] *Ibid.*, pp. 201-202.

becomes obsolete before the statutory seventeen-year patent grant expires, then the last few years of patent protection become superfluous and the firm can increasingly (as technological change becomes more rapid) rely solely on the imitation lag afforded by the lack of know-how on the part of the imitator. Thus, even when patent licenses are freely available, either due to court action (as with I.B.M.) or due to the liberality of the firm, the lack of know-how on the part of the imitator acts as a formidable barrier to imitation. Because firms recognize this, they do not need to enforce their patent rights strictly.

Finally, particularly during the early years of the computer industry, the role of the Government as a predominant consumer and patron of computers may have inhibited the exploitation of patent rights. The Government generally allows work done on Government contracts to be patented, but requires that the Government be allowed a royalty-free license to use the technology. Insofar as the Government was a prominant builder and consumer of computers, this limited the return which a firm could earn on its patent portfolio if that portfolio was acquired (as many were) at Government expense. Second, firms were probably somewhat wary of exploiting Government-financed patent rights too vigorously even vis-a-vis other private firms. There may have been a sense that the Government's tolerant policy of allowing patents to be taken out by private firms on Government-financed research might end if it appeared to be abused. The Atomic Energy Act of 1954, for example, specified that all inventions resulting from A.E.C.-sponsored research would be considered as being invented by the A.E.C. The Space Act of

1958 provided that patents on inventions resulting from N.A.S.A research would ordinarily be issued to N.A.S.A., unless the Director of N.A.S.A. found that it would be in the national interest not to do so.[39] Computer firms may have seen these acts as straws in the wind. Taylor and Silberston suggest that in Britain

> Even where patents are obtained on 'basic' developments, firms in this field are likely to grant licenses on them relatively freely, for several reasons....they are likely to be Government suppliers, and to rely heavily on the Government for R & D contracts or straight subsidization of research, and the Government is unlikely to approve a restrictive licensing policy, at least toward domestic firms.[40]

In summary, then, the minor role which patents have played in the computer industry is probably due 1) to the dubious validity of many of the patents, 2) to the relative ease of "inventing around" valid patents, 3) to the technical interdependence in the industry which makes all firms subject to retaliation to an aggressive policy of patent enforcement, 4) to the importance of undisclosed "know-how" as a barrier to entry which often makes patent protection superfluous, especially given the rapid rate of technological change which makes the extended protection of the patent grant unimportant, and 5) the importance of the Government as a supporter of computational research (at least in the early years of the industry) and as a buyer of computers, and the fact that the Government might not tolerate the aggressive exploitation of publically supported research results.

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[39] Frederick M. Scherer, Otto J. Bachman, et al., Patents and the Corporation, 2nd ed. (Boston: 1959), pp. 80-81.

[40] Taylor and Silberston, op. cit., p. 339.

The weak significance of patents raises the question of why firms continue to amass patent portfolios if they plan to enforce their rights. The reasons have little to do with the intended purposes of the patent system, but they are important for the firms involved.

Partly firms continue to patent because the incentives not to patent are weak for the same reasons that the incentives to patent are weak. The rapid rate of technological change weakens the incentive to patent because it means that the useful period of protection is much shorter than the statutory period of protection. But it also means that the costs of disclosure associated with patenting are small, since an imitator would be able to imitate only for a few years before the patented technology became obsolete.

Second, patents are useful in bargaining with other firms which have patents. While firms ordinarily do not exact high royalties, one never can tell when they might change their strategy. Having one's own patent portfolio, and the power of retaliation which it implies, serves to deter others from asserting their patent rights too aggressively. Forgoing patenting would amount to unilateral disarmament. Taylor and Silberston write that "patents are used as convenient bargaining counters and aids to licensing rather than monopoly devices."[41] Paul King says, "Right now, in the computer business it's primarily for protection. You know, I've got my patents and you've got yours, so I won't bother you, and you don't bother me."[42] Patents are sometimes handy for

[41] Ibid., p. 296.

[42] Paul King, interviewed by Robina Mapstone, February 27, 1973, Smithsonian (CHP), p. 71.

defensive purposes when the firm is under attack for other reasons. In 1955, for example, Sperry Rand sued I.B.M. under the Sherman and Clayton acts for monopolizing the data processing industry. Five months later, in 1956, I.B.M. proposed a settlement of the suit to Sperry Rand, including a settlement of several patent interferences. Shortly thereafter, to press Sperry Rand to give serious consideration to their proposed settlement, I.B.M. countersued for patent infringement on 35 I.B.M. patents, mostly on card-handling equipment, and the two firms finally achieved the settlement referred to above (p. 240) in August of 1956.[43] When I.B.M. was sued by Telex for antitrust violations, it again countersued, in part for patent infringement.[44]

Finally, a patent portfolio may provide a modest income in license fees to offset part of a firm's research and development costs, but such income appears to be truly minor. Western Electric, for example, collected about $9 million in royalties on its transistor patents between 1952 and 1963,[45] while by 1965 its research costs on transistors had reached over $56 million.[46] Thus, the motivations for patenting seem to have little to do with the apparent purposes of the patent system. They are primarily defensive rather than intended to create exclusive rights to inventions. Nor do they play their intended role of

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[43] Honeywell vs. Sperry Rand, pp. 159-165.

[44] Brock, op. cit., p. 175.

[45] C. Freeman, "Research and Development in Electronic Capital Goods," National Institute Economic Review, no. 34 (November , 1965), p. 65n.

[46] John Jewkes, David Sawers, and Richard Stillerman, The Sources of Invention, 2nd ed. (New York: Norton, 1969), p. 215.

disclosing fully new inventions, because much of the important information about new inventions remains secret "know-how" and is not disclosed. Even of information that is publicized, more useful information is apparently made available in articles in technical journals than in patent applications.[47]

Notwithstanding the minor role which patents issued have played in the development of the computer, it is possible that the patent system has had a significant role in encouraging computational development, simply because the promise of patent protection might have encouraged research and development even if that promise remained unfulfilled. Eckert and Mauchly certainly seem to have been very conscious of patent protection, and its promise may have been a strong motivating influence for them. Goldstine reports that they spent most of their time for their last year or two at the Moore School working on patent applications for the ENIAC, and, while the increasing bitterness between them and Goldstine and von Neumann over credit for the key ideas in the ENIAC and EDVAC may have been motivated by professional pride, it also seems that the financial rewards to the patent protection recognizing such credit were a major factor.[48] By 1950, however, it seems likely that anyone who was familiar with the industry would have realized that, with so many competing computer development projects, it was unlikely that anyone would secure a valuable patent monopoly. The only valuable

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[47] Taylor and Silberston, op. cit., p. 303.

[48] Herman H. Goldstine, interviewed by the author, at Princeton, N.J., July 23, 1975. See also Goldstine, op. cit. (1972), pp. 220-224.

patent monopolies at the time seen to have been the I.B.M. and Remington Rand card-handling patent portfolios.[49]

In this respect, the computer industry is probably somewhat like industry generally. Scherer, in discussing the effectiveness and necessity of the patent system in encouraging invention, notes that two important factors are the size of the firms involved and the size of the market involved.[50] The smaller the firm and the smaller the market the more the firm is likely to depend upon patent protection. The small firm will not have the extensive marketing network that acts as a barrier to entry protecting the large firm. The firm acting in a small market will have a hard enough time making a profit if it dominates the market--if it suffers competition from imitators it may be unable to make a profit at all. A larger market allows more room for innovator and imitator alike to produce profitably. In the late 1940's, the computer business was characterized both by small firms and a small market, so that one would have expected patents to play a more important role in firms' research and development decisions than was the case later on in the 1950's. It was probably the growth in the market more than the growth in the size of firms that reduced the importance of patents. By

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[49] Supra, pp. 239-242, or Honeywell vs. Sperry Rand, pp. 187-207. The patents were valuable in the sense that lack of access to the patents closed off a very valuable technology. But there was little value in access to the patents alone because detailed technological information was necessary in order to manufacture the patented equipment to a satisfactory level of reliability.

[50] Frederic M. Scherer, The Economic Effects of Compulsory Patent Licensing (New York: New York University Graduate School of Business Administration Center for the Study of Financial Institutions, 1977), p. 84.

the late 1950's, it was clear that the market was growing rapidly enough that even small, inexperienced firms could do well in the business. Innovators would probably not have been inhibited significantly at that point if patent protection were suddenly withdrawn, but such a withdrawal might have inhibited them significantly in the late 1940's when the market was small and struggling and when it was not clear if small firms could compete against the giants if and when they entered the market. Patents, in short, did not have much of an effect *ex post*, but they might have had a significant effect *ex ante*.

CHAPTER EIGHT

CONCLUSION

This dissertation has been concerned with what I have described as a "radical" innovation. The computer as a case study is of interest to us because of its great economic and social impact. Innovations of such pervasive impact upon society will often be radically new technologies, so that it is of some interest to inquire into what special forces, if any, affect the development of such innovations.

I have suggested that one way in which radical innovations are different is that market forces, in general, are likely to play a less significant role in their development (Chapter Three). I have not attempted to make this argument rigorously for innovations other than the computer, but I would like to sketch some reasons here why the case of the computer might be generalizable to other cases of radical innovation.

Radical innovation generally involves the creation of new products. This is true even when the innovation itself is a "process" innovation. The distinction between product and process innovations is always somewhat vague--is the jet airplane merely a new process for providing transportation, or is it a new product because it permits travel times not previously achievable? I would argue that, even where a radical innovation is clearly a process innovation, its significance will arise as a result of new products. Thus, the Darby process for using coke in the production of iron was a new process, but its significance lay in the fact that, by lowering the cost of iron-making, it allowed the use of iron in a much wider variety of products. It was the new iron products, more than the new process for making iron, which were most significant. Even if the iron were not used in any new product, but only made old iron products cheaper, it would induce much more widespread use of the products. For our purposes, a product whose price has been cut in half as the result of a process of innovation is, in effect, a new product.[1]

The significance of the creation of a new product is that new products create new markets, and the creation of a new market is highly uncertain. This uncertainty which is associated with radical innovations will restrict the role which market forces can play. In Chapter Three, we discussed the fact that in neoclassical microeconomic theory, the entrepreneur plays the role of little more than an intelligent clerk. He has an array of information before him concerning costs, demands, etc., and he has a well defined algorithm for maximizing profits given those data. The optimal choice is neatly defined for him. The price system acts as a communications medium to bring information about market forces to him so that he can react to them, so that he can be guided by the "invisible hand." But if we are creating a new market, then he has none of these data before him on which to make

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[1]There are some exceptions. If the demand for the product is inelastic, then the effect of the innovation will simply be to cut the price of the product without stimulating greater consumption. Innovations in farm machinery fall into this category.

a decision. The product has never been produced, so that there are no cost data on it. It has never been sold, so that there are no demand data on it. The price system cannot act as a communications system, because there are no data to be communicated to him. He must imagine the data; he must guess. The more radical the innovation, the more unlike anything currently available it is, the fewer data will be available, and the less market forces can have an effect on his decision. If market forces cannot determine his decision, then other factors must do so. These other factors will be the behaviorist factors we emphasized in Chapter Three--what kind of technological experience does the potential innovator have? What firms has he worked for, and what technological commitments do they have? How severe is the pressure upon him to achieve an improvement in performance? To what extent does he select his own goals, and to what extent are these goals selected for him?

If an innovation uses a radically new process of production, then its costs will be uncertain, and this will limit the extent to which market forces can influence its development. How can the price of capital influence how capital-intensive a production process is chosen if the innovator doesn't know yet how capital-intensive the process will turn out to be? In the case of the computer, the capital-intensity of the process turned out to be much lower, after a few years, than it appeared at first (Chapter Four). If the innovation additionally involves the production of a radically new product (or if it so radically lowers the price of an old product that it opens an equally new market), then his decision to produce the product or not cannot be influenced by changes in demand, since there is nowhere where he can observe these changes in latent demand. It is in this respect that radical innovations are unlike the incremental innovations most commonly studied by economists. Even the "significant" innovations often studied develop within a well-defined market where the market demand is an observable datum (else, how could econometric studies be done relating innovation to changes in demand?--one might say that it is by definition impossible to do a study relating radical innovation to market demand, since the market demand data are by definition unavailable).

One consequence of the attenuated role of market forces in radical innovation is that established firms play a relatively modest role in the development of the technology. It is these firms which are, after all, most closely tuned in to market forces. If market forces were significant, one would expect them to play the leading role in technological development. If they do not, as was the case in computers, then it is apparent that market forces are not the predominant motivating factor. The modest role of established firms is also due to the fact that the radical innovation never falls neatly into the established business of any on-going firm. The firm may make itself over in the image of the new product (as Haloid Corporation did with Xerography), but ordinarily the established firm will feel uncomfortable with the new technology, so that the burden of developing it will fall upon new firms, established for that purpose. In the case of the computer, this burden also fell upon the academic sector--academic engineers, physicists, and mathematicians. These new firms will ordinarily come to constitute a new industry, for the radical innovation fits so awkwardly within the established industry groupings that it tends to stand permanently alone. The creation of a new industry brings with it the creation of industry institutions, such as trade press, professional organizations, perhaps even related academic disciplines, which act as media of communication within the new industry, and encourage the diffusion of technological knowledge within it. The absence of a well-defined industry in advance of the innovation, and the correspondingly underdeveloped mechanisms for diffusion of technical knowledge, partly explain the weakness of market forces in influencing the development of such innovation. Insofar as the data are available, there are no well defined communications media for communicating them (Chapter Five).

Because radical innovations are not induced by ordinary market forces, other factors must take their place. The behaviorist analysis which we developed in Chapter Three emphasized the role of a "crisis" in inducing radical technological development. Whether we describe this phenomenon as a "crisis" or, as Rosenberg does, a "focusing device," is immaterial. The event forces potential innovators out of the precincts of local, incremental research, and forces them to consider more radical possibilities.

Perhaps the archetypal example of such a "crisis" is war, and, as we argued in Chapter Six, war has several characteristics which make it a particularly suitable environment for the encouragement of radical technological change. War disrupts normal market relations which inhibit the consideration of radical technologies. It often breaks people away from their accustomed institutional connections and mixes them up with other people from other organizations with other technological backgrounds. This mixing effect may expand the range of technologies to which people give consideration. War is a crisis in the most obvious sense, and presents a host of problems, the solution of all of which seems essential. There is thus both increased pressure to solve problems and reduced inhibitions to consider radical solutions of them. In the case of the computer, mathematicians like Goldstine and von Neumann were pulled away from their ordinary theoretical pursuits and pushed into solving practical problems. Their lack of practical experience made them more open to fantastic sounding ideas, and their theoretical orientation put them in a better position to broaden the implications of the technological development after the War was over.

The military, of course, were the operating agents during the crisis which induced the development of the computer, and they continued to play a crucial role after the crisis was over. The radical nature of the computer made their role more important. Because the radical nature of the computer made the latent demand for it obscure, the assurance of military contracts was necessary in order to get the industry started, especially considering the small size and undercapitalized balance sheets of most of the first computer companies. The military also played the role in some cases of actually coaxing firms into supplying computers. The military plays a much more active role in the market than most purchasers do. Because of the unique nature of its demands, the military goes out and causes products to be made available; it does not simply choose what is offered by private firms. This activist role was sometimes necessary in order to induce firms, especially established firms in the industry, to supply computers.[2]

Finally, patents play no special role in radical innovation. If anything, the role of patents is perhaps less in the case of radical innovation than with more incremental innovation (Chapter Seven). If, as we have suggested, the fundamental uncertainty involved in radical innovation is the nature of the market which it will serve, and the performance characteristics which the innovation should have, patents will play a modest role, because these innovations are often not patentable. If, as we have suggested, the technical difficulties are not really that great, then the patents which protect the solutions to these technical difficulties will not be of great value.

Whether this sort of analysis can be extended to other cases of radical innovation must await research into those cases. There are some clear similarities that can be found. In the case of radio, for example, the market which was ultimately most significant, the broadcast market, was not guessed at when the technology was first developed. The established firms in electrical communications, like Western Union, indicated little interest in the technology, as did the established

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[2]"Well, the story goes, and I don't know whether it's apocryphal or not, but I'm sure you must have picked this up, that IBM was literally forced, kicking and screaming, to build the Defense Calculator. They didn't want to and it was the government who twisted their arm."--Jerry Mendelson, interviewed by Henry S. Tropp, January 3, 1972, Smithsonian Institution, The National Museum of History and Technology (Washington: Smithsonian Institution, 1973), p. 70. IBM was consistently reluctant to build computers until Thomas J. Watson, Jr., began to take over the company from his father.

electrical manufacturers at first. Frank Whittle, in developing the jet engine, assumed that its primary application would be high speed mail service. And certainly the War had the same effect on bringing jet engines to the stage of practicality as they had on computers. Some innovations of great economic impact may not be classifiable as "radical." The automobile, for example, might be more appropriately classified with the modern steamship as an accretion of small advances, among which no breakthroughs are visible.[3] I leave it to those who follow to determine if the concept of radical innovation is fruitful in advancing our understanding of the process by which man extends his mastery over nature.

[3] S. Colum Gilfillan, Inventing the Ship (Chicago: Follett, 1935).

APPENDIX A

THE CONCEPT OF VICINITY

We have described behaviorist search as taking place "in the vicinity" of the current technology without specifying what we mean by "vicinity." Let us consider in more detail what we mean by the idea of "vicinity" or "distance."

Suppose we begin by considering that a technology can be defined in terms of a number of parameters--it can be assigned values along a number of dimensions. This means that the technology can be mapped onto a point in n-dimensional Euclidean space, where n is the number of parameters or dimensions necessary to define the technology. If we can make the technology correspond to a point in space, then the meaning of "in the vicinity" becomes immediately clear. One technology is "in the vicinity" of another if the points corresponding to the two technologies are in the vicinity of one another in their n-dimensional space.

It is this approach which was used by Nelson, Winter, and Schuette in their model.[1] They defined a technology in terms of only two parameters, capital productivity and labor productivity. Any technology can therefore be mapped onto a point in a two-dimensional plane. To say

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[1]Nelson, Winter, and Schuette, "Technical Change in an Evolutionary Model," Quarterly Journal of Economics, v. 90, no. 1 (February, 1976), pp. 90-118.

that two technologies are "in the vicinity" of one another is simply to say that the measured distance between the points corresponding to them in the plane is short.

There is a problem buried here which Nelson et al. acknowledge but do not explore very thoroughly. The problem is that the parameters defining the technology are, in general, measured in different units. Any concept of distance in more than one dimension involves combining the distances along the various dimensions. If these distances are in disparate units (or even if they measure different things in the same units), then some weighting scheme must be invented to combine a set of disparate measures into a single measure. Many such schemes are possible, and no one scheme seems to make more sense than the others. Nelson et al. consider two weighting schemes, but offer no rationale for one making any more sense than the other.

Consider, for example, a plant-scale chemical process. Consider also a second process, identical to the first in terms of the scientific principles underlying the two, but on a laboratory scale rather than a plant scale. The two processes therefore use markedly different capital-labor ratios, though the proportions of raw materials they use are identical. Consider finally a third plant-scale process using a completely different scientific principle from the first two. Being plant-scale, we assume, its capital-labor ratio is similar to that of the first process. Since they are based upon wholly different scientific principles, however, they use quite different ratios of raw materials. The first process is therefore closer to the second process in terms of underlying scientific principles, but closer to the third in terms of capital-labor ratio. To which one is it closer? How should similarity of scientific principle and similarity of capital-labor ratio be weighted to yield overall similarity?

More particularly, consider the tabulating machine and the Harvard Mark I, considered as a pair, and the tabulating machine and the ENIAC, considered as a pair. The tabulating machine and the Mark I are similar in that both use similar components--electro-mechanical counters. The tabulating machine and the ENIAC, on the other hand, are similar in that both are programmed in the same way, by plugboards. If we ask which machine, the ENIAC or the Mark I, was more in the vicinity of the tabulator, we get no clear answer. The Mark I was in the vicinity of the tabulator insofar as its components were concerned, but the ENIAC was in the vicinity of the tabulator insofar as its programming system was concerned. Which machine we place in closer vicinity depends upon how we weight components vs. the programming system.

It seems clear then that, while behaviorist theory uses a concept of distance as if it existed independently, in fact no such independent concept of "vicinity" or "distance" exists. If we wish to use the concept, we shall have to define it ourselves; we cannot pick up a pre-existing concept. The definition, therefore, is arbitrary.

Our choice of definition in fact resolves into a choice between two basic alternatives. On the one hand, we could define the concept of distance independently, as Nelson _et al_. have done, and hope that the assumptions about distance in the behaviorist theory are still true given that definition (if they are not true, there will be a tendency to revise the definitions so as to make them true). On the other hand, we could define the concept of distance in terms of the behaviorist assumption, so that the assumption is true by definition of the concept of distance. That is, we define the distance between two technologies as the time (say) before one of them occurs to a researcher familiar with the other. With this definition of distance, search occurs in inverse order of distance (i.e., in order of vicinity) by definition.

This latter approach may appear to be cheating, but it is in fact consistent with the usual practice in scientific theorizing and saves us the kind of ex post facto fiddling with definitions referred to above as the likely consequence of the assumption's being disproved. Rather than define the concept to suit reality and hope it fits the theory, we define the concept to suit the theory and hope it fits with reality. That is, we hope that the concept of "distance" defined in terms of behaviorist theory is close enough to more commonplace concepts of distance as to make its role in the theory helpful in understanding reality. The former alternative tends to approach the latter alternative, in any case.

When we compare the definition with a definition based on weighted parameters, we find that only a few of the many conceivable parameters of a technology are relevant (deserve to be weighted) in determining its distance from other technologies. The parameters of primary interest relate to the scientific theory underlying the technology.

We consider this to be true on the assumption that we are primarily dealing with scientists and engineers (when we speak of "researchers") who view technologies and organize them in terms of their underlying scientific principles. In the case of non-scientist researchers other principles of similarity would apply. In the case of the invention of the safety razor, for example, it seems clear that the idea occurred to King Gillette not because he was familiar with scientifically related technologies, but because he was familiar with commercially related technologies, i.e., with products incorporating some disposable element which would establish a profitable long-term market for refills.

In any case a number of parameters of technologies seem to be ruled out as serious determinants of distance. Prominent among them are a number of parameters which are of significance primarily to economists (note: these characteristics of a technology are not insignificant to the question of whether or not a technology is adopted; I am only saying that they are insignificant to the question of whether or not a technology is considered, quite a different matter), such as capital-labor ratio, capital productivity, labor productivity, etc. Two technologies are unlikely to suggest one another in the researcher's mind because they have similar capital-labor ratios. It is possible, however, that technologies already sharing a common scientific basis would be more closely associated in the researcher's mind because they shared a common capital-labor ratio as well, but here capital-labor ratio would enter as a proxy for scale, and would be of minor importance, in any case, compared with the commonality of scientific principle.

Other parameters which might appear to be related to distance are in fact less so than might appear at first glance. Performance and technical riskiness, for example, suggest themselves as possible indices of distance. While it may be true that technologies offering markedly higher performance levels or degrees of technical risk are in general radically different (distant) from current technologies, the converse is in general not true. Technologies radically different from one another may offer similar levels of performance and entail similar technical risks.

Scientific relatedness, as we have seen, still cannot provide a unidimensional index of distance. The index will vary from researcher to researcher. A "hardware man," for example, will tend to weight component differences more heavily in evaluating (subconsciously) distances, so that two technologies with different component systems would be sensed as being more distant to him than to someone else. Put another way, technologies with novel component systems would be less likely to occur to him. A "software man," on the other hand, would be more likely to consider alternative components, but less likely to consider alterations to the basic organizational structure of the technology.

Insofar as individuals in a firm have common technical interests and loyalties, the firm will have a technical orientation just as individuals within it do. It will thus become meaningful to talk about the firm's evaluation of distance between two technologies. The firm will incorporate not only the technical loyalties of the technical staff, but the commercial loyalties of the sales staff. It will have a conception of "what we are in business to do" that will discourage technically related but commercially radical technologies. It was this sort of factor that led, for example, to the spin-off of the computer people from the Northrop Corporation.

Finally, an entire economy may share certain technical loyalties. Habakkuk, in his British and American Technical Change, mentions the predisposition of the British to build durable, high-quality machinery, while the Americans turned out high-volume, low quality equipment. This predisposition, like all the loyalties we have discussed, was influenced by conventional economic forces. But it survived them to exert its own independent influence.

APPENDIX B

CHRONOLOGY OF COMPUTER INSTALLATIONS AND APPLICATIONS

The table below is designed to show the pattern of computer installations, with such information as is available about their applications, from the installation of the ENIAC in 1946 to the "take-off" of the industry around 1955. The list excludes analogue computers and non-electronic computers, as well as some special purpose machines. The computers are listed in approximately chronological order, but the order should not be taken too seriously as an indication of the priority of one machine over another. The years given are the years in which the machine was installed, and usually refer to the year in which the machine was officially accepted by its purchaser. Obviously one machine might have been conceived and even built well before another, and yet listed after it here because its purchaser was more demanding in designing acceptance tests. Often machines were working properly and performing useful work well before they were officially accepted (e.g., the ENIAC was doing useful work in December, 1945, but was not officially accepted until June, 1946). Machines built for the use of the builders often went through long periods of tinkering during which they were intermittently doing useful work and being redesigned by their builders. Most of the years given here can be considered correct to within six months or so. The primary data sources for this table were:

U.S. Office of Naval Research, A Survey of Automatic Digital Computers, by N. M. Blachman (Washington: Office of Naval Research, 1953); Martin H. Weik, A Survey of Domestic Electronic Digital Computing Systems, Ballistic Research Laboratories, Report No. 971 (Aberdeen, Md.: U.S. Proving Ground, December 1955); and Idem, A Second Survey of Domestic Electronic Digital Comuting Systems, Ballistic Research Laboratories, Report No. 1010 (Aberdeen, Md.: U.S. Proving Ground, June, 1957). The table also excludes all computers built abroad. For each machine, the entry gives the name and, immediately below it, the name of the builder. If the machine was built for the builder's own use, no other organization is listed. If not, the user is listed after the builder. Applications are listed if they are known and if they are not obvious from the name of the user.

1946 ENIAC (Electronic Numerical Integrator and Computer)
Moore School of Electrical Engineering, University of Pennsylvania
U.S. Army Ordnance, Ballistics Research Laboratory, Aberdeen, Md.
firing tables and ballistics research

1949 BINAC (Binary Automatic Computer)
Eckert-Mauchly Computer Corporation, Philadelphia, Pa.
Northrop Aircraft, Snark missile project
missile guidance

1950 SEAC (Standards Eastern Automatic Computer)
U.S. National Bureau of Standards, Washington, D.C.
supported by U.S. Air Force
applied mathematics, miscellaneous scientific applications

ERA 1101
Engineering Research Associates, Minneapolis, Minn., and Arlington, Va.
"U.S. Government" (National Security Agency?)
uncertain, perhaps cryptoanalysis

Whirlwind
Massachusetts Institute of Technology, Digital Computer Laboratory
U.S. Air Force
flight simulation, air defense, aircraft tracking and landing problems

1951 Harvard Mark III (Aiken Dahlgren Electronic Calculator)
Harvard University Computation Laboratory
U.S. Navy, Bureau of Ordnance, Naval Proving Ground, Dahlgren, Va.
firing tables and ballistics research

Burroughs Laboratory Computer
Burroughs Adding Machine Co.
Wayne State University, Detroit, Mich.

UNIVAC (Universal Automatic Computer)
Remington Rand, Inc., Eckert-Mauchly Div., Philadelphia, Pa.
#1: U.S. Bureau of the Census, Washington, D.C.
census tabulations and calculations

MADDIDA (Magnetic Drum Digital Differential Analyzer)
Northrop Aircraft Co.
#1: Northrop Aircraft Co.
#2: North American Aviation
aerospace engineering computations, missile guidance

1952 IAS Computer
Institute for Advanced Study, Princeton, N.J.
sponsored by IAS, AEC, ONR, RCA, USAF, U.S. Army Ordnance, and Princeton University
meteorological computations, nuclear physics, academic research

SWAC (Standards Western Automatic Computer)
U.S. National Bureau of Standards, Institute for Numerical Analysis, Los Angeles
supported by U.S. Air Force, Wright Air Development Center, Flight Research Lab
applied mathematics

CADAC (Cambridge Digital Automatic Computer)
Computer Research Corporation, Hawthorne, Calif.
U.S. Air Force Cambridge Research Center, Project Lincoln, Cambridge, Mass.
air defense and tracking

Hughes Airborne Control Computer
Hughes Aircraft Co., Culver City, Calif.
U.S. Air Force, Wright Air Development Center, Wright Patterson AFB, Dayton, Ohio
aircraft and missile guidance and weapons control

MANIAC (Mathematical Analyzer, Numerical Integrator, and Computer)
U.S. Atomic Energy Commission, Los Alamos Scientific Laboratory
nuclear weapons research

ORDVAC (Ordnance Discrete Variable Automatic Computer)
University of Illinois
U.S. Army Ordnance, Aberdeen Proving Ground, Aberdeen, Md.
ballistics research

1952 EDVAC (Electronic Discrete Variable Automatic Computer)
Moore School of Electrical Engineering, University of Pennsylvania
U.S. Army Ordnance, Ballistics Research Laboratories, Aberdeen, Md.
ballistics tables and research

Harvard Mark IV (Harvard Magnetic Drum Calculator)
Harvard University Computation Laboratory
supported by U.S. Air Force
academic research

ILLIAC (Illinois Automatic Computer)
University of Illinois
supported by U.S. Army Ordnance, ONR, AEC, U.S. Air Force
academic research

Teleregister SPEDDH (Special Purpose Electronic Digital Data Handling)
Teleregister Corp., Stamford, Conn.
U.S. Air Force, Rome Air Development Center, Griffiss AFB, Rome, N.Y.

Magnetronic Reservisor
Teleregister Corp., Stamford, Conn.
American Airlines
special purpose airline reservation system

ELECOM 100 (Electronic Computer 100)
Underwood Corp., Electronic Computer Div.
#1: U.S. Army Ordnance, Development and Proof Services, Fire Control Branch, Aberdeen Proving Ground, Aberdeen, Md.

UNIVAC
Remington Rand, Inc., Eckert-Mauchly Div.
#2: U.S. Air Force, Office of the Air Controller, Washington, D.C.
inventory management and logistical planning
#3: U.S. Army Map Service

MADDIDA
Northrop Aircraft Co.
#3: University of Utah, Applied Physics Laboratory
#4: " " " " " "
#5: " " " " " "
#6: Arnold Engineering

1953 OMIBAC
General Electric Co., Aeronautical and Ordnance Systems Div.,
Schenectady, N.Y.
ballistics studies

CRC 105
Computer Research Corp., Hawthorne, Calif.
#1: U.S. Army Ordnance, Ballistics Research Laboratories, Aberdeen, Md.
#2: U.S. Navy Bureau of Ordnance, Washington, D.C.
#3: Lockheed Aircraft Co., Burbank, Calif.
#4: U.S. Naval Ordnance Test Station, Pasadena, Calif.
#5: U.S. Air Force, Holloman AFB, Alamogordo, N.M.

AVIDAC (Argonne's Version of the IAS Digital Automatic Computer)
Argonne National Laboratory, Lemont, Ill.
nuclear physics research

MONROBOT
Monrobot Corp., Morris Plains, N.J.
#1: Monrobot Corp., Morris Plains, N.J.
#2: " " " " "
#3: U.S. Air Force Cambridge Research Center, Cambridge, Mass., air defense res.
#4: Monroe Calculating Machine Co.
#5: U.S. Army Corps of Engineers

The Logistics Computer
Remington Rand, Inc., ERA Div.
Logistics Research Project, George Washington University
owned by the U.S. Office of Naval Research

Circle Computer
Hogan Laboratories, New York, N.Y.
#1: U.S. Atomic Energy Commission, Westinghouse Elec. Corp., Atomics Products Div.
#2: U.S. Army, ORO, Johns Hopkins University, Chevy Chase, Md.

OAREC (Office of Air Research Automatic Computer)
General Electric Co., Syracuse, N.Y.
U.S. Air Force Office of Air Research, Flight Research Lab, Computation Branch, Wright Air Development Center, Wright-Patterson AFB, Dayton, Ohio

IBM 701
International Business Machines Corp,
#1: International Business Machines Corp., New York, N.Y.
#2: Lockheed Aircraft Co., Burbank, Calif.
#3: United Aircraft Co., East Hartford, Conn.
#4: U.S. Naval Ordnance Test Station, China Lake, Calif.

1953 FLAC (Florida Automatic Computer)
U.S. Air Force Missile Test Center, Patrick AFB, Cocoa Beach, Calif.
flight test data reduction

CRC 107
Computer Research Corp., Hawthorne, Calif.
#1: U.S. Army Ordnance, White Sands Proving Ground, Las Cruces, New Mexico
#2: U.S. Navy Bureau of Aeronautics, Washington, D.C.

MINIAC
Marchant Research Inc., Oakland, Calif.

MIDAC (Michigan Digital Automatic Computer)
U.S. Air Force Willow Run Research Center, Wright Air Development Center, Ypsilanti, Mich.

CRC CADAC 102A
Computer Research Corp., Hawthorne, Calif.
#2: U.S. Army Ordnance, White Sands Proving Ground, Las Cruces, New Mexico
#3: Rand Corp., Santa Monica, Calif.
#4: U.S. Army Ordnance, White Sands Proving Ground, Las Cruces, New Mexico

CEC 36-101
Consolidated Engineering Corp., Pasadena, Calif.

Stevens Institute of Technology Digital Differential Analyzer
Stevens Institute of Technology, Experimental Towing Tank, Hoboken, N.J.
reduction of water tank test data

ABC (Automatic Binary Computer)
U.S. Air Force Cambridge Research Center, Cambridge, Mass.
air defense research

ORACLE (Oak Ridge Automatic Computer and Logical Engine)
Argonne National Laboratory, Lemont, Ill.
Oak Ridge National Laboratory, Oak Ridge, Tenn.
nuclear physics research

JAINCOMP-C (Jacobs Instrument Computer, Model C)
Jacobs Instrument Co., Bethesda, Md.
supported by U.S. Navy Bureau of Ordnance

CALDIC (California Digital Computer)
University of California, Electrical Engineering Dept., Berkeley, California
supported by U.S. Navy Office of Naval Research
classroom instruction

1953 DYSEAC (Second Standards Eastern Automatic Computer)
U.S. National Bureau of Standards, Electronic Computer Laboratory, Washington, D.C.
moved to U.S. Army Signal Corps in 1954

MINAC (Minimal Automatic Computer)
California Institute of Technology, Digital Computing Group
supported by Continental Oil Co.

RAYDAC (Raytheon Digital Automatic Computer)
Raytheon Manufacturing Co., Waltham, Mass.
U.S. Navy Bureau of Aeronautics, Naval Air Missile Test Center, Pt. Mugu, Calif.
administered by Office of Naval Research Special Devices Center

Magnefile-D
Electronics Corp. of America, Business Machines Div., Cambridge, Mass.
Bernard Altman & Co., New York, N.Y.
inventory control

ERA 1102
Remington Rand, Inc., Engineering Research Associates Div., St. Paul, Minn.
#1: Arnold Engineering Development Center, Tullahoma, Tenn. owned by U.S. Air Force, Wright Patterson AFB, Dayton, Ohio.
#2: " " " " " " " " " "
#3: " " " " " " " " " "

ERA 1103
Remington Rand, Inc., Engineering Research Associates Div., St. Paul, Minn.
#1: U.S. Army Ordnance, White Sands Proving Ground, Las Cruces, New Mexico
#2: U.S. Air Force Operations Research Office, Eglin AFB, Fla.

ALWAC (Axel Wenner-Gren Automatic Computer)
ALWEG Corp., Cologne, Germany (but built in U.S.)
#1: U.S. Navy Bureau of Ships, David Taylor Model Basin, Carderrock, Md.

UNIVAC
Remington Rand, Inc., Eckert-Mauchly Div., Philadelphia, Pa.
#4: U.S. Atomic Energy Commission, New York University
#5: U.S. Atomic Energy Commission, University of California Radiation Laboratory, Livermore, Calif.
#6: U.S. Navy Bureau of Ships, David Taylor Model Basin, Carderrock, Md.

UDEC (Unitized Digital Electronic Computer)
Burroughs Corp., Paoli, Pa.
Wayne State University, Computation Laboratory, Detroit, Mich.

1953 BAEQS
Teleregister Corp., Stamford, Conn.
Toronto Stock Exchange, Toronto, Ont.
stock quotation system

ELECOM 100
Underwood Corp., Electronic Computer Div.
#2: Reeves Instrument Corp., New York, N.Y.
owned by U.S. Navy Bureau of Aeronautics and operated by Reeves under Project Cyclone
#3: Stevens Institute of Technology, Hoboken, N.J.

1954 Bendix D-12 (copy of MADDIDA)
Bendix Aviation Corp., Computer Div., Los Angeles, Calif.
#1: Bendix Aviation Corp., Computer Div., Los Angeles, Calif.
#2: U.S. Air Force, Rome Air Development Center, Griffiss AFB, Rome, N.Y.

JOHNNIAC (John von Neumann Numerical Integrator and Computer)
Rand Corp., Santa Monica, Calif.
owned by U.S. Air Force

Magnefile-B
Electronics Corp. of America, Business Machines Div., Cambridge, Mass.
#1: Bernard Altman & Co., New York, N.Y.
#2: Harvard Business School, Boston, Mass.
inventory control

DATATRON
Electrodata Corp., affiliate of Consolidated Engineering Corp., Pasadena, Calif.
#1: Allstate Insurance Co., Skokie, Ill.
#2: California Institute of Technology, Jet Propulsion Laboratory, Pasadena, Calif.
#3: Purdue University, Statistical Laboratory, Lafayette, Ill.

ELECOM 120
Underwood Corp., Electronic Computer Div., Long Island City, N.Y.

ORDFIAC (Ordnance Fiscal and Inventory Automatic Computer)
also known as ELECOM 200
Underwood Corp., Electronic Computer Div., Long Island City, N.Y.
U.S. Army Letterkenny Ordnance Depot, Chambersburg, Pa.

NCR-CRC-102A
National Cash Register Corp., Hawthorne, Calif.
#1: Gulf Research and Development Co., Pittsburg, Pa.
#2: U.S. Naval Ordnance Test Station, Inyokern, China Lake, Calif.
#3: U.S. Naval Post Graduate School, Monterey, Calif.
#4: U.S. Army Chemical Center, Chemical and Radiological Labs, Edgewood, Md.

1954 UDEC-II (Unitized Digital Electronic Computer, Model II)
Burroughs Corp., Paoli, Pa.
Burroughs Corp., Electronic Instruments Div., Philadelphia, Pa.

NAREC (Naval Research Electronic Digital Computer)
U.S. Naval Research Laboratory, Radio Division III, Operations Research Branch, Washington, D.C.

MDP-MSI-5014 (Mountain Data Processor, Mountain Systems Inc. 5014)
Mountain Systems, Inc., Thornwood, N.Y.
Hickok Manufacturing Co., Rochester, N.Y.

IBM 650
International Business Machines Corp.
#1: U.S. National Advisory Committee on Aeronautics, Ames Aeronautical Laboratory, Moffett Field, Calif.
#2: U.S. Bonneville Power Administration, Portland, Ore.
#3: State of California, Department of Public Works, Sacramento, Calif.
#4: U.S. National Advisory Committee on Aeronautics, Lewis Flight Propulsion Laboratory, Cleveland, Ohio
#5: U.S. Army Signal Corps Engineering Laboratory, Fort Monmouth, New Jersey
#6: U.S. Naval Avionics Facility, Indianapolis, Ind.
#7: U.S. Naval Ordnance Laboratory, White Oak, Md.
#8: Aeronutronic Systems Inc., Glendale, Calif.
#9: American Telephone and Telegraph Co., New York, N.Y.
#10: Armour Research Foundation, Chicago, Ill.
#11: Battelle Memorial Institute, Columbus, Ohio
#12: Bell Aircraft Corp., Tonawanda, N.Y.
#13: Bell Telephone Laboratories, Murray Hill, N.J.
#14: Chrysler, Corp., Chrysler Engineering Computing Laboratory, Detroit, Mich.
#15: Chrysler Corp., Missile Operations, Detroit, Mich.
#16: Chrysler Corp., Plymouth Div., Detroit, Mich.
#17: Chrysler Corp., West Coast Div., Los Angeles, Calif.
#18: Clark Bros. Inc., Olean, N.Y.
#19: Continental Oil Co., Ponca City, Okla.
#20: Cook Research Laboratories, Skokie, Ill.

WISC (Wisconsin Integrally Synchronized Computer)
University of Wisconsin, Electrical Engineering Laboratory
supported by Wisconsin Alumni Research Foundation

Lincoln Memory Test Computer
Massachusetts Institute of Technology, Lincoln Laboratory
supported by U.S. Army, Navy, and Air Force

MODAC-404 (Mountain Digital Automatic Computer 404)
Mountain Systems Inc., Thornwood, N.Y.
#1: Readers Digest Association, Condensed Book Club, Pleasantville, New York
#2: Hickok Manufacturing Co., Rochester, N.Y.

1954 MSAC (Moore School Automatic Computer)
Moore School of Electrical Engineering, University of Pennsylvania
supported by U.S. Army Signal Corps Engineering Laboratories,
Fort Monmouth, N.J.

TIM-II (The Inventory Machine II)
Laboratory for Electronics, Inc., Boston, Mass.

UNIVAC
Remington Rand Inc., Eckert-Mauchly Div.
#7: Remington Rand Inc., UNIVAC Service Bureau, New York, N.Y.
#8: General Electric Co., Major Appliance Div., Louisville, Ky.
#9: Metropolitan Life Insurance Co., New York, N. Y.
#10: U.S. Air Force Material Command, Wright-Patterson AFB, Dayton, Ohio
#11: U.S. Steel Corp., National Tube Div., Pittsburg, Pa.
#12: E.I. DuPont de Neumours & Co., Wilmington, Del.
#13: U.S. Steel Corp., Gary Works, Gary, Inc.
#14: Franklin Life Insurance Co., Springfield, Ohio
#15: Westinghouse Electric Corp., Pittsburg, Pa.
#16: Pacific Mutual Life Insurance Co., Los Angeles, Calif.
#17: Sylvania Electric Products Inc., New York, N.Y.
#18: Consolidated Edison Co. of New York, Commercial Relations Dept., New York, N.Y.
#19: Consolidated Edison Co. of New York, Commercial Relations Dept., New York, N.Y.

MINIAC
Marchant Research Inc., Oakland, Calif.
#2: Atlantic Refining Co., Research and Development Dept., Point Breeze, Philadelphia, Pa.
#3: Atlantic Refining Co., Dallas, Tex.

ERA 1103
Remington Rand Inc., Engineering Research Associates Div.
#3: Ramo-Wooldridge Corp.
#4: Convair Aircraft Corp., White Sands Proving Ground, Las Cruces, New Mexico
also six others to various buyers

IBM 701
International Business Machines Corp.
#5: University of California Radiation Laboratories, Livermore, Calif.
#6: Lockheed Aircraft Co., Burbank, Calif.
#7: U.S. Weather Bureau, Washington, D.C.
#8: Boeing Aircraft Co., Wichita, Kan.
#9: Douglas Aircraft Co., El Segundo, Calif.
#10: General Motors Corp., Detroit, Mich.
#11: Glenn L. Martin Co., Baltimore, Md.
also eight others to various buyers for scientific purposes

1954 ALWAC III
Logistics Research Corp., Redondo Beach, Calif.
#1: U.S. Weather Bureau, Weather Records Center, Asheville, N.C.
#2: ALWEG Corp., Cologne, Germany
#3: Institute of Gas Technology, Chicago, Ill.
#4: Logistics Research Corp., Redondo Beach, Calif.

1955 MONROBOT-III
Monroe Laboratories, Monroe Calculating Machine Co., Morris Plains, N.J.
U.S. Air Force Cambridge Research Center Computing Laboratory, Cambridge, Mass.

IBM 702
International Business Machines Corp.
#1: U.S. Naval Aviation Supply Office, Philadelphia, Pa.
#2: Bank of America, San Francisco, Calif.
#3: Chrysler Corp., Detroit, Mich.
#4: Commonwealth Edison Co., Chicago, Ill.
#5: Ford Motor Co., Dearborn, Mich.
#6: General Electric Co., Hanford Atomic Producs Operations, Richland, Wash.
#7: Monsanto Chemical Co., St. Louis, Mo.
#8: Prudential Life Insurance Co., of North America, Newark, N.J.
also six others to various buyers for commercial purposes

NORC (Naval Ordnance Research Calculator)
International Business Machines Corp.
U.S. Naval Proving Ground, Dahlgren, Va.

WHITESAC (White Sands Computer, also called CRC 106)
National Cash Register Corp., Hawthorne, Calif.
U.S. Army Ordnance, White Sands Proving Ground, Las Cruces, N.M.

MONROBOT V (Electronic Survey Computer)
MONROBOT Laboratories, Monroe Calculating Machine Co., Morris Plains, N.J.
U.S. Army Surveying Branch, Engineering and Development Labs, Fort Belvoir, Va.

TC-1 (Telemeter Computer, Model 1)
International Telemeter Corp., Los Angeles, Calif.
pay television

NCR-CRC-102D
National Cash Register Corp., Dayton, Ohio
#1: Georgia Institute of Technology, Engineering Experiment Center, Rich Electronic Computing Center, Atlanta, Ga.
#2: Dow Chemical Co., Midland, Mich.
#3: National Cash Register Corp., Dayton, Ohio
#4: National Cash Register Corp., Hawthorne, Calif.

1955 LGP-30 (Librascope General Purpose Computer)
Librascope, Inc., Glendale, Calif.
#1: California Institute of Technology, Pasadena, Calif.
#2: General Dynamics Corp., Convair Div., Pomona, Calif.
#3: Ethyl Corp., Baton Rouge, La.

BIZMAC
Radio Corporation of America, New York, N.Y.
#1: U.S. Army Ordnance Tank-Automotive Center, Detroit, Mich.
#2: RCA Data Center, Camden, N.J.

Burroughs E-101 (Burroughs Desk Size Electronic Computer)
Burroughs Corp., Detroit, Mich.
2 prototypes

Bendix G-15
Bendix Aviation Corp., Computer Div.
#1: Bonneville Power Administration, Portland, Ore.

Mellon Institute Digital Computer
Mellon Institute of Industrial Research, University of Pittsburg

Technitrol 180
Technitrol Engineering, Philadelphia, Pa.

READIX
J. B. Rea Co., Santa Monica, Calif.
#1: U.S. Air Technical Intelligence Center, Wright-Patterson AFB, Dayton, Ohio

WEDILOG (Digital-Analogue Differential Analyzer)
Wang Laboratories, Cambridge, Mass.

IBM 704
International Business Machines Corp.
#1: University of California Radiation Laboratory, Livermore, Calif. Calif.

AN/VJQ - 2(xA - 1)
Haller Raymond and Brown, Inc., State College, Pa.

IBM 705
International Business Machines Corp.

MODAC 410 (Mountain Digital Automatic Computer)
Mountain Systems Inc., Thornwood, N.Y.
Readers Digest Association, Condensed Book Club., Pleasantville, New York

PENNSTAC (Pennsylvania State University Automatic Computer)
Pennsylvania State University, Electrical Engineering Dept., State College, Pa.

1955 DATATRON

Electrodata Corp., affiliate of Consolidated Engineering Corp., Pasadena, Calif.

#4: Southern California Cooperative Wind Tunnel, Pasadena, Calif.
#5: American Bosch Arma Corp., Arma Div., Eglin AFB, Fla.
#6: " " " " " " " " "
#7: U.S. Naval Ordnance Laboratory, Corona, Calif.
#8: Magnolia Petroleum Co., Dallas, Texas
#9: Socony-Mobil Oil Co., Paulsboro, N.J.
#10: " " " " " "
#11: Cornell Aeronautical Laboratory, Buffalo, N.Y.
#12: " " " " "
#13: U.S. Air Force, Edwards AFB, Edwards, Calif.
#14: U.S. Air Force, Wright Air Development Center, Wright-Patterson AFB, Dayton, Ohio
#15: Dow Chemical Co., Midland, Mich.
#16: U.S. National Advisory Committee for Aeronautics, Moffett Field, Calif.

ELECOM 120A

Underwood Corp., Electronic Computer Div., Long Island City, N.Y.

#2: U.S. Air Force, Rome Air Development Center, Griffiss AFB, Rome, N.Y.
#3: Westinghouse Aviation Gas Turbine Div., Kansas City, Mo.
#4: Shell Development Laboratories, Houston, Texas

NCR-CRC-102A

National Cash Register Corp., Dayton, Ohio

#5: A. V. Roe Ltd., Malton, Ontario
#6: Royal Canadian Air Force, Edmonton, Alberta
#7: U.S. Air Force, Holloman AFB, Alamogordo, N. M.
#8: " " " " " " "
#9: Polytechnic Institute of Milan, Milan, Italy
#10: U.S. Air Force, Randolph AFB, Randolph Field, Texas
#11: Sandia Corp., Albuquerque, N.M.
#12: Standard Oil Co. of California, San Francisco, Calif.
#13: National Cash Register Co., Dayton, Ohio
#14: National Cash Register Co., Hawthorne, Calif.

also nine others to various buyers.

IBM 650

International Business Machines Corp.

#21: Cornell Aeronautical Laboratory Inc., Buffalo, N.Y.
#22: Dow Chemical Co., Midland, Mich.
#23: El Paso Natural Gas, El Paso, Texas
#24: Equitable Life Insurance Co., New York
#25: General Dynamics Corp., Convair Div., San Diego, Calif.
#26: General Electric Atomic Research Laboratory, San Jose, Calif.
#27: Harrison Radiator Co., Lockport, N.Y.
#28: Indiana University, Bloomington, Ind.
#29: Lockheed Aircraft Corp., Sunnyvale, Calif.
#30: Minneapolis-Honeywell Aeronautical Div., Minneapolis, Minn.
#31: New York Central Railway, Buffalo, N.Y.

1955 IBM 650 (cont.)

#32: Ohio State University Research Center, Columbus, Ohio
#33: Olin Mathieson Chemical Co., Niagara Falls, N.Y.
#34: Allstate Insurance Co., Skokie, Ill.
#35: Atlantic Refining Co., Philadelphia, Pa.
#36: Atlantic Regining Co., Dallas, Texas
#37: Chesapeake and Potomac Telephone Co., Baltimore, Md.
#38: Iowa State College, Ames, Iowa
#39: New York University, College of Engineering, New York, N.Y.
#40: Northwestern University, Aerial Measurements Laboratory, Evanston, Ill.
#41: Pittsburg Steel Co., Pittsburg, Pa.
#42: Prudential Life Insurance Co. of North America, Newark, N.J.
#43: Republic Aviation Corp., Farmingdale, N.Y.
#44: Stanford University Computing Center, Stanford, Calif.
#45: State College of Washington, Pullman, Wash.
#46: University of California Radiation Laboratory, Livermore, Calif.
#47: University of California Radiation Laboratory, Livermore, Calif.
#48: University of Houston Computing Center, Houston, Texas
#49: University of Rochester Computing Center, Rochester, N.Y.
#50: Washington University, St. Louis, Mo.
#51: Westinghouse Atomic Power Div., Pittsburg, Pa.

also 69 others to various buyers

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