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A SOCIAL AND POLITICAL EXPLANATION OF
SOCIAL SCIENCE TRENDS: THE CASE OF
POLITICAL DEVELOPMENT RESEARCH.

Northwestern University, Ph.D., 1976
Political Science, general

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A SOCIAL AND POLITICAL EXPLANATION OF SOCIAL SCIENCE TRENDS:
THE CASE OF POLITICAL DEVELOPMENT RESEARCH

A DISSERTATION
SUBMITTED TO THE GRADUATE SCHOOL
IN PARTIAL FULFILLMENT OF THE REQUIREMENTS

for the degree

DOCTOR OF PHILOSOPHY
Field of Political Science

by

JEAN HARDISTY DOSE'

Evanston, Illinois

June, 1976

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ACKNOWLEDGMENTS

I am grateful for the advice and guidance of the chairman of my dissertation committee, Professor Donald A. Strickland, and for the support and valuable criticisms of two members of the committee, Professors Gordon Hilton and Ted Robert Gurr.

Professors Ronald Cohen, Susan Woodward, and Ibrahim Abu-Lughod, also of Northwestern University, provided crucial and timely assistance with various aspects of data collection.

A lengthy discussion with Professor Robert Hall of the University of Illinois, Chicago Circle Campus, helped me to organize my observations and led me to partially redirect my reading. Professor Irving Louis Horowitz of Livingston College, Rutgers University provided timely interest and encouragement.

Finally, I benefited from the competence of Ms. Joan Stahl, who typed the manuscript. Throughout the endeavor I was fortunate in having the support and advice of Fred Dosé.

INTRODUCTION

Historically the academic intellectual has enjoyed a unique place in American society. Protected by conventions such as tenure and academic freedom, it appears that the intellectual in the academic setting is almost completely uninhibited in the pursuit of ideas.

In reality, however, this is far from the case. It would, in fact, be more accurate to characterize the intellectual in the modern American setting as nearly totally subject to controls of various kinds. Some of these controls are obvious to even the most superficial observer, while others are subtle and intriguing because they defy measurement and often work at the level of the sub-conscious.

This dissertation will examine those factors which influence intellectual work, and will present the skeleton of a theory of why certain research areas develop popularity within the academic community, rise to prominence, and subsequently decline in importance. The central thesis examined here is that, in spite of elaborate legal and social mechanisms for the protection of intellectual freedom, the intellectual operates under the influence of social controls which direct and shape his or her research. In the social sciences this control serves to block and suppress intellectual heterodoxy. Specifically, those ideas are

blocked which are unconventional, controversial, or threatening to established values. The controlling influence which will be examined most closely here is that exercised by the funding agency, particularly in the relationship between the researcher and the research sponsor.

While this is the central interest of this dissertation, it would be theoretically non-productive to examine the relationship in isolation from a general theory of the causes of research trends. There is no widely accepted theory which can be summarized and used as an appropriate framework for this research. It is necessary, therefore, to construct at least the bare skeleton of such a general theory in order to nest the problem in a larger framework. The skeleton presented in the first five chapters of this dissertation is a synthesis of the theoretical literature from four different areas of inquiry. Specifically, these areas are 1) sociology of knowledge, 2) sociology of science, 3) philosophy of science, and 4) politics of science.

Sociology of Knowledge

The major contributors to the sociology of knowledge, Karl Marx, Karl Mannheim, Robert Merton, Talcott Parsons, and Peter Berger, represent a broad spectrum of interests; but each is concerned, generally speaking, with the relationship between knowledge and the environment or context of its development. The crucial issue is how social relations influence thought and how this affects the validity of the knowledge produced.

Karl Marx and Frederick Engels in The German Ideology (1947) identify social class or status as the principal social relation influencing thought. Social class is determined by the relation of the individual to the means of production and the individual's thought is a reflection of his position in the class structure. This thought is known as his ideology. As Curtis and Petras further explain Marx:

Ideology develops as a function of a particular social class within a predetermined structure that also has a predetermined content. Extending this principle further, Marx believed that class position can also determine an 'ideological advantage' for the power elite of any society. Thus, those who occupy positions of power in any society have an ideology that trickles down through the social structure and contaminates that of the proletarian classes. The ruinous effect of the ideology of the ruling class is heightened by the fact that economic and political power go hand in hand; to control the ideology in one realm is, by definition, to control it in the other.

(Curtis and Petras, 1970:9)

Thus, Marx's primary concern was the unmasking of ideologies; the exposing of the relationship between ideology and social class.

Karl Mannheim, in his landmark essay in Ideology and Utopia attempted to broaden the concerns which Marx and Engels addressed. For Mannheim:

The sociology of knowledge is concerned not so much with the distortions due to a deliberate effort to deceive as with the varying ways in which objects present themselves to the subject according to the differences in social settings. Thus, mental structures are inevitably differently formed in different social settings. (Mannheim, 1936:265)

Mannheim saw two forms of ideology, the specific and the general. The term "specific ideology" refers to those ideas which are used to mask the true interests of any individual or group. This type of ideology parallels that discussed by Marx. As Mannheim indicates above, this is not the defining concern of the sociology of knowledge for him. On the contrary, he is interested in what he calls "total ideology," which corresponds to the "over-all phenomenon of the social determination of ideas as represented in the Weltanschauungen of different societies and social groups." (Curtis and Petras, 1970:10) Thus, sociology of knowledge has been broadened to encompass the determinants of the individual's "world view" in any particular social, cultural, or historical setting.

After the broadening of the scope of sociology of knowledge by Mannheim, Robert Merton saw a need for the field to begin to define itself in more precise terms. He introduced a "basis of comparability among the welter of studies which have appeared in this field." (Merton, 1957:460) This "basis of comparability" is a categorization of the basic questions asked in the sociology of knowledge. It identifies five basic questions and considers the solutions proposed by five major theorists of this field: Marx, Scheler, Mannheim, Durkheim, and Sorokin. The five questions are:

- (1) Where is the existential basis of mental productions located?

- (2) What mental productions are being sociologically analyzed?
- (3) How are mental productions related to the existential basis?
- (4) Why? Manifest and latent functions imputed to these existentially conditioned mental productions.
- (5) When do the imputed relations of the existential base and knowledge obtain?
(Merton, 1957:461-462)

This paradigm brought order and structure to the study of the sociology of knowledge.

Talcott Parsons, writing at approximately the same time as Merton, criticizes the sociology of knowledge for operating with "too undifferentiated a conceptual scheme." In a paper given at the Fourth World Congress of Sociology in 1959, Parsons attempts to integrate the sociology of knowledge into his own general theory of action. Here he says:

I see the problem area ordinarily known as the sociology of knowledge as involving the interdependence and the interpenetration of what I have called the social system and the cultural system.
(Parsons, 1959:26)

These two systems are two of the four primary subsystems which form a framework for the analysis of all human action conceived as a system. Parsons believes that only through an analysis of both social and cultural systems and of their relations to each other can an adequate sociology of knowledge be developed.

Parson's contribution is twofold: he called attention to the neglected role of the cultural system as a

determinant of knowledge, and he placed the sociology of knowledge in a general theory of action. According to Parsons, the cultural system is concerned primarily with underlying "grounds of meaning," an area of explanation which he felt had been neglected in favor of the exploration of the relationship between knowledge and the social system.

Finally, Peter L. Berger moved away from the traditional approach to sociology of knowledge and proposed that theoretical gains might be expected from an integration of the approaches of social psychology and the sociology of knowledge. The concept which he proposes in order to bridge the gap between the two approaches is the concept of social identity. He notes that:

Identity, with its appropriate attachments of psychological reality, is always identified within a specific, socially constructed world. Or, as seen from the viewpoint of the individual: one identifies oneself, as one is identified by others, by being located in a common world. (Berger, 1970:378)

Historically, then, the sociology of knowledge has vacillated between narrow and broad definitions of its central concerns. A consistent awareness of the need to build theory in this field is beginning to bear fruit, although as usual in the social sciences, there are several competing theoretical directions within the field at the moment. It is hoped that this dissertation might make a small contribution toward the theory-building effort which is gaining momentum as the field begins to mature.¹

Sociology of Science

The concerns of the sociology of knowledge were originally thought to be irrelevant to the natural sciences, where methods of observation and norms of research appeared so standardized as to be impervious to the influences of societal or cultural environment. The natural sciences were thought to be a closed society unto themselves, dominated by a rigid orthodoxy which permitted no deviations and suffered no interference from outside the community. This mystique of the isolating effects of the scientific method led sociologists of knowledge away from the consideration of the natural sciences in their attempts to identify the influences of society and culture on the generation of knowledge.

Attention was first drawn to the natural sciences when it became obvious, with the events of World War II, that science has vast and far-reaching social consequences. A growing awareness of the importance of these consequences led to an increased awareness of the reciprocity of the relationship: an awareness that the social order also has important influences on science. Robert Merton (1957) deserves much of the credit for perceiving the importance of studying the social structure of the scientific community and its relations with the larger social and cultural environment. The field of study which grew out of these concerns, the sociology of science, has served a dual

purpose: to provide a better understanding of the social system which produces scientific knowledge, and to demystify the scientific community's presumed isolation and pristine rationality.

One of the central concerns to this field is the concept of rewards and the role which rewards of various kinds play in the motivation of scientists. Hagstrom (1965) has formulated three "theories" of the organization of science, which address the motivation behind scientists' efforts in the pursuit of knowledge. The first theory is called "naive individualism," and explains the pursuit of science as motivated purely by disinterested curiosity. This is the model of scientific inquiry which is presented to the young scientist as he is being socialized into his profession. In this "theory," intellectual curiosity provides the sole stimulus for research. There is no consideration of recognition or reward underlying the scientist's choice of research topic. As Hagstrom says,

Research as an activity comes to be 'natural' for them: they find it self-evident that persons should be excited by discoveries, intensely interested in the detailed working of nature, and committed to the elaboration of theories that are of no use whatever in daily life. They develop a vocabulary of motives that makes curiosity about nature and an interest in understanding it an intrinsically important component of the human personality. (Hagstrom, 1965:9)

The second theory is called "contractual theory" and sees the scientist as motivated by the same sorts of rewards as everyone else; namely status, position, and money. According to Hagstrom:

This would assert that the decisions of scientists are determined by the authorities who control these rewards--that scientists publish and work on certain topics and with certain techniques rather than others because only if they do so will they be rewarded by the higher authorities. (1965:54)

The scientist, according to this point of view, is little more than a "brain for hire" to those who can pay him the most or provide rewards that are equally attractive. Such rewards might include an elegantly equipped lab, a pied-à-terre in a foreign country in which to relax and work, money to hire research assistants, or other types of rewards. The research topic is dictated by the provider of these rewards--presumably the "authorities" as Hagstrom says--and thus the scientist is not following his individual proclivities in choosing a research topic. This "theory" does allow some leeway for the scientist to preserve self-respect (necessary because his training and socialization placed a high value on disinterested pursuit of knowledge): the scientist may be assured complete freedom to conduct the research as he chooses. It is also possible, and often seems true, that the choice of research topic on the part of the scientist working individually and on the part of the sponsor mesh so that neither is forced to compromise.

Hagstrom himself feels that neither of these theories is satisfactory and proposes a third one of his own, which he calls "information exchange theory." According to this explanation, scientists exchange "gifts" of information in return for recognition from their colleagues.

Because scientists desire recognition, they conform to the goals and norms of the scientific community. Such control reinforces and complements the socialization process in science. It is partly dependent on the socialization of persons to become sensitive to the responses of their colleagues. By rewarding conformity, this exchange system reinforces commitment to the higher goals and norms of the scientific community, and it induces flexibility with regard to specific goals and norms. The very denial by scientists of the importance of recognition as an incentive can be seen to involve commitments to higher norms, including an orientation to a scientific community extending beyond any particular collection of contemporaries. (1965:52)

Hagstrom strongly emphasizes the role of social control in his theory of the reward system in science. He maintains that conformity to scientific norms is enforced by the scientist's colleagues: work which conforms strictly to the norms receives praise and recognition; that which violates the norms is criticized or passes unnoticed. If this mechanism is to be effective in exercising social control, the scientist must be highly motivated to attain recognition from his colleagues. Hagstrom cites as evidence of this motivation the fact that scientists are intent upon publishing their work and are highly dissatisfied if they do not receive praise for it from their colleagues. He points to the demoralization which can occur when the scientist is denied this sort of recognition.

In addition to the role of rewards in the motivation of scientists, the sociology of science is concerned with communications among scientists (Fehr, 1962; Crane, 1970), the social process of scientific discovery and innovation (Merton, 1962; Mulkay, 1972), the acceptance and recognition

of scientific work (Ziman, 1968), and the historical and contemporary role of the scientist in society (Ben-David, 1971). While there is good reason to lament the underdevelopment and neglect of the sociology of science by sociologists (Merton, 1952), it is equally true that this field has made impressive strides in accumulating knowledge about the social structure of the scientific community and its relations with the larger society.²

Philosophy of Science

Although the label philosophy of science is a fairly recent adaptation, it refers to the well-established problem of the nature of the pursuit and development of science. As science becomes more inaccessible to the layman, philosophy of science also becomes more complex--thus the perceived need to demarcate it as a field of study of its own. In fact, however, it remains an ill-defined field, encompassing a wide variety of concerns. Ernest Nagel has described the breadth of the field known as philosophy of science:

The catalogue of themes often classified under that heading includes: traditional issues in the epistemology of sense perception; problems concerning the genesis, the development, and the social effects of scientific ideas; projected philosophical syntheses of specialized scientific findings, not uncommonly to support some system of religious convictions; moral evaluations of the accomplishments and the likely future fruits of the scientific enterprise; axiomatizations of various branches of theoretical inquiry; proposed justifications of inductive procedures; criteria for meaningful discourse and types of definitional techniques; the structure of scientific laws; and the status and function of theoretical ideas. (Nagel, 1960:12)

Nagel goes on to point out that this list is by no means exhaustive.

It is neither necessary nor wise to attempt to summarize the literature in the philosophy of science here.³ Only one section of that literature is relevant to this discussion--that which deals with the development of science and the ways in which that development is generalizable to the social sciences. An appropriate title for this part of philosophy of science might be "history of science."

There is as yet no general agreement about the nature of the development of science. As taught in scientific textbooks, science develops in the following way:

If science is the constellation of facts, theories, and methods collected in current texts, then scientists are the men who, successfully or not, have striven to contribute one or another element to that particular constellation. Scientific development becomes the piecemeal process by which these items have been added, singly and in combination, to the ever growing stockpile that constitutes scientific technique and knowledge. And history of science becomes the discipline that chronicles both these successive increments and the obstacles that have inhibited their accumulation.
(Kuhn, 1970:2)

This might be called "development by accumulation," or science as a process of accretion. It presents a picture of the development of science which is linear and logical, and has served as the reconstruction most suitable for use in socializing the new scientist into the ways and means of science.

Recently, however, historians of science have found this formulation awkward and embarrassing, for it forces

them to label as erroneous or unscientific all the work of scientists which was later superseded by other developments. This produces the following quandry:

If these out-of-date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge. If, on the other hand, they are to be called science, then science has included bodies of belief quite incompatible with the ones we hold today. (Kuhn, 1970:2)

Out-of-date theories which were produced by methods which for their time were as scrupulously scientific as our modern day methods simply cannot be labelled as unscientific, yet they reflect beliefs which we now consider as unacceptable.

The dilemma has been solved by the introduction of Thomas Kuhn's notion of scientific "paradigms," which are changed through scientific "revolutions." (Kuhn, 1970) According to this version of the development of science, its growth is not linear, but involves a series of plateaus interspersed among periods of relatively uneventful linear development. Each plateau is a turning point, when the nature of scientific inquiry changes through a fundamental shift in conceptualization. The former conceptualization, which dominated one period of inquiry, is superseded by a more productive conceptualization when 1) the former focus begins to prove unproductive, and 2) a new conceptualization, or paradigm, becomes available.

It was through the recognition of the differences between the natural and the social sciences--the fact that

the natural sciences seem to be relatively free of the violent disputes which are found in the social sciences-- that Kuhn developed his notion of the dominant paradigm which unites the natural science community. The significance of the paradigm is that it explains the major theoretical reorientations which have occurred within the scientific community, and also helps to explain the exceptional rate of achievement in the discovery of knowledge which the natural sciences have enjoyed.

Kuhn's interpretation of the development of science, while widely noted and discussed, is not universally accepted. Steven Toulmin makes a persuasive argument for an interpretation of science which sees its development as evolutionary. According to Toulmin:

Moving from one historical cross-section to the next, the actual ideas transmitted display neither a complete breach at any point--the idea of absolute 'scientific revolutions' involves an over-simplification--nor perfect replication, either. The change from one cross-section to the next is an evolutionary one in this sense too: that later intellectual cross-sections of a tradition reproduce the content of their immediate predecessors, as modified by those particular intellectual novelties which were selected out in the meanwhile-- in the light of the professional standards of the science of the time. (Toulmin, 1967:466)

Here, Toulmin is making an argument similar to that made earlier by Karl Popper (1963) and in fact, the subtle differences among Popper, Kuhn and Toulmin are often difficult to grasp. Popper sees the development of science as a sequence of accepted theories, each theory retaining dominance until it falls in much the same way that Kuhn's

paradigms fall. For Popper and Toulmin, however, the pursuit of what Kuhn has called "normal science," the solving of puzzles in an almost rote fashion within the confines of the dominant paradigm, is an inaccurate description of what a good scientist does in his research. Popper and Toulmin both feel that a good scientist is constantly conjecturing about new ways of looking at things, and his conjectures are constantly kept in check by the rigorous standards of the scientific community. This provides the opportunity for spectacular advancements in science, while limiting the possibility of a large degree of error.

The social sciences have adopted many of the ideas of the development of science discussed above and applied them to the history of the social sciences, but the results have been mixed. It is questionable whether this application of ideas which reflect the development of the natural science to the social sciences clarifies, or simply further obscures, our understanding of the social sciences. There is no doubt that similarities exist: analogies between the two types of science constantly come to mind. However, we must remember that there is always a temptation to associate the social sciences with the natural sciences in any way possible because of the halo effect which will then carry over to the social sciences by this association. It may prove true in the long run that an understanding of the development of the social sciences was ultimately

delayed by this attempt to fit it to the reconstructed history of the natural sciences. It is important to remember that in his formulation Kuhn himself equivocates when discussing the social sciences. While he does not actually state that the social sciences are pre-paradigmatic, he does note:

. . . it remains an open question what parts of social science have yet acquired such paradigms at all.

(Kuhn, 1970:15)

It may be a misdirected and wasteful effort to attempt to fit the social sciences to Kuhn's formulation of the history of science.⁴

Politics of Science

A close working relationship developed between the natural science community and the federal government during World War II, resulting in a new awareness on the part of scholars of 1) the role of politics in the conduct of science, and 2) the role of scientists in the conduct of politics. Consequently, the post-war period saw the emergence of the study of the politics of science, an area of inquiry modeled to some extent after the sociology of science, but concerned with societal factors influencing science rather than relationships within the community of science, the concern of the sociology of science. (Price, 1954; Dupree, 1957; Smith, 1965; and Brooks, 1968) In addition, the politics of science examines the role of

science in the larger society in which the scientific community exists--primarily the influence of the scientific community on national policymaking. (Gilpin and Wright, 1964; Lapp, 1965; Strickland, 1968; and Schooler, 1971)

Joseph Haberer (1969) attempted to draw together the two concerns of the politics of science and develop a unified theoretical framework for its study. The goal, however, proved too ambitious, and Haberer concedes in his Preface that he has been forced to stop short of his original goal: the formulation of "the theoretical foundations of the politics of science, treated as a discrete field of inquiry." (p. iii) Chapter V of this dissertation may be seen as a continuation of Haberer's effort to develop a theoretical framework for the study of relations between science and politics, although our concern rests primarily with the social sciences.

The study of the politics of science springs from an area of historical inquiry which examined the emergence of science and researched cases of social and political persecution of scientists. Historically, persecution often involved labeling a scientist's work as heresy and thus justifying its suppression. When the relation of American science to its social and political environment finally began to receive scholarly attention after World War II, the principal concern among scientists was still the control of science by politicians and bureaucrats, but

the scientists' fear was now no longer of being labeled as heretics or of being forced to recant "heretical" discoveries. Rather, natural scientists now fear a subtle form of political control, operating through the allocation of funds and facilities for research and the classification of certain research findings by the federal government. (Price, 1954:2) For as the exchange of influence between scientists and policymakers increases, there is an increasing threat to scientific autonomy, a highly valued prerogative within the scientific community. Autonomy has been considered, in modern times, one of the causes of the success of the natural sciences: the reasoning goes; if scientists are allowed to pursue scientific knowledge according to the norms of scientific research, rather than according to the dictates of society, the scientific method (unencumbered) will prove the most effective tool for the discovery of knowledge as yet known to man. Because of the high degree of concern with scientific autonomy, the politics of science has examined extensively the most flagrant example in recent years of the interference of the social and political environment on the conduct of science--the case of Germany under the National Socialists. (see Haberer, 1969)

Discussions of current relations between scientists and the federal government are an ongoing feature of such influential publications as the Bulletin of Atomic Scientists and Science. There are still those scientists,

of course, who recognize no influence on scientific activity from sources outside of the scientific community (itself a testimony to the strength of the myth of complete scientific autonomy), but increasingly scientists are aware of, and concerned about, the politics of science.

Statement of the Problem

The normative issue addressed by this dissertation is: How much freedom should the scholar be allowed in the pursuit of ideas and the conduct of research? The question has been resolved in the western capitalist countries with the consensus that intellectuals must be free to pursue ideas and to criticize, analyze, and teach without governmental censorship or restraint. Several different justifications underlie this consensus: that a healthy society needs good critics who bring up issues for debate, that censorship leads to the control of ideas, and that open debate is always more desirable than governmental suppression. The tenet central to these justifications is that individual freedoms such as freedom of speech are the most highly valued of all political prizes, and therefore must be most rigorously protected.

In the Third World, however, the question of how much individual freedom of expression and thought should be allowed is far from settled. When resources are scarce, it is a costly decision to allow the intellectual segment of the population to pursue its own interests. Not only

might the state then be deprived of talent which it badly needs to press into service for national development, but the intellectuals may be consuming resources while at the same time contributing little if anything to the national development effort. In such situations it often seems sensible to press intellectuals into service for the state until development has reached a point where the nation can "afford" to support an unencumbered academic community.

Further, where Third World countries are concerned, the issue is potentially more explosive than in the relatively stable "advanced" western countries. Criticism in the latter countries involves a great deal of rhetoric and often (if coming from the Left) advocacy of a program of rapid social change, but seldom takes the form of widespread revolutionary activism.⁵ Therefore, it is less dangerous for these governments to be tolerant of criticism because there is no genuine threat of violent overthrow. This is not the case in the Third World, where the need for social change is often so severe that the intellectual community's criticisms of the status quo tend to be radical in content and represent a formidable threat to the existing governments. Survival of the government means control of the intellectual community.⁶

On the other hand, Third World governments in the process of instituting rapid and large scale social change may find themselves equally unwilling to grant individual freedoms to intellectuals who want to criticize

their activities. These intellectuals are often seen as a counter-revolutionary force, which will disrupt and set back the efforts of the revolutionary regimes in an attempt to regain privileges enjoyed before the new reforms were instigated.

Thus, there are three separate motives for government control of intellectual work in Third World countries:

- 1) conservation of resources for national development,
- 2) protection of an unpopular and reactionary regime, and
- 3) suppression of counter-revolutionary activities. Because it is often true that one of the three situations prevails in Third World countries, western capitalist countries appear to be more liberal in their protection of the individual freedoms of the intellectual than national governments elsewhere, and, therefore, enjoy a reputation for exceptional tolerance of the right to express opinions openly.

A debate concerning the amount of freedom and self-control a scholar should have over his own work begs the question of how much freedom he is enjoying at any particular time. This question is the empirical counterpart of the normative question just discussed. Formulated to address the current American situation, it might be phrased: How much freedom does the intellectual actually enjoy in the United States today?

A strong case can be made that the intellectual enjoys almost unlimited freedom. It is general knowledge

that censorship by fiat is illegal and that the state does not exercise control over the individual's right to publish his opinions. The widespread employment of intellectuals in state-run institutions of higher education would ostensibly put the intellectual in a vulnerable position, endangering his independence from state interference: in fact it is often the case that the intellectual is better protected and enjoys more freedom in these institutions than he does in private universities and colleges. In cases where the intellectual has been contracted for a specific project and the resulting product is altered or suppressed, it is morally and legally unclear whether his rights have actually been violated. Indeed, the absence of overt suppressive mechanisms in contemporary American society strongly supports the characterization of the United States as a model of the "open" society.

State interference and control can take other forms, however, and one mechanism, effective but difficult to detect, is suppression of the intellectual by depriving him of the tools of his work, be they a well-equipped laboratory, or travel expenses related to field work. Suppression by deprivation is not always effective; the intellectual may need no special provisions for his work--we have the famous example of Robert Lowell's heading an insurance firm during the day and writing poetry at night. In certain fields, however, personnel, equipment, and supplies

of various kinds are vital to the researcher. In these cases, it is tantamount to censorship of the researcher's work to deprive him of these "means of production."

Recently it has become necessary for social scientists to use computer time and employ research assistants as the behavioral revolution has made new techniques desirable. Thus, the social sciences have become increasingly dependent on supplementary funding to pursue research once performed in the library or in the field on a very small budget. The result has been the emergence of a dependency relationship between the social sciences and the suppliers of the necessities of research, the funding agencies, which raises the question to be explored here: How much control do the funding agencies in fact exercise over the academic community, especially the social sciences? The question focuses on the social sciences because there the need for outside funding is fairly recent, and as yet little research has been done on the influence which funding agencies exert. Interpretations of this influence have been proposed often and stridently, but there is almost no systematic research on the topic. Marxists accuse the social sciences of being totally subordinate to the whims of the funding agencies and the military-industrial establishment of which they are an arm. Members of that establishment minimize the control exercised through the funding of research projects, and argue that the new

availability of funding to the social sciences has been a boon to the academic community without costing it any loss of self-control. Thus, the boundaries of the debate have been drawn: what is needed now is a body of systematic research to establish credibility for one side of the argument or the other. One goal of this dissertation is to begin to develop such a body of research.

Certain assumptions will be made in the following examination of the question. The search for influences exercised by the funding agencies within the social science community will address the product of the community--social science research--and this product will be seen as the result of many different influences which have led ultimately to the choice and execution of any particular research problem, not simply the influence of the funding agency alone. Therefore, it is necessary to give the reader some idea of what other influences act on the researcher, and what sorts of influence these other factors are likely to exercise. At this point we have returned to issues central to sociology of knowledge, sociology of science and philosophy of science.

The Theory

As yet there is no theory which explains the various influences at work on the individual scholar to determine the nature of his scholarship. The study of, first, sociology of knowledge, second, sociology of science,

third, philosophy of science, and fourth, the politics of science, outline the essential questions and identify the principal influences, but offer no widely accepted formal theory of how all the influences relate to the research product or of the relationship among the influential factors themselves. Yet each intellectual area respectively poses one principal question:

- 1) How does the scholar's ideology affect his perception of reality?
- 2) How does the system of production, diffusion, and evaluation of research affect the scholarship produced?
- 3) How has the content of past scholarship developed and what norms govern current intellectual inquiry?
- 4) How do external factors influence the content and direction of research?⁷

Each of the four questions implies a concept which could be employed as an explanatory factor in a model of the causes of the nature and direction of social science research. A useful exercise for the reader might be a presentation of each question paired with the explanatory factor which inheres in it. With this illustration, the origin of each of the major concepts becomes clear:

<u>Question</u>	<u>Concept</u>
1) How does the scholar's ideology affect his perception of reality? (sociology of knowledge)	1) Ideology
2) How does the system of production, diffusion, and evaluation of research affect the scholarship produced? (sociology of science)	2) Academic reward structure

- | | |
|--|--------------------------------|
| 3) How has the content of past scholarship developed and what norms govern current intellectual inquiry? (philosophy of science) | 3) Dominant disciplinary norms |
| 4) How do external factors influence the content and direction of research? (politics of science) | 4) Societal reward structure |

Thus, the four derived concepts are: 1) ideology, 2) dominant disciplinary norms, 3) academic reward structure, and 4) societal reward structure. Each concept represents a factor exerting influence on the scholar in his choice of research problem and direction. In applying these concepts to the study of the social sciences, is there some way in which we can look at all the concepts from a standpoint which makes the nature of their convergence more evident? Is there some fresh approach which might be applied to integrate the four concepts in one coherent framework?

A framework of this sort will be a major contribution of the following chapters. The task will be to organize the four concepts into a cohesive framework which casts social science research as the explained variable. In doing so, each concept will be presented and discussed in a separate chapter, closing with the relationship between the concept and the research it influences, and a description of ways the concept relates to other concepts in the framework.⁸ The result will be a model, in the sense of a theoretical pattern showing the inter-relation of different concepts. The model will serve to demonstrate that

the several intellectual areas are harmonizable within a larger, more integrated account.

The significance of the model is that it both organizes existing research in some meaningful fashion which makes its various parts understandable by placing them within a larger context, and provides a theoretical framework for the study of the influence of outside funding agencies on social science research. A further contribution of the model is to establish within one framework the ability to discuss both social science research itself and the persons who produce it. Thus there is no need here to separate the discussion of the history and development of ideas from the activities of the men and women who produce them.

The model might be labeled "a political sociology of social science," a somewhat cumbersome title, but one which is more accurate than the various labels which have been used for the area of inquiry addressed here: sociology of sociology, sociology of social science, or politics of social research. The title chosen is quite self-consciously not "the political sociology of social science," but rather "a political sociology of social science," for there are undoubtedly competing theoretical models which are equally viable and productive.

The Case Study

To clarify the presentation of the theory and to help in the generation of hypotheses concerning the relationship among the theoretical concepts, one particular research trend will be used. The research "trend" was chosen as the unit of analysis because it is manageable, it reflects the dynamic nature of the social scientist's research interests, and it is not so cumbersome as a research "field" nor so controversial and ill-defined as a research "paradigm." Because a "trend" is comparable to a research "sub-field," it has less inertia and stability than does a research field, and therefore it is more sensitive to the influences being examined here. In short, the trend seems the most suitable unit of analysis for these purposes.⁹

The sub-field of comparative politics known as political development serves as the case study here. Heavily sponsored by the federal government and by many private foundations, the study of political development is ideal in two respects: 1) as a trend which was heavily funded it presents an opportunity to examine thoroughly the influence which this funding exerted, and 2) since it was also subject to many other influences, it therefore serves as a "hypothesis-generating case study." (Lipjhart, 1971: 691) In other words, an examination of the trend of political development research will clarify existing hypotheses

concerning the relationship among the central concepts and lead to the discovery of new relationships.

Empirical data concerning the influence of outside funding on the research trend of political development will be collected in two ways: by interview and by mail questionnaire. Both the interview data and the mail questionnaire data will serve the hypothesis generating function.

Because the reader may not be familiar with the research trend known as political development, Chapter I will present a review of the literature of this trend and will locate the trend in the context of comparative politics, a field of political science. Each of the subsequent four chapters will discuss the literature--from both the natural sciences and the social sciences--concerning one of the four theoretical concepts identified earlier. Details of the sampling procedure, the design of the hypothesis-generating study measuring the influence of funding agencies on political development research, and analysis of the questionnaire data will be presented in Chapter VI. Chapter VII will be devoted to a summary and conclusions.

Problems of the Theory

Those who are familiar with the quantitative methodology of political science known as politometrics will note that the model presented here is "underidentified." That is, there is insufficient information in the model to permit anything beyond mere descriptive statements because

it is impossible to isolate and hold constant enough of the model's parts to determine the effect this would have on others of its parts. In brief, there are not enough exogenous variables in the model, vis-à-vis the number of endogenous variables which have been identified, to fill the information gap and make testing of the model as a whole possible. (Gurr, 1972:160-161) The problem is resolved as more information becomes available for use in the model: at this point, however, we are working with a theoretical framework in the early stages of formulation. Therefore, a scarcity of information in the model is inevitable. For this reason, the model's underidentified nature must be tolerated as the only feasible alternative to simply inserting exogenous variables without sufficient theoretical justification. As more information about the political sociology of social science becomes available, exogenous variables will undoubtedly emerge, making the model identified, and hence truly testable.

A second problem which the model faces is its non-recursive nature: that is, there are relationships of mutual causality between the central theoretical concepts of the model. Hilton and Gillies (1973) have argued that if the mutual causality occurs simultaneously, the model is invalid. However, there is no stipulation in the model that the relationships are occurring simultaneously. Nor is this implied by the case study chosen. The trend of

political development spans a ten year period, and therefore allows for a non-recursive model which, though characterized by mutual causality, does not necessitate that the relationships be simultaneous.

INTRODUCTION

Notes

¹For further reading in sociology of knowledge, see: Jacques J. Maquet, The Sociology of Knowledge (Boston: The Beacon Press, 1951); Werner Stark, The Sociology of Knowledge (Glencoe, Ill.: The Free Press, 1958); Gunter W. Remmling (ed.), Toward the Sociology of Knowledge (London: Routledge and Kegan Paul, 1973); Georges Gurvitch, The Social Frameworks of Knowledge (New York: Harper and Row, 1971); and Peter L. Berger and Thomas Luckman, The Social Construction of Reality (New York: Doubleday and Company, Inc., 1966).

²Other important books in the sociology of science are: Robert K. Merton, The Sociology of Science (Chicago: University of Chicago Press, 1973); Bernard Barber, Science and the Social Order (New York: The Free Press, 1952); Bernard Barber and Walter Hirsch (eds.), The Sociology of Science (New York: The Free Press, 1960); Norman Kaplan (ed.), Science and Society (New York: Rand McNally, 1965); Norman Storer, The Social System of Science (New York: Holt Rinehart, 1966); and Leslie Sklair, Organized Knowledge (London: Hart-Davis, MacGibbon Ltd., 1973).

³For a review of the methodological issues which characterize philosophy of science, see: Ernest Nagel, The Structure of Science (New York: Harcourt, Brace & World, Inc., 1961); Karl R. Popper, The Logic of Scientific Discovery (New York: Basic Books, Inc., 1959); Carl G. Hempel, Philosophy of Natural Science (Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1966); and Steven Toulmin, The Philosophy of Science (London: Hutchinson's University Library, 1953).

⁴For a further discussion of the issues raised here, see a collection of papers growing out of the International Colloquium in the Philosophy of Science, London, 1965; Imre Lakatos and Alan Musgrave (eds.), Criticism and the Growth of Knowledge (Cambridge: The University Press, 1970).

⁵The notable exception here is the union of workers and students in the Paris uprising of May, 1968.

⁶Another interpretation of the revolutionary attitudes of Third World intellectuals sees their own lack of real power in the society as the source of their discontent, rather than the injustices of the society as a whole.

⁷"Internal" concepts connote the accumulation of knowledge, the state of disciplinary development, the interactions of scholars and other activities within the academic community. "External" concepts connote various political and economic factors which influence the activity of scholars. (McCartney, 1965:21)

⁸A notation should be made here of an important distinction which applies to these concepts: they are theoretical concepts, which must first be reduced to observational concepts if they are to be of any value in empirical testing. (Abell, 1971:27)

⁹For a review of the literature on trends in sociological research, see McCartney. (1965:20-40)

CHAPTER I

POLITICAL DEVELOPMENT AS A SOCIAL SCIENCE RESEARCH TREND

One of the most important substantive areas in the post-World War II study of political science is the field of political development. Most often considered a sub-field of comparative politics, it is concerned with the progression of Third World governments from a traditional, through a transitional, to a modern form. The literature which falls in this category is distinct from area studies and international relations research: political development is more theoretically oriented than the former and more parochial than the latter. However, the "boundaries" of political development research are murky, and it is not unusual for a piece of literature to be included in the categories of area studies or international relations as well as political development.

The growth of the sub-field of political development was stimulated by two major international events, each occurring after the conclusion of World War II. The first was the greater involvement of the United States in the affairs of other countries, especially a new involvement in the Third World, which had previously been the domain of the colonial powers. Secondly, the break-up of the colonial empires after World War II created a

spate of newly independent nations, especially in Asia and Africa.¹ Each event created an exciting expansion in feasible research topics available to scholars of comparative politics. No longer was it necessary to confine comparative scholarship to the countries of Western Europe: the Third World was now available to U. S. scholars, who could pursue research there without the stigma of the "colonial master" role. (Wasby, 1970:486-490)

In addition to the new accessibility of decolonized or decolonizing countries, U. S. scholars were attracted by the events occurring within these countries. Theories of development, then only crudely formulated, could be embellished and tested in the Third World setting, where the "birth" of new nations created an almost experimental research situation. Liberal academics, anxious to be engaged with progressive causes, interested in promoting the growth and spread of democracy, and frustrated by the McCarthyism which characterized U. S. politics, directed their energies towards the independence and self-government movements of the Third World. More conservative academics, equally committed to self-government for Third World countries, saw the cause as an anti-Communist crusade and sought to preserve freedom in the newly independent countries. For both groups, the opening of this sub-field of research offered opportunities to develop and apply new methodological techniques which were then emerging within the disciplines of the social sciences.

Perhaps most timely of all, however, was the sharp interest in Third World countries on the part of the U. S. government, caught by its emergence as a world leader with a paucity of information about the "underdeveloped" nations. It was only natural, in this situation, for the government to turn to the academic community for assistance in "tooling up" for its new role. During World War II academic social scientists had established their credibility as helpful partners to the government in many areas, especially through the work of the Office of Strategic Services. It had been demonstrated that, ivory tower reputation aside, the academics tapped by the government for help with war-related problems performed well and "delivered the goods." (Packenham, 1973:3-22)

The result of these circumstances was the birth of a research trend. Scholarly interest in the Third World was high; research was feasible; knowledge about developing nations was severely lacking and badly needed by the government, all of which gave the generation of such knowledge high priority. Predictably, research on the subject of developing nations flourished, and in political science it took the title "political development research."

Political development research has now passed through each typical stage in the "life cycle" of a research trend. Initially it was the object of increased interest on the part of scholars and on the part of various funding sources, both government and private. An increase in research in

turn led to increased space in disciplinary journals, more new books on the topic in print, and a proliferation of professional conferences designed to advance the field. Course offerings increased as student interest rose, undoubtedly as a result of both genuine interest and as a reflection of the interest of the discipline. The trend became firmly ensconced when it began to appear as a standard sub-field to be covered by introductory texts in the discipline.²

Three possible fates befall a research trend. It may become institutionalized as an ongoing substantive area within the discipline (voting behavior, quantum mechanics, etc.). Alternatively, it may fall into disrepute and be dropped as an area of interest by the discipline (the study of national character, community power structure studies, etc.). Finally, it may shift its emphasis, change its vocabulary, and continue to exist in an adaptive form, with all the benefits of appearing as a new trend.³ It will be argued, later in this chapter, that the trend of political development research has taken the last route described above: it has changed colors and taken a new identity. First, however, a brief intellectual history of the trend will acquaint the reader with its content and the position it has occupied in the political science discipline.

Conventional Political Development Literature

The most common approach to political development contrasts tradition and modernity and perceives movement from the former to the latter as explained by social, economic, cultural, and institutional factors within societies. Modernity, a package of characteristics associated with the current epoch and best exemplified by the "advanced" western democracies, is loosely defined as a state of economic and political stability, equality, and human and political rights, measured using the nation-state as the unity of analysis. The "modern" nation-state is committed to the norms of secular rationality and is characterized by differentiation of roles and institutions. Further, it is integrated and operates at a high level of effectiveness and with some minimal efficiency in the processing of demands.⁴

In contrast to modernity is the concept of traditionalism, associated with "underdevelopment." In the traditional society, power is distributed hierarchically, with the legitimacy of a ruler normally based in heredity. Kinship ties form the basis of power sharing: the political culture is characterized by ascriptive values, little social mobility, and an unequal distribution of power and goods. Societal complexity and role and institutional differentiation are minimal relative to "modern" societies.

Dominant theories of political development project two alternative models of the progression from tradition

to modernity. In each model, political development is cast as the dependent variable: the distinction between the two models is that one represents the progression from tradition to modernity as stages of development (Rostow, 1960; Organski, 1965) and the other sees it as a reversible linear progression along a continuum (Huntington, 1965; Binder et al., 1971). Certain concepts, however, are common to both models: an understanding of the meaning and derivation of the shared concepts is the key to both the content and the underlying assumptions of political development research. Therefore, a review of the literature should begin with a discussion of central concepts.

Each of the eleven concepts to be reviewed in the following paragraphs is widely accepted in conventional political development literature: aggregated, they are the crux of the common language shared by development theorists. The concepts are diverse in origin: borrowed from political theory (democracy), borrowed from other disciplines (acculturation), or arising from the original work of political development theorists (national integration). They range from extremely general (modernization), to quite specific (effectiveness). In order to assure that the presentation of the concepts is clear to the reader, an ordering of them on some underlying principle would be desirable. However, such an ordering would be artificial, for the concepts have been adopted by the

literature by the usual process of trial and error: the principle underlying their choice has historically been their intellectual usefulness. Thus they will be presented with only one pretense to any form of ordering--that those concepts directly related are placed adjacent to each other.

The concept of the nation-state, associated with modernity, is the accepted unit of measurement in the political development literature. It is considered at once a reflection of modernity that a specific geographical area is consolidated under one national government, and also an inevitability of modern circumstances that the world be divided into these units, often despite the irrationality of the divisions in some instances. Related and equally important concepts are those of national integration and acculturation.

National integration is a concept involving several processes. First, it implies interaction or interdependence among the various segments of the nation, be these cultural, ethnic, caste, communal, economic, linguistic, religious, or regional segments (Shils, 1963:3). Second there must be a growing common agreement on the national boundaries as acceptable delimiters of the national community, with a growing feeling of common identity permeating all the groups contained within those boundaries (Emerson, 1960:102). Within the literature it is generally agreed that local loyalties must be discouraged if this

process of national integration is to progress.

Acculturation may be thought of as the psychological aspect of national integration. Leonard Doob has described this process as:

Those phenomena which result when groups of individuals having different cultures come into continuous first hand contact, with subsequent changes in the original culture patterns of either or both groups. (1960:3)

Doob describes the result of this contact as a change in psychic mobility, an increased ability to form opinions about stimuli outside of one's own direct experience. These changes are seen to be necessary for (and to some extent to define), socio-economic development and political development.

Rationalization is a concept borrowed by scholars of political development from Max Weber's analysis of modern western bureaucracies. It describes the modern political mode of increasingly impersonal interpersonal contacts, the presence of rational and secular procedures for the making of political decisions, judicial and regulatory techniques based upon a predominantly secular and impersonal system of law, and the allocation of political roles by achievement rather than by ascription.

Political culture is once again a "borrowed" (and slightly adapted) concept central to the study of political development. In their study of five western democracies, Almond and Verba (1963) identified three types of political culture: subject, parochial, and participant. Defining

political culture as "the political system as internalized in the cognitions, feelings and evaluations of the population," the authors perceive participant political culture as (one feels, both normatively and empirically) the culture internalized by the most highly developed, most modern western democracies, where members of the society tend to be explicitly oriented to the system as a whole and to both the political and administrative structures and processes. The members of the polity tend to have an "activist" orientation, to be confident in their own civic competence and to feel that their government can be affected by their actions.

While political culture is the cultural component of the modern political society, its structural component is represented by the concept of democracy. The main feature of this concept, of course, is electoral participation, presumably without unfair restrictions or intimidation. Also part of the concept, although often only tacitly included, is a notion of political stability and an orderly and lawful change of power, in which one group will yield power at the demand of the population for the ascendance of another group. Accountability of public officials to the citizens they serve, while subject to variations in structural implementation, lies at the base of the concept of democracy. Democracy is so closely associated with the notion of modernity in the literature of political

development that it is often seen as inevitable that it will eventually emerge in the modern state.

Differentiation is another concept associated with modernity in this body of literature. Both S. N. Eisenstadt (1963:99) and D. A. Rustow and R. E. Ward (1964:7) include differentiation in their inventories of the characteristics of political development. While the concept itself remains ill-defined, it generally refers to the division of the administration of government into specific and separate roles and institutions. This division contrasts with the traditional society where religious, political, familial, and judicial roles may all inhere in one figure. Differentiation of the modern sort is a reflection of the increased complexity of a modern society and arises from the secular, impersonal bureaucratic government which modern societies produce.

The concept of effectiveness represents efficient governmental processing of citizen demands, the maintenance of order in a society, and the execution of governmental decisions, and is seen as a reflection of modernity in a nation-state. Once again, the concept is ill-defined, but is usually employed in assessing the performance of the national bureaucracy of underdeveloped or developed nations.

Economic development and urbanization are two vital concepts usually associated with political systems defined

as developed. Economic development is a goal of all nations, for the obvious reason that it represents an improved standard of living for the national citizenry. Scholars of political development have proposed that it also raises the level of national integration, improves the quality of the political culture and in fact, it has been proposed as a prerequisite for the emergence of democracy (Lipset, 1959). Urbanization is associated with the industrial revolution and for this reason is considered a necessary condition for economic development. Industrialization, such as that which has occurred in the advanced western democracies, is presumed part of the path from economic underdevelopment to economic development.

Modernization is the umbrella concept which defines the process of change from traditional society to modernity, a dynamic process considered by most scholars of political development to be both unidirectional and inevitable. In short, modernization is the ascendance of modernity over tradition and it is in the context of the assumption of the desirability of modernization that the field of political development has flourished.

While these central concepts of political development were being defined and refined, the field was passing through several stages in its own internal development. The stages, 1) institutionalism, 2) determinism, 3) structural functionalism, and 4) behavioralism, all reflected

the political science discipline and served to influence that discipline's own intellectual history.

Institutionalists dominated the early post-World War II study of political development. Their approach saw the course of the underdeveloped nations as determined by the legal apparatus adopted (or imposed by the colonizing countries). That is, if the developing country adopted the legal and constitutional system of the British, the French, or the Belgians, it would soon begin to operate much as did the colonial country from which the system was borrowed. This assumption followed naturally from the dominant view at the time; that law-making was the principal activity of politics and that laws were always, or almost always executed as intended by the legislator. (Spiro, 1970:139-140). The institutional approach fell increasingly into disfavor within the political science discipline as a whole, and as of 1960 was almost totally superceded by other approaches. It proved particularly inappropriate when applied to developing nations, where constitutional forms transplanted from the west neither grew roots nor behaved predictably.⁵

Determinists, however, almost completely neglected the influence of constitutional and legal forms. Their approach, which was contemporary with the institutional approach but outlasted it by a few years, assumed that:

. . . the causal sequence of development ran from the economy to the society to the culture and finally,

to politics and government. Politics was basically only a secondary reflection of underlying economic processes.

(Sprio, 1970:142)

That assumption led to the inevitable identification of the most highly developed political systems--those found in the most highly developed economic systems. Thus, democracy, a system associated with economically developed countries (the U. S. and Western Europe) was cast as the ideal political system. Underlying the elevation of democracy in this manner were 1) the tendency on the part of scholars at that time to expect of the developing countries the same sequence of industrialization and democratization which had characterized the advanced western nations during the 19th and 20th centuries, and 2) the traumatic events of World War II, when fascism was barely defeated by the forces of democracy. The post-war Communist threat presented another form of totalitarianism to be fought: it was imperative that the new nations be saved for democracy. According to the determinists, the best assurance of the emergence of democracy was a healthy, growing economy. From the perspective of the 1970's the approach now seems ethnocentric in its condescending and self-congratulatory tone. As an approach, it fell from favor when the new nations failed to follow a course consistent with the determinists' assumptions.⁶

By 1960 both the institutionalist and the determinist approaches were out of favor in political science

and it was obvious that a new approach was needed for the study of the Third World. The most promising innovations were occurring in sociology and anthropology, where confrontation with the problems of studying "underdeveloped" areas and cultures had been a long-standing concern. Systems theory, an approach developed in sociology, proved a fertile area from which to borrow and adapt. Indeed, with the publication of the edited volume by Gabriel Almond and James S. Coleman, The Politics of the Developing Areas (1960), the adaptation was made. Almond, in his Introduction to the volume, proposed a combination of systems theory (sociology) and structuralism (anthropology) to create an approach for the study of comparative politics known as structural-functionalism. This approach is grounded in the assumption that a system which is not in collapse is in a state of equilibrium. That is, it must by necessity be performing all functions vital to its maintenance. The performance of vital functions is a necessity common to all political systems, regardless of stage of modernization. Therefore, the comparison of the performance of universal functions makes a cross-cultural theory of political development possible. Concentration on analytic rather than concrete structures allows the framework to "stretch" to encompass both advanced western and Third World societies.

The structural-functional approach studies macro-units, that is, whole societies and nations. It also

analyzes a society's related parts or sub-systems, such as government institutions, caste associations and lineages, kin groups, interest groups, or political parties (Apter and Andrian, 1968:102-103). Societal sub-systems can be fitted into the structural-functional framework by correctly identifying the functions which they fulfill for the society as a whole. The pitfall inherent in this approach lies in the identification of a society's vital functions. The seven vital functions identified by Almond have been adopted by other political scientists until they are now almost synonymous with structural-functionalism. However, Almond did not present an unambiguous set of rules for making the crucial identification of vital functions, and it is on this point that the approach currently flounders. Criticism of the approach has centered on 1) the problem of ambiguity in the rules for identification of a society's functions, and 2) the inherent conservatism of the systems framework. While structural-functionalism continues to be a fruitful framework, these criticisms have weakened its position in political science.⁷

Another promising approach was adopted by development theorists attuned to the behavioral revolution sweeping the political science discipline. The behavioral approach focuses on the individual as the source of explanatory variables: all central analytic problems are variations on the question, "What personality traits and

behavioral processes lead to political development?" Personality traits studied are 1) attitudes, 2) needs, and 3) emotions: behavioral processes studied are 1) learning, 2) socialization, 3) perception, 4) motivation, and 5) adaptation. (Apter and Andrian, 1968:91). The principal techniques of the approach are the interview, questionnaire, experiment, intensive observation of small groups, content analysis, and statistical analysis.

The popularity of the behavioral approach continues to the present time; within political science it appears to be the most promising and fruitful now available. While there are many valid criticisms of the approach, especially its tendency to encourage blind empiricism, it has made a major contribution in creating an awareness of the individual as a political actor and the role which politics itself plays in determining political development.⁸

Spanning both the structural-functional and the behavioral approaches is a group of scholars who are members of the Social Science Research Council's Committee on Comparative Politics. The series of books published by the Committee through Princeton University Press is the single most influential set of publications in political development research. Each of its seven volumes, with two exceptions, is an edited collection of essays relating a fundamental explanatory concept to political development. The third volume, Political Modernization

in Japan and Turkey (1964) presents an in-depth analysis of two specific cases. The seventh volume, Crises and Sequences in Political Development (1971) is an attempt to superimpose the historical experiences of European political development on the current political development of Third World countries in hopes of identifying crucial inconsistencies between the two. The members of the SSRC Committee on Comparative Politics shaped and dominated the study of political development during the decade of the 1960's, when the trend was at the height of its prominence within political science. Their work epitomizes the contribution made by political science to the study of new nations.⁹

The Neo-Marxist Perspective

An alternative perspective on the issues of political development is the neo-Marxist one, which casts the economic and cultural domination of the advanced western countries as the principal determinant of development in Third World countries. Just as the conventional approach to political development contrasts tradition and modernity and sees development as the movement from one to the other, the neo-Marxist perspective contrasts dependence and liberation and sees development as the transition from one to the other. During the 1960's, when the study of political development was at its peak, the neo-Marxist approach was associated with European scholarship and scholars such

as Pierre Jalée, Gunnar Myrdal, Regis Debray, J. P. Nettl, Johann Galtung, Felix Greene, Robin Blackburn, and Robin Jenkins. Some, such as Walter Rodney, Samir Amin, Ali Mazrui, and Hamza Alavi were from Third World countries. A few were American: Irving Louis Horowitz, Richard J. Barnet, Paul A. Baran, Paul M. Sweezy, and David Horowitz, among others. Subsequently, in the 1970's, many of the concepts and assumptions characteristic of the neo-Marxist perspective became widely adopted. During the 1960's, however, this perspective was the "alternative" interpretation of development.

The central concept of the dependency perspective is imperialism, an economic relationship characterized by a relationship of dominance and exploitation of Third World countries by capitalist "big power" countries motivated by the need for expanding markets. The relationship is based principally on the exchange of raw materials which originate in the Third World for manufactured products produced in the advanced industrialized countries. It is an exploitative exchange because the Third World countries are totally dependent on the advanced countries for the sale of their product--raw materials--and therefore must accept the terms of exchange which are offered to them, while the advanced countries are only minimally dependent on the Third World countries for their economic survival, in that only a fraction of their marketable goods go to these

countries. Thus, while the advanced countries are only marginally dependent on the market which lies in the Third World, the Third World is almost totally dependent on the market which lies in the advanced countries.¹⁰

The modus operandi of the advanced capitalist countries is colonialism, or more recently, neo-colonialism. Under the colonial relationship, the colonizing countries had nearly total political and economic control over the colonized countries. The result was a cultural, political, economic, and psychological dependency which often left the colony, at independence, unable to break the ties which had been forged by the mother country. (Fanon, 1963; Rodney, 1972). This situation opened the way for neo-colonialism, the relationship which exists between many advanced countries and the Third World countries within their spheres of influence. Under a neo-colonial relationship, there is no formal political domination of one country over another. Instead, the political domination is informal and depends on the loyalty of the ruling group within the Third World country to the western power which maintains and protects it through arms sales, foreign aid, and low-interest loans. Economic dependency is maintained by international price-fixing on the part of the advanced countries, who conspire to keep the price of raw materials down and to continue the Third World countries' characteristic dependence on one or two principal crops or minerals. (Amin, 1973) The

benefits to the advanced industrialized countries of the economic dependence of the underdeveloped countries is relatively unchanged from those which made colonization attractive during the 19th century. That is, inexpensive raw materials, cheap labor, and expanded markets for finished products make for profits far higher than can be obtained within the advanced countries themselves. (Barnet and Müller, 1974)

The neo-Marxist perspective not only differs in its interpretation of development, but also serves as a platform for a critique of the dominant analysis of development. Three major criticisms of the political development literature articulated by the neo-Marxist analysis are: 1) ethnocentrism, 2) neglect of the destructive effects of aid and trade relationships for Third World countries, and 3) prostitution to the goals of American foreign policy. A discussion of each criticism will both clarify the neo-Marxist analysis and demonstrate ways in which it contrasts with the dominant analysis.

Charges of ethnocentrism against prominent American scholars of political development usually point to the vulgar teleologism of their work. Though not a "neo-Marxist" himself, Herbert Spiro's critical remarks are a good example. Here he is criticising W. W. Rostow's book The Stages of Economic Growth (1960):

He assumes that presently underdeveloped countries will have to pass through the same stages in the same sequence toward the same goal as their predecessors. This approach is teleological in that it posits an

end, goal, or telos, to the process of growth. We call it 'vulgar teleologism' because of its wholly erroneous assumption that the leaders of underdeveloped countries want to achieve, as the be-all and end-all of their present activities, the kind of economies that exist in the United States or Great Britain today. Moreover, it tends to ignore that these economies are also still engaged in a process of either growth or decay. Vulgar also is its assumption that, even if this were the end goal, the road traveled toward it today has to be, or indeed can be, the same as that over which Britons and Americans traveled during the past two centuries.

(Spiro, 1970:149-150)

Viewed in the context of the 1960's, when so much of the prominent work in political development appeared, the ethnocentrism of much of that work reflects the self-satisfaction felt by American scholars as they compared the stable, reasonable effective and ostensibly egalitarian political system of the United States with other political systems throughout the world. To the neo-Marxist perspective, however, this view of the world is simply a carry-over from the colonial mentality and demonstrates that, while the U. S. may not have been a major colonial power, it shares the assumptions on which colonialism was built.

Political development scholars of the dominant approach were for the most part committed to helping the emergence of the less developed countries through the use of foreign aid, technical assistance, low-interest loans, and trade concessions, as well as military aid and supplies. Such efforts on the part of the advanced western countries were intended, from these scholars' point of view, to bring about some minimal redistribution of the world's wealth,

to protect and stabilize governments friendly to the various big powers, and to increase the pace of modernization in underdeveloped countries. Results which did not live up to these goals were often attributed to the backwardness of those being helped, and explained by the role of corruption in traditional societies or the underdeveloped nature of the national bureaucracy and its lack of sophistication in the art of economic management.

By contrast, the neo-Marxist perspective sees these efforts at nation-to-nation assistance as a clever guise for the perpetuation of the practice of imperialism and exploitation. The effect of foreign aid, for instance, is seen to reinforce the dependency relationship which exists between rich and poor countries. Almost totally determined by political considerations, foreign aid is distributed to those countries which are compliant and/or strategic to the needs of the advanced countries. The aid itself is often given in the form of credits to be spent in the donor country, a practice which primarily benefits the giver. Furthermore, such a large proportion of foreign aid is devoted to the provision of armaments that the small amount channeled to the people themselves to improve their standard of living is negligible. Thus the cycle is unbroken and the developed and underdeveloped countries move farther and farther apart.¹¹

It is not difficult to see, given the criticisms described above, how neo-Marxists might accuse establishment

political development scholars of prostituting their scholarship to the goals of American foreign policy. A. R. Dennon (1969) discusses the close collaboration between scholars and policy makers on questions of development. He makes the familiar argument that American scholars are uncritical of their government's activities, and in fact are so compatible ideologically with government officials as to serve as an arm of the government within the university. The accusation could be expected to leave most of those accused unruffled: it would be difficult for them to see support for national policies as cause for guilt or embarrassment. To the neo-Marxists, however, such support constitutes complicity in the implementation of American imperialism.¹²

The critical content of the neo-Marxist analysis, its casting of American foreign policy as imperialist and destructive of Third World development, and its implicit advocacy of revolution in developing countries elicited strong defensive reactions on the part of establishment scholars personally or structurally threatened by such criticisms and accusations. Because theirs was the analysis most widely accepted during the 1960's, the minority neo-Marxist approach was considered unconventional. It was associated with much of the criticism of American foreign policy generated by the Vietnam war and dismissed to a large extent as radical rhetoric. For this reason, scholars who took the perspective seriously were quite often extremely controversial.

The Decline of the Trend

During the early 1970's three circumstances helped to bring about a decline in conventional political development research. First, the criticisms voiced by the neo-Marxists during the 1960's became more convincing and more damning as their fine points were elaborated and continually restated. Second, the traditional approach to political development, with its assumption of a dichotomy between tradition and modernity, consistently predicted outcomes which did not occur, and failed in its analysis of political events in Third World countries. Third, the existence of an imperialist relationship between advanced and underdeveloped countries became so well documented as to be nearly undeniable. In addition, scandals such as the Watergate and the International Telephone and Telegraph (ITT) scandals in the United States gave increased credibility to the sinister accusations of the radicals of the 1960's that the U. S. government was involved in illegal foreign intervention on a scale equaled only by former colonial powers.

The result of these circumstances was that American scholars, seeing the bankruptcy of conventional research in the area of political development, recast their examination of Third World countries as the study of social change or international dependency. As Samuel Huntington points out in his article "The Change to Change:"

These new theories of political change were distinguishable from earlier approaches because of several characteristics. First, the theoretical frameworks could be utilized for the study of political changes in societies at any level of development. Second, these frameworks were either unrelated to the process of modernization or, at best, indirectly related to that process. Third, the variables and relationships which were central to the theories were primarily political in character. Fourth, the frameworks were sufficiently flexible to encompass sources of change and patterns of change in both the domestic and the international environments of the political system. Fifth, in general the theories were relatively more complex than earlier theories of political modernization and political development: they encompassed more variables and looked at the more extensive relationships among these variables.

(1971:314)

The study of social change (in comparative politics) and imperialism (in international relations) have almost entirely eclipsed the study of political development in the 1970's. Thus the trend has taken the third option outlined earlier in this chapter--it has changed its identity and faded in prominence. The trends which have replaced it still bear some of its identity, but have rid themselves of the former trend's liabilities (accusations of ethnocentricity, national chauvinism, complicity for imperialist purposes, etc.).

The decline of the political development trend was influenced by factors other than the disillusionment of the academic community with the success of its past efforts. Other influences affected its decline. The following four chapters will each examine one of the four major factors which influence the rise and decline of

research trends: the political development trend will serve as an illustrative case. In examining the influence of each major factor affecting research on the case of political development research, the reader will see clearly how such influences might actually mold the content and the direction of academic scholarship. As developed in the Introduction, the four influential factors are: 1) dominant disciplinary norms, 2) ideology, 3) the academic reward structure, and 4) the funding structure.

CHAPTER I

NOTES

¹Of course many underdeveloped countries became independent before 1945. See Pakenham (1973:12) for a categorization of countries by period of independence and area of the world.

²Several introductory texts devoting a special section to a discussion of political development research are: James C. Charlesworth (ed.), Contemporary Political Analysis (New York: The Free Press, 1967); Marian D. Irish (ed.), Political Science: Advance of the Discipline (Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1968); Stephan L. Wasby, Political Science: The Discipline and Its Dimensions: An Introduction (New York: Charles Scribner's Sons, 1970); Herbert J. Spiro, Politics as the Master Science: From Plato to Mao (New York: Harper and Row, 1970); Michael Haas and Henry S. Kariel (eds.), Approaches to the Study of Political Science (Scranton: Chandler Publishing Company, 1970).

³The reader should note here that the term "trend" is not interchangeable with the concept of a scientific "paradigm." On the contrary, presuming that we do have "paradigms" in the social sciences, the research trend might rise, flourish, and decline within one paradigm, or it might span a "revolution" in the dominant paradigm. Briefly, the trend is a substantive area of interest, which is likely to reflect the dominant methodological commitment within the discipline.

⁴In addition to these political characteristics, the concept of modernity usually includes some notion of increased industrialization and a decrease in the percentage of the population engaged in agriculture.

⁵Examples of the institutional approach are: David Apter, The Gold Coast in Transition (Princeton: Princeton University Press, 1955); Gwendolen M. Carter (ed.), African One-Party States (Ithaca, N. Y.: Cornell University Press 1962); J. D. Fage, Ghana: A Historical Interpretation (Madison: University of Wisconsin Press, 1959); and Virginia Thompson and Richard Adloff, The Emerging States of French Equatorial Africa (Stanford: Stanford University Press, 1960).

⁶Examples of the determinist approach are Seymour Martin Lipset, "Some Social Requisites of Democracy," American Political Science Review, 53 (March, 1959), 69-105; Rupert Emerson, From Empire to Nation (Boston: Beacon Press, 1960); Max F. Millikan and Donald L. M. Blackmer (eds.), The Emerging Nations (Boston: Little, Brown and Company, 1961); and Roland J. Pennock (ed.), Self-Government in Modernizing Nations (Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1964).

⁷Representative works using the structural-functional approach are: David Apter, The Politics of Modernization (Chicago: University of Chicago Press, 1965); Marion J. Levy, Jr., Modernization and the Structure of Societies (Princeton: Princeton University Press, 1966); Fred W. Riggs, Administration in Developing Countries (Boston: Houghton-Mifflin, 1964); Gabriel Almond and G. Bingham Powell, (eds.), Comparative Politics: A Developmental Approach (Boston: Little, Brown and Company, 1966), and S. N. Eisenstadt, Modernization: Protest and Change (Englewood Cliffs, N. J.: Prentice-Hall, 1966).

⁸Examples of political development literature using the behavioral approach are: David McClelland, The Achieving Society (New York: Free Press, 1967); Everett E. Hagen, On the Theory of Social Change (Homewood, Illinois: The Dorsey Press, 1962); Daniel Lerner, The Passing of Traditional Society (New York: The Free Press, 1958); Gabriel Almond and Sidney Verba, The Civic Culture (Princeton, New Jersey, Princeton University Press, 1963); and Lucian Pye, Politics, Personality, and Nation Building (New Haven: Yale University Press, 1962).

⁹The seven volumes of the SSRC Committee on Comparative Politics are: Lucian W. Pye (ed.), Communications and Political Development (1963); Joseph LaPalombara (ed.), Bureaucracy and Political Development (1963); Robert E. Ward and Dankwart A. Rustow (eds.), Political Modernization in Japan and Turkey (1964); James S. Coleman (ed.), Education and Political Development (1965); Lucian W. Pye and Sidney Verba (eds.), Political Culture and Political Development (1966); Leonard Binder, James S. Coleman, Joseph LaPalombara, Lucian W. Pye, Sidney Verba, and Myron Weiner, Crises and Sequences in Political Development (1971). All books are published by Princeton University Press, Princeton, New Jersey.

¹⁰Recent events, particularly those which surround the current international market in oil, suggest that a growing awareness of the exhaustibility of raw materials may strengthen the Third World countries' bargaining power. For an overview of the dependency perspective see: Pierre

Jalée, The Pillage of the Third World (New York: Modern Reader Paperbacks, 1968); Harry Magdoff, The Age of Imperialism (New York: Monthly Review Press, 1969); Robin Jenkins, Exploitation (London: Granada Publishing, Ltd., 1970); Johann Galtung, "Structural Theory of Imperialism," Journal of Peace Research, 2 (1971); Kenneth Boulding, Economic Imperialism (Ann Arbor, University of Michigan Press, 1972); James D. Cockcroft, André Gunder Frank, and Dale L. Johnson, Dependence and Underdevelopment (Garden City, N. Y.: Doubleday and Company, Inc., 1972); and Kenneth J. Tarbuck (ed.), Imperialism and the Accumulation of Capital (London: Allen Lane, 1972).

¹¹For support of these arguments regarding aid and trade, see: Denis Goulet and Michael Hudson, The Myth of Aid (Maryknoll, N. Y.: Orbis Books, 1971); Theresa Hayter, Aid as Imperialism (Harmondsworth: Penguin, 1971); C. R. Hensman, Rich Against Poor: The Reality of Aid (London: Allen Lane, 1971); Arghiri Emmanuel, Unequal Exchange: A Study of the Imperialism of Trade (New York: Monthly Review Press, 1972); and Felix Greene, The Enemy (New York: Vintage Books, 1971).

¹²Robert Packenham (1973) takes a position mid-way between that of the conventional scholars and that of the neo-Marxists. Packenham sees the collaboration and cooperation between scholars and policy-makers during the 1960's to have occurred as a result of their shared commitment to the liberal tradition in America. These common liberal roots led both factions to accept four assumptions regarding development: 1) change and development are easy, 2) all good things go together, 3) radicalism and revolution are bad, and 4) distributing power is more important than accumulating power. It is in the mutual acceptance of these assumptions that the two groups--scholars and policy-makers--were in closest agreement.

CHAPTER II

DOMINANT DISCIPLINARY NORMS: SCIENTIFIC AND ETHICAL VALUES

Philosophy of science, especially that area which focuses on the history of science, has demonstrated that the emergence of a scientific discovery may be partially determined by the degree to which the research underlying it conforms to the research norms dominant at the time. Thus, it is often the case that a discovery will go unnoticed if it is too far "ahead of its time" in terms of the framework of assumptions and techniques within which it was made.

An examination of the trend of political development suggests that this premise is also true of the social sciences: a research trend which is prominent within the discipline is likely to reflect the disciplinary norms of the time. This chapter will first present a summary of the values accepted by the broad base of the political science discipline during the 1950's and 1960's and will then compare these values with those reflected in political development research. The proposition underlying this exercise is that in order for research to become sufficiently prominent to be considered a research trend, it must conform to the dominant disciplinary norms.

The following discussion of the dominant norms of the political science discipline during the 1950's and 1960's will focus on two aspects of those norms; the growth of a widespread adherence to scientific values and the emergence of a set of ethical values. The former took place within the context of a shift in research orientation known as the "behavioral revolution." The emergence of a consensus concerning ethical standards resulted from several embarrassing scandals within the social sciences, nearly all relevant to political science research, and some actually involving political development research.

A note of caution is appropriate here, alerting the reader to the subtle deference between research which is considered deviant and that which is labelled innovative. There are no standard rules to predict which reaction a given piece of research might elicit from the disciplinary community. History seems to point to the existence of a rule of thumb similar to the principle of "ripeness" apropos legal judgements. That is, research may be considered deviant at a point in time when the community as a whole is not able to accept it, yet at another time, when the community's values have moved closer to the position taken by the research, it may be labelled innovative and may then take a prominent position within the hierarchy of respected research: Examples of both judgements appear in the case of political development research: in one instance a new

technique proposed by a researcher working in the sub-field of political development may prove a significant innovation within political science; in another a variation on standard practice may be considered deviant, unethical, or simply shabby research practice.

Logical Positivism

While early political science was dominated by normative theorizing and descriptive research--some of which was empirical descriptive research--the 1950's witnessed the rise of a new form of political science research, loosely identified by an ill-defined term, "the behavioral revolution." Although this new research thrust was a distinct departure from traditional research, it is often argued that it was a logical outgrowth of that research, in that both behavioral and traditional research are within the epistemological mode known as logical positivism. Stemming from the philosophical work of David Hume and the sociological methodology of Max Weber, logical positivism prescribes objectivity in social research. Objectivity is achieved by separating the untestable value component of the researcher's observations from the testable factual observations. Factual statements are, to a greater or lesser degree, capable of empirical verification, the verification process determining the quality and meaningfulness of the empirical observations.

The logical positivist may be defined as a disciplined thinker who applies the principles of either deductive or inductive reasoning to his observations, rigorously excluding personal value presuppositions. While logical positivism was definitely the dominant mode of research in political science during the decades of the 1950's and 1960's, there was a sizable and vocal minority who saw in it a threat to the morality of political science research. A brief discussion of the minority view may serve to clarify the dominant views on objectivity. The minority view is also important for the contribution it made in modifying the commitment to objectivity which lies at the heart of logical positivism. Few but the most extreme of logical positivists in political science are not now more realistic in their claim of objectivity, largely as a result of the influence of this minority clique.

The most common criticism of logical positivism is that, because every researcher is influenced by his own cultural milieu, true objectivity is not possible: therefore, to claim objectivity is to deceive the audience, which will then fail to detect the researcher's underlying cultural values. (Rothbard, 1960:173) One danger in this deception, so the critique goes, lies in the fact that more and more social scientists are currently being asked to fill the role of social technician, partially because they are thought to have the tools necessary for objective

social analysis. If objectivity is not possible in the social sciences, then social scientists can be seen as misrepresenting themselves, but almost more importantly, they can be seen as misrepresenting their product.

Yet another critique of the logical-positivists' claim to objectivity in social research comes from those who feel that such a claim fails to point out the role that values play in the choice of research questions. On this point, Gunnar Myrdal has said:

The questions express our interest in the matter. The interests can never be purely scientific. They are choices, the products of our valuations . . . the factual analysis cannot be carried out except when guided by the value premise.

(Myrdal, 1958:51-52)

Myrdal is stating that values affect the very direction of research. To deny this or to fail to deal with it, Myrdal feels, is no less flagrant an oversight than to deny the role that values play in the factual analysis itself. As mentioned earlier, a greater appreciation of this position now pervades political science, and is accepted to at least some extent by even the most disgruntled logical positivists. This modification in the claim of objectivity occurred during the 1960's and is an example of a shift in a dominant disciplinary norm.

The Behavioral Revolution

A shift in methodological approach also under the rubric of logical positivism, but representing a more

radical shift within that mode, was the behavioral revolution or the ascendancy of the methodology of behavioralism. This shift is so central to the development of political science that it is discussed at length in various accounts of its history.¹ Robert Dahl, in his famous speech at the 1961 International Political Science Association convention, declared:

Where will the behavioral mood go from here? I think it will gradually disappear. By this I mean only that it will slowly decay as a distinctive mood and outlook. For it will become, and in fact already is becoming, incorporated into the main body of the discipline. The behavioral mood will not disappear, then, because it has failed. It will disappear rather because it has succeeded. As a separate, somewhat sectarian, slightly factional outlook, it will be the first victim of its own triumph.

(Dahl, 1961:770)

This is not to say, by any means, that the behavioral mode was not hotly debated before receiving the sort of acceptance Dahl postulated. As the literature concerning behavioralism suggests, it has often and still is under strong "counter-revolutionary" pressure.² The substance of the critique of behavioralism will be discussed later in this chapter.

Historically, the behavioral persuasion was a protest movement, adopted by those who were dissatisfied with the achievements of "traditional" political science. "Traditional" political science had originally concentrated on the historical analysis of institutions and their development, and subsequently changed its emphasis to the description and evaluation of current institutions. The early

behaviorlists, as early as 1925, had a vision of the eventual union of all the social sciences under the scientific method, and sought to achieve that end by committing political science to behavioral methodology. For reasons which Dahl discusses in his analysis of the behavioral movement, the new mode failed to be attractive to the discipline until after World War II. (Dahl, 1961) A complete transformation of the research style of political science then began in earnest.

The behavioral approach has developed much like an octopus, putting out such varied tentacles as survey research, systems theory, and structural functionalism, all so closely associated with the behavioral movement as to be nearly synonymous with it. The various manifestations of the movement have resulted in general confusion about the true identity of the behavioral revolution. There is consensus on only the most basic principles: first, that the "new" political science concentrates on the observable behavior of individuals rather than institutions. Second, hope for real progress in the accumulation of knowledge about politics lies in the ability of political scientists to exploit the canons of science by use of the scientific method and the development of a sophisticated methodology for the quantitative analysis of empirical data. This goal is consistent with the third principle: political science must adopt the behaviorism dominant in the other social

sciences, so that it might emerge as part of a united social science, leading eventually to a unity of all science.

Thus Bernard Barber represents, perhaps unknowingly, the "ideal" position of a dedicated behavioralist when he says:

Everything we have said already has been based on the assumption that social science is not only possible but even essentially the same as natural science. . . . Science is a unity, whatever the class of empirical materials to which it is applied, and therefore natural and social sciences belong together in principle.

(Barber, 1952:311)

As mentioned earlier, the behavioral revolution grew in several different directions, with little more than the principles just outlined to unite the various approaches under one rubric. Three directions within the movement are particularly prominent: 1) the study of individual behavior, especially the pursuit of voting behavior and attitude measurement, 2) the development of the theoretical framework known as systems theory, and 3) the development of a related framework known as structural-functionalism.

Sophisticated measurement of attitudes was stimulated by the military needs of the national government during World War II. Much of the methodology developed in that context, primarily the technique of survey research and improved methods of attitude measurement, was subsequently adopted by political science, with particular success in the area of voting behavior. To this date, it is voting behavior which is the model of the achievements of the behavioral revolution.

However, the study of larger units, such as groups, institutions, or other aggregates needed the help of a theoretical framework in order to advance beyond its previous level of historical description, which the behaviorists felt had not been sufficiently productive. Though not a "behavioralist" himself, a framework was provided by David Easton in his famous book The Political System (1953), and later elaborated in his article in World Politics entitled "An Approach to the Analysis of Political Systems" (1957). With the help of the systems theory framework (briefly, the notion of inputs, conversion, outputs, and feedback within the boundaries of the defined "system") empirical theory building has made unprecedented strides. The framework has been criticized for establishing a conservative bias in its equation of system equilibrium with system quality, and its failure to provide a viable means for revolutionary change. There is little doubt, however, that this framework dominated the theoretical work in political science during the 1960's, and David Easton is certainly justified in his preface to the second edition of The Political System (1971) in his implicit claim that it inspired a spate of empirical theory in political science.

A second influential theoretical framework widely used in political science during the 1960's is the structural-functional framework; an attempt to apply the structural-functional analysis developed by Parsons (1951)

and Levy (1951) to the purposes of political analysis. The most famous adaptation for political analysis is that developed by Almond (1960), in which he proposes two categories of functions: the political and the governmental. The framework has never been a dominant mode of analysis in political science, but it has been widely employed and is considered to be of extremely high potential for the development of a general theory of political systems. (Mitchell, 1962:vii) There is little doubt that its prominence within the discipline was largely based on this potential, considering the importance of the development of empirical theory to behavioralists.

The structural-functional framework rests on many of the same assumptions which underlie systems theory (and in fact it presupposes an acceptance of the notion of a "system"), therefore, it is subject to the criticisms applied to systems theory, and additional ones as well. As with systems theory, the structural functionalist framework does not provide for revolutionary change, and rests on the conservative assumption that equilibrium is a positive goal for all systems. Furthermore, when applied to political systems, as Almond (1960) has done, there is no theoretical guidance provided by the framework in identifying the functional prerequisites of a political system. Almond, therefore, offers only one justification for the functions he proposes; that they seem to be fulfilled by every existing

political system. Another criticism is that the framework provides no indicators of when a system is in a state of equilibrium, leaving us to assume only two possible states for a system; "equilibrium" or system disintegration. This leads to the assumption that if a system is not in a state of disintegration, it must be in a state of maintenance, a violation of what we intuitively know by everyday observation.³

Post-Behavioralism

As its name indicates, post-behavioralism is a reaction to the behavioral revolution. Specifically it is a reaction against the complacency of American political science; its conservatism, its governmental links, and the "irrelevance" of the discipline. Originally formulated by the Caucus for a New Political Science (CNPS) in 1967, this approach called for a cleansing of both the outside influences and internally held myths of the discipline. Among the myths found most objectionable was that of objectivity, and therefore, the post-behavioralists found common cause with the established critics of value-free social science.

CNPS popularized the radical critique of political science research; that is, that behavioral political science has served as a strategic tool for the U. S. government and corporate establishment. In addition, the critique charges that political science teachers in the classroom

have defended a limited democracy to their students, justifying it as the ideal system in view of the dangers inherent in a society of complete mass participation. Thus, teachers encourage the notion that limited democracy (known euphemistically as "workable pluralism") is preferable to mass participation and that there is little that needs to be done to improve an already unsurpassed system.⁴

Within the ranks of the post-behavioralists there are two variations on this position. One, formulated by David Easton, is to the "right" of the position taken by the CNPS. The other, represented by the editors and contributors to the most definitive post-behavioral statement, An End to Political Science (Surkin and Wolfe, 1970), presents a more radical view than that of the original CNPS position. The CNPS position has never been prominent, much less dominant in the discipline, and therefore is not of particular interest here, where the aim is to identify the dominant disciplinary norms during the period of political development research. The position represented by David Easton, however, has played much the same role as have the critics of logical positivism in modifying certain of its claims. That is, Easton's brand of post-behavioralism has, while not gaining dominance, served as a modifying influence on the dominant norm, behavioralism. Easton's position is spelled out in his 1969 Presidential Address to the American Political Science Association.

Here he is not calling for radical reform within the discipline, but is simply asking that the discipline enter a new episode, one characterized by increased relevance in political research. He states that his post-behavioralism has no ideology and embraces scholars from the right or the left. His only platform for determining the most pressing needs of society is the application of "humane criteria." (Easton, 1969:1053)

How did Easton's brand of post-behavioralism modify the behavioralism in political science in the late 1960's and early 1970's? As mentioned earlier, post-behavioralism has not come to dominate the discipline, but has merely affected some modification in the dominant position. The modification is principally the creation of a greater awareness of the threat posed by close relations between the discipline and the federal government, and the political implications of the discipline's ignoring the existing state of the nation and the world in a narrow-minded commitment to objectivity. Policy-relevant research has become more protective of the interests of the oppressed in societies, without going so far as to abandon the distinction between advocacy and objective research.⁵

The ethical standards which are widely accepted within a discipline represent another norm of research. One of the motivating sentiments behind post-behavioralism was a sense of embarrassment on the part of political scientists caught in the probably inadvertent act of serving

as handmaiden and accomplice to foreign meddling on the part of the U. S. government. The most important scandal of this sort was the scandal of Project Camelot, which was of national proportions and had international repercussions. The details of Project Camelot will be discussed in Chapter V: here it raises the question, What changes did a growing awareness of the ethical problems inherent in social science research bring about within political science?

Professional ethics were of little concern to political scientists when they were primarily involved in descriptive research concerned with institutions. The determination of what was and was not ethical did not seem to be a pressing problem in the everyday practice of political science, and in any case, seemed best left to the political philosophers, who were concerned with questions of morality in general.⁶ However, two factors brought ethics under the active consideration of political scientists, and all social scientists; the behavioral revolution, and the widespread practice of social science "field work" in foreign countries. In some ways these two developments go hand in hand. That is, the sophisticated methodological techniques developed with the rise of the behavioral revolution made it a greater, and potentially more rewarding, challenge to social scientists to attempt some empirical testing of their theories cross-nationally. The impetus to do

research abroad was also stimulated by a growing government interest in foreign areas after World War II, when nations formerly dominated by the colonial countries were becoming autonomous participants in world affairs. As a result, the government made large sums of money available to scholars in order to develop U. S. competence in area studies, since in pre-World War II higher education the United States and Europe were the only regions considered "proper subjects for scholarly inquiry" and there existed only a tiny segment of scholars who were at all conversant in other areas of the world. (Lambert, 1973:1)

In domestic research, the ethical issues raised by the behavioral revolution and its emphasis on the observation of individual behavior are primarily those of privacy and welfare, as these pertain to the subjects being studied. The problem of privacy is usually solved by assuring the respondents anonymity.⁷ Protection of both privacy and welfare is particularly crucial when political scientists are involved in delicate domestic or foreign research, and must act responsibly toward their respondents, who, for relatively small reward, may be taking serious risks should the researcher involved expose their political beliefs or activities.

The most simple and straightforward method for determining a discipline's ethical standards is to examine its code of ethics, if such a code has been adopted by

an organization of professionals within the discipline. However, there are few professional organizations in the social sciences which have done so. Exceptions are the American Anthropological Association, the Society for Applied Anthropology, the American Psychological Association, and a "statement on Professional Ethics" adopted by the American Association of University Professors in 1966. The American Political Science Association has not yet adopted such a code. However, seven principles have been recommended as necessary inclusions in any code of ethics in the social sciences, and these principles serve as a useful indicator of the ethical standards which are characteristic of mainstream social science, including political science, during the 1960's. The principles, in brief form, are:

- 1) There should be a recognition , and an affirmation, of the claim to private personality.
- 2) There should be a positive commitment to respect private personality in the conduct of research.
- 3) To the fullest extent possible without prejudicing the validity of the research, the informed, and voluntary, consent of the respondents should be obtained.
- 4) If consent is impossible without invalidating the research, then before the research is undertaken, the responsible officials of the institutions financing, administering, and sponsoring the research should be satisfied that the social good in the proposed research outweighs the social value of the claim to privacy under the specific conditions of the proposed invasion. These officials in turn are responsible, and must be responsive, to the views of the larger community in which science and research must work.

- 5) The identification of the individual respondent should be divorced as fully and as effectively as possible from the data furnished. Anonymity of the respondent to a behavioral research study, so far as possible, should be sought actively in the design and execution of the study as a fundamental characteristic of good research.
- 6) The research data should be safeguarded in every feasible and reasonable way, and the identification of individual respondents with any portion of the data should be destroyed as soon as possible, consistent with the research objectives.
- 7) The research data obtained for one purpose should not thereafter be used for another without the consent of the individual involved or a clear and responsible assessment that the public interest in the newly proposed use of the data transcends any inherent privacy transgressions. (Ruebhausen and Brim, 1966:437)

While these principles adequately cover the protection of the subject's privacy, there are additional ethical issue-areas which merit a brief review. The first is the ethical conduct of the American researcher in foreign countries. As mentioned earlier, one responsibility which the researcher must acknowledge is the protection of assistants, collaborators, and subjects from any possible danger resulting from the research. This danger might take many forms, which the researcher must take steps to prevent in the course of research. If danger to those who have helped with the research will not arise until after the work is completed and the Americans have left the country, then it cannot be considered ethical to "abandon" participants in the research project to an uncertain fate. In this case, the research either should not be undertaken, or provisions must be made for the protection of those involved before, during, and after the research is done.

The researcher has a second responsibility; to inform the subjects of the true nature of the research and to make completely clear what publications will result from it, the consequences of this publication, the eventual use of the results, and the degree to which anonymity will be protected. A controversy over just these issues was raised by the study of "Springdale," conducted by Arthur Vidich and Joseph Bensman, published in 1958 as Small Town in Mass Society. The identity of "Springdale" was only slightly protected, and there was a backlash of resentment against the researchers as a result of what the subjects perceived as inadequate protection of their privacy.⁸ The research was thought to have reflected badly on the community; much the same controversy which arose cross-nationally with Oscar Lewis' 1961 study of a poor Mexican family, The Children of Sanchez.

A third ethical problem which has caused controversy in foreign research settings is the misrepresentation, to the public and to those involved or touched by the research, of the sponsoring agency involved. This is an ethical problem, above and beyond the obvious issues of honesty in providing proper self-identification, in that sponsoring agencies prompt varying degrees of suspicion, depending on whether they are foreign or local, private or governmental, profit or non-profit. Frederick W. Frey, on the basis of his own cross-cultural research, has

attempted to rank types of sponsors in terms of their suspiciousness to respondents. Frey's ranking, from the least to the most suspicious, is:

- 1) indigenous universities and foundations
- 2) the indigenous national government
- 3) the indigenous local government
- 4) indigenous local polling agencies
- 5) international philanthropic organizations
- 6) international governmental organizations (e.g., UNESCO, WHO)
- 7) foreign universities and foundations (i.e. of a single foreign country)
- 8) foreign government agencies (with those agencies bearing responsibility for foreign relations most suspect--i.e., the foreign ministry, propaganda ministry, defense ministry, and intelligence agency)

(Frey, 1970:212)

The consequences of misrepresenting the sponsoring agency, in an attempt to reduce the level of suspicion among subjects and collaborators in foreign countries is clearly witnessed by the case of Project Camelot. Dr. Hugo G. Nuttini, peripheral associate of Project Camelot, caused the eruption of the Camelot scandal in Santiago by assuming that he alone in Chile had information about the true nature of the U. S. Army sponsorship of Project Camelot. In his discussions with potential participants in the project in Chile, he obscured the true nature of the sponsorship, and was unmasked by Johan Galtung, a Norwegian social scientist who had information regarding the Army sponsorship. When Nuttini's misrepresentation was unveiled, the Chilean Congress and media refused any further participation in the project, which ultimately was cancelled.

(Horowitz, 1967)

Two final issues not addressed by the typical social science code of ethics are 1) instances of conflict of interest and 2) constraints imposed by a sponsor or employer on research or publication. An example of a controversy sparked by issues of conflict of interest is the involvement of the executive director and the treasurer of the American Political Science Association in another organization, Operations and Policy Research, Inc., identified as a recipient of funds from CIA supported foundations. The simultaneous involvement of the two political scientists in a professional organization and a government-sponsored private firm raised the suspicion that this involvement compromised the integrity of the APSA, due to possible conflict of interest on the part of the two organization officers. An investigation was called, and it was determined that no conflict of interest existed in this case. (Samuelson, 1967)

A more common instance of conflict of interest, discussed by Harold Orlans, is the use, by funding agencies, of academic scholars as consultants in making their decisions as to whom to fund. The conflict occurs when these same academics, or their close associates or colleagues, make application to the same funding agency for research support. Orlans has recommended that:

The wisest course may be to require those who render advice to give those who receive it full statement of their interests so that these can be discounted. Though it cannot be required, it can also be hoped that fewer experts will take a sanctimonious view of

their specialty, and more will be able to distinguish its interest to themselves from its usefulness to society.

(Orlans, 1967:14-15)

The issue of outside constraints imposed on an individual's work is also an ethical one, insofar as it is germane to a central part of the academic "code"-- academic freedom. This is not the place to discuss the central issues of academic freedom in depth, but an important ethical problem is raised when this concept is violated; principally, whether or not a scholar has a duty to demand assurances of the freedom to publish the results of his research. Generally speaking, although not usually covered in codes of ethics, the freedom to publish is a long-cherished academic privilege widely respected in the social sciences, where research may be particularly delicate. It can be said to be firmly part of the dominant ethical norms of the social sciences.

One final issue concerning ethical norms in the social sciences addresses the social scientist's responsibility for the use of research after it has been completed. This issue has been acutely troublesome to the natural sciences, where the results of research may be applied for ends resulting in widespread death and destruction. As the social sciences become more scientific there is increasing concern that scholars may become so single-minded in their acceptance of the scientific method that they become indifferent to the political, social, and human consequences

of their work. It has been argued that a scientific ideology, if allowed to become enshrined, will lead to the creation of a class of unaccountables, much the same as an aristocratic class is unaccountable for its acts.

(Willhelm, 1964) Karl Popper has said:

The problem of the unintended consequences of our actions, consequences which are not only unintended but often very difficult to foresee, is the fundamental problem of the social scientist.

(Popper, 1963:57)

The ethical stance of the social sciences in general favors a conservative approach to the use of results: that is, scholars must take responsibility for what is done with their work in order to avoid its use in a manner which would lead to flagrant moral violations.⁹

Dominant Political Science Norms and Political Development Research

Presumably, it should be possible to measure with some precision the extent to which political development research conformed to political science norms during the 1950's and 1960's. Such an exercise will not be attempted here, but a cursory comparison of the norms of research and the political development case is certainly feasible. Such a comparison will rest on the assumption that influence flows both ways in the relationship. That is, disciplinary norms certainly help or hinder the rise of a trend according to its extent of conformity. However, at any point in its development a trend may also exert

its own influence on the disciplinary norms of a given time. How, then, did this two way relationship function in the case of political development research?

Without doubt, the rise of the trend of political development research was compatible with the rise of the behavioral revolution in political science. While quantitative studies based on survey research techniques were not widespread in political development research, an interest in attitude measurement, a theoretical orientation, and a commitment to empirical rather than normative research were common to both political development research and the behavioral revolution.

It is also significant that the neo-Marxist perspective was not compatible with the behavioral revolution, an incompatibility which undoubtedly accounted in part for its somewhat delayed acceptance by political scientists. The scholarship of the neo-Marxist perspective tended to be partisan, non-quantitative, and only rarely based on empirical data. For these reasons, it appeared to the behaviorists to be shabby research, and was discounted on the basis of disciplinary standards of good scholarship. The fact that much of what the perspective proselytized later became acceptable to mainstream political development theorists does not change the unacceptability of the approach during the 1950's and 1960's. It was for its very advocacy of points later to be confirmed and widely accepted

that it was considered to be unscientific research, for at the height of the behavioral revolution, advocacy of a position without rigorous scientific testing to confirm one's faith convinced few political scientists.

At the same time that political development research was prospering by its conformity to the dominant disciplinary norms of its time, it was also strengthening its position even further by making positive contributions to those norms. The most outstanding was the application of structural-functionalism to politics, contributed by Gabriel Almond (1960). Because structural-functionalism had potential as a theory-building tool, it was viewed as an innovative contribution to political science research. Under different circumstances, it may have been regarded by the political science discipline as deviant.

The role of political development research as a positive contributor to political science norms of research cannot be overemphasized. While a case study does not permit generalizations, the case of political development certainly supports the proposition that a research trend is helped by conformity to established norms of good research and harmed by deviation from those norms. The role of deviation-as-innovation is a crucial and fascinating issue which may hold the key to the establishment of an exact point at which research is acceptable versus a judgement that it is unacceptable.

That is not to say, however, that political development research was without its deviant aspects. In fact, it was in part the result of some deviance from accepted norms that the trend began its decline. When scandals exposing the involvement of political scholars in morally questionable U. S. military projects sullied the reputations of several political scientists (see Chapter V), political development research suffered by association. Although much political development research had violated no ethical norms, its reputation was tarnished by the scandals of that which did, and by association with cross-cultural research in other disciplines touched by scandal. As a research trend it was seriously harmed by perceived violations of understood ethical norms.

A second series of events having an adverse effect on the political development research trend were those germane to the rise of post-behavioralism. As described earlier, post-behavioralism served as a critique of the behavioral movement: as a critique it could be overlooked with academic impunity during most of the 1960's, but it became a force of its own when many of its positions were adopted by the political science establishment during the 1970's. It is interesting to note the congruence between some neo-Marxist positions and those taken by the post-behavioralists. There is little doubt that the simultaneous institutionalization of both perspectives as part

of the political science discipline was, in some part, a result of their mutual reinforcement. The neo-Marxist perspective was aided in its rise to respectability by the post-behavioral critique. The post-behavioral critique was undoubtedly informed by the neo-Marxist perspective.

The examples outlined above, drawn from the case of political development research, illustrate the two-way relationship between research and dominant disciplinary norms. A trend dominant within the political science discipline conforms to the established standards of research and ethics which prevail within the discipline. So long as this conformity continues, and the standards do not change drastically, the trend may be expected to flourish. A violation of the dominant disciplinary norms on the part of scholars, or a shift in disciplinary norms not matched by conformity on the part of the research trend may be expected to spell the decline of the trend.

Disciplinary norms, however, are not the only factor exerting an influence on the rise and fall of a research trend, but merely one of four to be reviewed in this dissertation. Chapter III will discuss the role of personal ideological commitment in the life of a research trend.

CHAPTER II

NOTES

¹For accounts of the history of political science, see: Albert Somit and Joseph Tannenhaus, The Development of American Political Science (Boston: Allyn and Bacon, 1967); Frank J. Sorauf, Perspectives on Political Science (Columbus, Ohio: Charles F. Merrill Books, Inc., 1965); Richard Jenson, "History and the Political Scientist," in Seymour Martin Lipset (ed.), Politics and the Social Sciences (New York: Oxford University Press, 1969), pp. 1-28; Charles S. Hyneman, The Study of Politics (Urbana: University of Illinois Press, 1959); and Bernard Crick, The American Science of Politics (Berkeley: University of California Press, 1959).

²Literature on the rise and effects of the behavioral revolution includes: Heinz Eulau (ed.), Behavioralism in Political Science (New York: Atherton Press, Inc., 1969); Austin Ranney (ed.), Essays on the Behavioral Study of Politics (Urbana: University of Illinois Press, 1962); James C. Charlesworth (ed.), The Limits of Behavioralism in Political Science (Philadelphia: American Academy of Political and Social Science, 1962); and Herbert J. Storing (ed.), Essays on the Scientific Study of Politics (New York: Holt, Rinehart and Winston, 1962).

³For further discussion of structural-functionalism as it has been employed in political science, see: William Flanigan and Edwin Fogelman, "Functionalism in Political Science," in Don Martindale (ed.), Functionalism in the Social Sciences (Philadelphia: American Academy of Political and Social Science, Monograph 5, 1965), pp. 111-126.

⁴Recently, several books have been published dealing with post-behavioralism; perhaps the best is: George J. Graham and George W. Carey, The Post-Behavioral Era: Perspectives on Political Science (New York: David McKay Company, 1972). Books not on post-behavioralism, but written from the post-behavioral perspective are: Charles A. McCoy and John Playford (eds.), Apolitical Politics: A Critique of Behavioralism (New York: Thomas Y. Crowell Company, 1967); and Derek L. Phillips, Knowledge From What? (Chicago: Rand McNally and Company, 1972).

⁵For a discussion of this growing awareness of the problems raised by the post-behavioralists, and a discussion of the issues involved, see Herbert C. Kelman, "The Relevance of Social Research to Social Issues: Promises and Pitfalls," in The Sociology of Sociology, The Sociological Review, Monograph 16 (Keele, Staffordshire: University of Keele, 1970), pp. 77-100.

⁶This attitude is reflected in a 1946 debate; Gabriel Almond, "Politics and Ethics--A Symposium," American Political Science Review, 40, no. 2 (April, 1946), pp. 283-312. The debate arose out of two articles which were published in the APSR in 1943 and 1944 respectively. The first was William F. Whyte, "A Challenge to Political Scientists," vol. 37, no. 4 (August, 1943), pp. 692-697, and the second was John H. Hallowell, "Politics and Ethics," vol. 38, no. 4 (August, 1944), pp. 639-655.

⁷The practice of assuring anonymity, if strictly followed, does give privacy to the individuals being sampled. However, it should be noted here that there is no legal recognition of the confidentiality of data per se. (Beals, 1969:179)

⁸A discussion of the issues raised by the study of "Springdale," including the authors' defense, appears in Human Organization, vol. 17, no. 2 (1958-59), pp. 1-2 and vol. 17, no. 4 (1958-59), pp. 2-7.

⁹This statement, of course, begs the question of what constitutes a flagrant moral violation. That sort of judgement is undoubtedly nested in political ideology which varies from individual to individual and from discipline to discipline. One rather unsatisfactory formula has been offered by Talcott Parsons, who proposes that these important questions be answered by applying to them the same "fundamental system of rational orientation to the world" which characterizes the pursuit of science. See Talcott Parsons, "Some Aspects of the Relation between Social Science and Ethics," in Bernard Barber and Walter Hirsch (eds.), The Sociology of Science (Glencoe, The Free Press, 1962).

CHAPTER III

IDEOLOGY AND RESEARCH

A case has been made, in the previous chapter, for the importance of dominant disciplinary norms in determining the rise and decline of academic research trends. It would surely be naive, however, to suppose that an explanation for the development of research trends lies exclusively in factors which are so neatly identified and measured. An imperative in constructing a comprehensive model of the causes of fluctuations in research trends is the inclusion of one troublesome and problematic variable--the variable of individual proclivities and passions. For it cannot be denied that men and women, when approaching research, bring with their approach a package of human emotions, opinions, and experiences which partially determine the nature of the research to be produced. The most comprehensive, yet specific, social science term available to capture this personal persuasion is the term "ideology."

The study of ideology has always been a central concern of the sociology of knowledge, stemming from its concern with the way in which social relations influence knowledge. Social relations, in this case, are seen as a

principal determinant of individual and societal ideology. Individual and societal ideologies, in turn, influence the assignment of worth to knowledge produced within a society and influence the very production of knowledge itself.¹ It was Karl Marx and Friederich Engels' formulation of this process which became the most influential in the study of the sociology of knowledge. In The German Ideology (1947), Marx and Engels discuss the extent to which social relations influence (and distort) individual ideology and the ideology of the society as a whole. Their formulation is heavily dependent on their prior analysis of society as characterized by a class structure in which the ruling class dominates and exploits the working class or proletariat. Ruling class domination is seen as the principal nexus in the determination of ideology. To quote Marx and Engels

The ideas of the ruling class are in every epoch the ruling ideas: i.e., the class, which is the ruling material force of society, is at the same time its ruling intellectual force. The class that has the means of material production at its disposal has control at the same time over the means of mental production, so that thereby, generally speaking, the ideas of those who lack the means of mental production are subject to it. The ruling ideas are nothing more than the ideal expression of the dominant material relationships, the dominant material relationships grasped as ideas; hence of the relationships that make the one class the ruling one, therefore the ideas of its dominance. The individuals composing the ruling class possess, among other things, consciousness, and therefore, think. In so far, therefore, as they rule as a class and determine the extent and compass of an epoch, it is self-evident that they do this in their whole range, hence, among other things, rule also as thinkers, as producers of ideas, and regulate the production and distribution of the

ideas of their age: thus, their ideas are the ruling ideas of the epoch.

(Marx and Engels, 1947:39)

Others following Marx and Engels seemed less concerned with tracing the sources of cultural distortion than with sorting out and defining the initial and limiting conditions of the social components of thought. W. Stark, for instance, excludes the study of deliberate distortion of knowledge by the ruling class from the proper study of the sociology of knowledge:

. . . every society must take up some concrete vantage point from which to survey the broad--the unbounded-- acres of that which is, and every society will therefore have its own particular picture of reality because it sees reality, and must see it, in one particular perspective. The thesis of the sociology of knowledge is that the choice of the vantage-point from which the ens universale is envisaged, depends in every concrete society on the human relationships which make that society what it is; but it is not asserted that selfish or sectional interests enter into the matter already at the point where the fundamental vision first springs into being. That they may come later on and assert themselves is not to be denied; but that is an entirely different problem. The sociology of knowledge does not deal with warped thought . . .

(1958:49)

In either case, whether the investigator is interested in deliberate distortions and their class bases or simply the process by which distortion is introduced, the central concern is with societal and individual ideology, as "ideology expresses that point in social reality at which interests connect up to a picture of reality." (Horowitz, 1961:79) In other words, each individual member of society, and each society as a whole, has a pre-disposition to interpret reality in a manner consonant with its unique operational

base: the pre-disposition manifests itself in the individual or societal ideology. Sociology of knowledge has historically been interested in the content of this ideology, the process by which it influences perceptions of reality, and the consequences of such influences, both social and political.

The following discussion of ideology will begin by reviewing the development of the concept of ideology and its various definitions, followed by an examination of the ideological content of social scientists' attitudes, subsequently focusing on the specific ideological content of political development researchers' attitudes. Finally, the discussion will end with an examination of the relationship between the intelligentsia and the ruling class, once again ultimately focusing on the case of political development research and its relationship to the ruling class activity of foreign policy-making.

Definitions of Ideology

Ideology has been identified above as "that point in social knowledge at which interests connect up to a picture of reality." (Horowitz, 1961:79) Such a statement is not, however, an actual definition of ideology, but rather a description of the importance of ideology to the issues of concern to the sociology of knowledge. A true definition of the term "ideology" must trace its origins, note changes in its meaning so as to expose the entire

spectrum of its meaning, and give some definitive version of its current usage which will enable the reader to feel confident of what is implied by the use of the term.

The term 'ideology' was originally used by French scholars in the early nineteenth century to refer to "the investigation of the natural sensory origin of ideas." (Gould and Truitt, 1973:7)² The term at that time represented a revolutionary departure from a previous concern with the role of the soul and human spirituality in the determination of ideas. Its use was, therefore, squarely in the scientific mode.

The rise of Hegelian philosophy had the effect of widening the number of explanatory factors associated with "ideology." Hegel recognized that ideas were not simply a product of sense stimulation, but also were influenced by historical, cultural, and social conditioning. At this point, the stage was set for the contributions of Marx and Engels, who were to introduce a more critical and progressive interpretation of the formation of ideas. According to Marx and Engels, as stated in The German Ideology (1947), ideas are not only the product of 1) sensory stimulation, and 2) historical and cultural factors, but also of social institutions and the distribution of power within a society. As noted above, the relationship between the power structure of the society and the ideas dominant in that society, according to Marx and Engels, has proven historically to be a mutually compatible one. That is, the ruling class has

consistently been successful in the institutionalization of its ideas within the society. Thus, the concept of class interest, central to Marxian thought, is manifested in the ruling class promulgation of ideas which serve its own interests. The agent for this institutionalization is here identified as the intellectual segment of the society. The argument presupposes that ruling class interests and the interests of the intelligentsia are mutually reinforcing.

The formulation introduced by Marx and Engels is the most influential interpretation of the causes and effects of ideology to date. In reviewing the development of the term "ideology," John Plamenatz says:

Marx and Engels and their disciples have not been the only users of the word, nor has it been used only in the senses they gave to it. Yet it is their word more than it is anyone else's. They were the first to give wide currency to it and even today most writers who use it or who discuss its uses, even when they are not Marxists, are very much aware of the uses of it made by Marx and his followers. Even when they are not directly concerned with the theories of Marx and Engels, they are often as not trying to answer or reformulate questions that Marx and Engels put, or to put and answer other questions suggested by a critical analysis of Marxist views about ideology.

(Plamenatz, 1970:20-21)

Central to the Marx-Engels formulation of the concept "ideology" is the notion of "false-consciousness." Because the ideology of societies has historically been under the controlling influence of the ruling class, ideology is by its nature untruthful, "since it entails a 'masking' or 'veiling' of unavowed and unperceived motives or 'interests.'" (Shils, 1968:73) In other words, the

acceptance by members of a society of the ideology which is dominant in that society (and which reflects the interests of the ruling class and is promulgated by the intelligentsia acting in the service of the ruling class) is contradictory to the interests of the broad base of the society as a whole. Therefore, those who accept the ideology when it is against their own interests are victims of belief in a "false truth." The term employed by Marx is that they suffer "false-consciousness."

The question remains: What is exactly meant by the term "ideology?" Marx and Engels must be criticized for their unclear definition of the term, for while they use it to refer to the ideas and attitudes characteristic of a group or community, it is difficult to identify what groups or what ideas they had in mind. Contemporary scholars have attempted to bring more precision to Marx's definition of the term, but nearly every writer on the subject must redefine the term as a preface to any discussion of it in order to be assured that the reader knows what is being discussed. The variation among these definitions is notable: while some scholars stress the aspects of ideology most relevant to sociology of knowledge, others stress the more neutral definition of the term, one which does not focus on the potential of ideology for control and distortion which have been the object of study for some Marxists and sociologists of knowledge. The distinction between the two approaches to the definition of ideology might be seen

as a contrast between ideology as a process of perception and ideology as a tool of class interests. Examples of each type of definition will clarify the distinction.

An assumption of ideology as a class tool is implied by the definition of ideology offered by Irving Louis Horowitz:

Ideology is, from the point of view of the sociology of knowledge, the investigation of the social uses of ideas for the purpose of convincing or coercing men into actions having ultimate political and economic consequences.

(1961:81)

Here "ideology" is defined in a manner which makes its study inseparable from a study of the power structure of the society and the relationship between power and ideas. Examples of definitions less concerned with the societal power structure are those of Shils, Plamenatz, and Mannheim. Shils defines ideology as:

. . . one variant form of those comprehensive patterns of cognitive and moral beliefs about man, society, and the universe in relation to man and society, which flourish in human societies. Outlooks and creeds, systems and movements of thought, and programs are among the other types of comprehensive patterns which are to be distinguished from ideology.

(Shils, 1968:66)

The political origin and consequences of the "patterns of cognitive and moral beliefs" seem to be of less interest to Shils than the content of these patterns. Thus, the study of ideology in this case might profitably proceed in a political "vacuum," for it is not necessary to know the power structure of the society in order to examine the patterns of its members' beliefs.

Plamenatz defines ideology in a manner similar to Shils':

Ideology . . . refers to a set of closely related beliefs or ideas, or even attitudes, characteristic of a group or community.

(Plamenatz, 1970:15)

Mannheim, in a quote cited earlier, clarifies the position which may be assumed to underlie the definitions of Shils and Plamenatz. In discussing the concerns formulated by Marx and Engels, Mannheim says:

The sociology of knowledge is concerned not so much with the distortions due to a deliberate effort to deceive as with the varying ways in which objects present themselves to the subject according to the differences in social settings. Thus, mental structures are inevitably differently formed in different social settings.

(Mannheim, 1936:265)

In this statement Mannheim summarizes the problem of defining the limits to the proper study of ideology and the sociology of knowledge in general: namely, is it appropriate to include in a discussion of the determinants of ideas a consideration of deliberate distortions introduced by the forces of class interest? The debate need not be settled here. It does, however, serve as a convenient issue on which to partition the following discussion of the ideology of scholars working in the field of political development research during the 1950's and 1960's. The first section of the following discussion will examine the content of social science scholars' attitudes and beliefs, especially their political attitudes and beliefs. Such an examination will serve as a background to a subsequent

review of the relationships between these attitudes and beliefs and the scholar's product--political development research. The following section will deal with the congruence (or inconsistency) between the ideology of the ruling class and that of the scholars, members of the intelligentsia, who were involved in political development research. That section will deal directly with the problems raised by Marx and Engels; the relationship between the power structure of a society and the knowledge which that society produces and supports.

Ideological Content of Social Scientists' Attitudes

Research measuring the political attitudes of social scientists in the United States consistently has found the political ideology of the group as a whole to be skewed to the left of the distribution of political attitudes of the population as a whole. Seymour Martin Lipset, who has done extensive research on the subject of the political ideology of American academics, states that, in fact, intellectuals in general have sympathy for "liberal-left causes and ideologies." (Lipset, 1972:215) Of course, this is not to say there is not a cross-section of ideological positions represented within the academic community; merely that there is a strong tendency for academics to tend toward the left side of the ideological scale.³ Lipset reports:

The evidence that the dominant mood on the campus is liberal . . . is clear and decisive, even though the first detailed national survey of faculty

political opinion in all disciplines did not occur until 1966, an interesting fact in itself. A National Opinion Research Center (NORC) survey found that 61 per cent of the respondents described themselves as 'liberals,' as contrasted with 28 per cent 'conservative' with 11 per cent neither.

(1972:224)

Lipset goes on to say:

The most recent evidence is the data collected in the Spring of 1969 for the Carnegie Commission study. These findings, together with the results of similar studies of American students, the US public, and British academics . . . (report that) both faculty and American students gave comparable distributions of responses. They were each much more liberal-left than the US public as a whole.

(1972:225)

More specifically, the faculty of the social science disciplines tend to be farthest to the left politically within the academic community. Lipset reports that within the liberal arts:

innovative orientations are more likely to take a political (left) form among social scientists, next among humanists, who though less political are involved in the world of politically relevant intellectual ideas, and least among the natural scientists.

(1972:228-229)

More specifically still, Ladd and Lipset (1975) report that, based on the Carnegie Faculty Survey of 1969, sociologists are the farthest left politically of the social science disciplines, with 79% of respondents describing themselves as "Left" or "Liberal." Political scientists follow as a close second, with 69% of respondents giving a self-description as "Left" or "Liberal." Anthropologists, psychologists, and economists follow in descending order of percentage of liberal-left orientation. Economists, the least liberal

members of the social science disciplines reported, are still well to the left politically of the US public. Whereas 64% of economists responding indicated a political orientation of "Left" or "Liberal," only 21% of the American public characterizes itself in that fashion.

In a study focusing specifically on the political attitudes of American political scientists, Turner and Hetrick (1972) corroborate Ladd and Lipset's findings. In a survey of members of the American Political Science Association, the authors found that there are six times as many political science professors who are Democrats as there are who are Republicans. In fact, they conclude that the American Political Science Association is largely a "one-party organization, and therefore is farther to the left in its political ideology than is the American population as a whole."⁴

Several explanations have been formulated to explain the tendency of intellectuals to be farther to the left politically than the members of the society in which they work and live. Here again, Seymour Martin Lipset has made the most important contributions. Lipset offers the following analysis:

Intellectuals, as distinct from professionals, are concerned with the creation of knowledge, art, or literature. In awarding status within the occupation, the emphasis is on creation, innovation, avant gardeness. Professionals are the users of knowledge. And many writers have pointed out that inherent in the obligation to create, to innovate, has been the tendency to reject the status quo, to oppose the existing or the old as philistine. Intellectuals are also more likely than

those in other occupations to be partisans of the ideal, of the theoretical, and thus to criticize reality from this standpoint. The need to express the inner logic of their discipline, of their art form, also presses them to oppose the powers, the patrons, who seemingly are philistines, who prefer continuity rather than change.

(Lipset, 1972:216)

Elsewhere Lipset points out that there are two additional factors peculiar to the American setting which contribute to the tendency of intellectuals in this country to be politically left of center: 1) the historical ideology represented in the Declaration of Independence, which reflects the principles of the democratic left, and 2) the feeling of deprivation which scholars supposedly suffer as a result of those same egalitarian principles (which discourage the assignment of status to intellectuals). (Lipset, 1963:344) The latter factor, feelings of self-identification as an underprivileged group, has been shown to be closely correlated with adherence to liberal politics. (Lazarsfeld and Thielens, 1958:11-17)

An explanation for the liberal tendencies of members of the social science disciplines has been formulated by Theodore Geiger and translated from the German by Lipset. (1963:342) Geiger's account points to the capitalist context in which contemporary western social science exists:

Of all groups in the intelligentsia, the social scientists are most sensitive to the power dimension in society, and also the most exposed to the attacks on intellectual freedom by those in power. The loss of intellectual autonomy and freedom also endangers their professional work and calling. Therefore, we can expect that in a social order in which capitalist business

enjoys a great amount of power and has or could use pressure of one kind or another against criticism coming from academics . . . a significant number of social scientists--defined in the broadest sense possible--would be attracted to the left in one or another of its forms.

(Geiger, 1949:124)

In addition to Geiger's explanation, there can be little doubt that socialization experiences occurring after the initial decision to enter into the study of the social sciences serve to further shape the individual academic ideology. The student attracted to the social sciences, for whatever reason, is exposed to a socialization process which applies pressures on his or her still-flexible ideological structure, and is unlikely to pass through the educational training (socialization) period ideologically untouched. In other words, the student will be exposed to the pressures of conformity, and it is unlikely that these pressures will have no effect. Speaking specifically of the ideological pressure brought to bear on sociologists, Lewis Anthony Dexter (1958) has said:

The ideology of sociologists in the United States involved defense of, if not identification with, the underdog. Typically, excluding a few serious followers of Sumner and Pareto, American sociologists tried to get students disembedded from 'the cake of custom' so they might become 'less prejudiced' against counter-mores behavior. . . .

(1958:179)

Of course, such a statement is not meant to imply that childhood socialization is not vital to the formation of ideology.⁵ However, it does seem reasonable to speculate that the intensity of intellectual stimulation received

during the course of studying the social sciences results in a high degree of adult socialization, which may in turn result in a high level of ideological 'adjustment.' In fact, Turner and Hetrick (1972) hypothesize that:

. . . among the members of this (American Political Science) Association--in comparison with the general population--adult socialization would be more influential and childhood socialization less important in determining party affiliation and political behavior.
(1972:362)

The authors' hypothesis is supported by their finding that, although three out of four members of the American Political Science Association surveyed reported that they were Democrats, it is not the case that the respondents came primarily from Democratic families. Since parents' political party identification has proven the single most reliable predictor of political party affiliation in the American population as a whole, Turner and Hetrick's finding indicates that something peculiar to the academic life mitigates against this strong association. The authors conclude:

. . . the fact that a large proportion of the respondents supported a party different from that of their parents also indicates that for many of the respondents their party preferences represent deliberate choice. From information supplied by the respondents, it is apparent that knowledge gained in the profession and the influence of colleagues were important factors in their partisan preferences. As noted above, 75 percent of the members surveyed reported discussing political issues with colleagues at least once a week. Thus, the importance of face-to-face peer groups in influencing political attitudes and behavior is suggested by our findings.

(1972:374)

The findings reviewed above address the effects of the practice of scholarship on the practitioner: evidence

exists, however that the roots of the liberal tendency of academic social scientists lie in the personality of those who are drawn into intellectual life, and specifically into the study of the social sciences. Ladd and Lipset (1975) report evidence that the social sciences are attractive to left-inclined students, and that:

Political science attracts left-of-center students and this selective ideological recruitment contributes significantly to the structure of faculty orientation.
(1975:168)

Perhaps the social sciences offer the political activist with reformist or revolutionary tendencies a medium for expressing those tendencies.⁶ Perhaps selective recruitment is a factor, in that the social sciences may tend to admit and encourage advanced students whose ideological make-up seems most compatible with that of the existing community. It is only possible, at this point, to speculate on the explanation for Ladd and Lipset's finding, as research on this explanation has thus far been limited.

Having reported research findings which indicate that the social science community is ideologically to the left of the American public as a whole, is it possible to speak more specifically to the question of the ideological inclination of political development researchers? Unfortunately, there is no one piece of attitudinal research sampling political development researchers per se and thus it is not possible to discriminate between their political attitudes and those of other political or social scientists.

In order to determine the political attitudes of political development researchers, a secondary analysis must be used: that is, one must examine the research produced by this group of scholars for its ideological content, and from that examination infer the political attitudes of the researchers themselves.

Analyses of the ideological content of literature characterized here as the "trend" of political development research have tended to identify that research with the "liberal" perspective in American political thought. In a lengthy analysis of political development ideas in the social sciences and foreign policy, Robert A. Packenham argues that:

To account for the origins and persistence of such ideas, no single explanation is adequate. The stress in the present book is on certain premises embedded in American culture and political thinking which are collectively designated here as the American liberal tradition.

(1973:4)

Specifically, the four liberal premises which Packenham isolates and identifies as the principal premises underlying social science theories concerning political development are: 1) change and development are easy, 2) all good things go together, 3) radicalism and revolution are bad, and 4) distributing power is more important than accumulating power. These premises, along with the political development literature's goals--identified by Packenham as democracy, stability, anti-Communism, peace, world community, and pro-Americanism--can be traced directly to the liberal tradition

in American political thought. While Packerham is somewhat superficial and facile in his association of the liberal tradition and the ideological underpinnings of political development research, his assertion that both are characteristically "liberal" in orientation does seem a reasonable one. The four premises listed above are all either compatible with, or directly reflective of, the "Lockean," or "liberal" strain of political thought, which Louis Hartz has persuasively argued forms the basis of American political thought since the Civil War. (Hartz, 1955) In appraising Hartz's landmark discussion of the liberal tradition in America, Marvin Meyers (1963) explicates Hartz's association of Lockean thought and the tradition of liberal thought, showing exactly how Hartz equates the two by identifying their identical components. Meyers states that Hartz uses the word "Lockean" to represent the social order, way of life, and unanimous doctrine of a nation born equal, and associates the word with bourgeois enterprise, "atomistic social freedom," and laissez faire. Specifically, Meyers says:

In historical terms, 'Locke' stands for the modern regime of 'liberal capitalism' in opposition to the feudal Ancien Regime on the Right and Socialism on the Left. Professor Hartz's 'Locke,' in brief, comes very close to expressing what Tocqueville called 'individualism:' that 'mature and calm feeling,' nurtured by equality of condition, leading each man to seek his own material well-being with slight regard for fellows or community, ancestors or posterity, tradition or authority.

(1963:263)

Hartz argues that the political thought of Locke has dominated American thought, almost without challenge, throughout its development. He cites only proslavery white sentiment before the Civil War as a real challenge to that dominance.

It is not difficult to see the compatibility, indeed the parallels between the liberal tradition as interpreted by Hartz and the premises of political development research which Packenham places within the same liberal tradition.⁷ To have drawn these parallels rigorously, however, would have strengthened Packenham's argument that it is the ideological orientation known as the "liberal tradition" which underlies political development research. Without such an elaboration of his assertion, it is difficult to see Packenham's justification for linking his four "premises" of the political development literature, accurately identified in my opinion, with "the liberal tradition" in American political thought. The equation has been popularly accepted as an accurate one: it is not, however, the only ideological interpretation applied to political development literature.

Ali Mazrui identifies cultural Darwinism as the dominant ideological perspective of the study of political development. Distinguishing between biological and cultural Darwinism, Mazrui demonstrates how the assumptions associated with cultural Darwinism have resulted in a "ranking" of cultural superiority in the case of the development

literature. This ranking has tended to place the advanced western nations and their political institutions in the highest rank. Thus, the Darwinian assumption of evolutionary necessity "locks" the developing countries into a position of cultural inferiority. The basis for the placing of the advanced western nations in the highest rank is the "self-confidence of ethnocentric achievement" felt by the scholars from these countries. (Mazrui, 1968:82) Thus, according to Mazrui, it is not the "liberal tradition" as Pakenham states, but rather an ethnocentric chauvinism which is the guiding ideological perspective underlying political development research.

Both Pakenham's and Mazrui's interpretation of the ideological perspective of the political development literature are now widely accepted by scholars interested in Third World development. For the interpretations are distinct, but not necessarily mutually exclusive: in fact, when integrated they provide a convincing interpretation. Assuming that scholars of the advanced industrialized countries are firmly entrenched ideologically in the "liberal tradition," it is reasonable for these scholars to see in that tradition the potential solution to some of the problems faced by Third World countries. The premise follows: what worked for the advanced industrialized countries in their struggle to get where they are now will work for the underdeveloped countries, whose goals are similar. Underlying this premise are both the "liberal tradition"

and a certain degree of ethnocentric chauvinism. So, for instance, the liberal assumption that "change and development are easy" (identified by Pakenham as one of the four central assumptions of the political development literature) is applied to the experiences of Third World countries through a certain ethnocentric naiveté. Similarly, the premise that "all good things go together" also arises from the experience of the advanced western nations and is considered a valid example and blueprint for Third World development.

Scholarship and the Ruling Class

If an amalgam of both Pakenham's and Mazrui's interpretations does establish an accurate reconstruction of the ideological perspective of political development research (and hence, presumably, of those who authored this literature), it is now possible to go on to the question so often posed in the sociology of knowledge: Is this ideological perspective the same as that of the ruling class? The issue ultimately raised by such a question is: To what extent are the social sciences a tool of the ruling class? As mentioned earlier, it was this question which, for Marx, was the central issue of the sociology of knowledge. Inevitably, the answer reflects a political bias. To those of the "New Left," American scholarship is the tool and legitimator of the ruling class. To moderates, scholars and members of the ruling class simply share a

common ideology or world-view, and hence the similarity in their conclusions. To those on the far right, academic scholarship in general is the product of a dangerously irresponsible element in American society--one which is far to the left of the populous as a whole, yet which wields power disproportionate to its number. The following review of the three positions will not settle the debate concerning the true relationship between the academic establishment and the ruling class in the United States today. It will, however, elaborate the various positions taken in that debate.

The leftist critique has focused on two aspects of the support given the ruling class by the social sciences. First, social scientists have created a myth of technical skill and objectivity which critics claim lends ruling class programs an air of legitimacy and makes it easier for the ruling class to maintain control over the lower classes. The proliferation of positivist and behavioral social science research in the last two decades and its increasing use by government and private industry spawned this first aspect of the critique. The second thrust of the critique attacks the involvement of social scientists in government foreign policy and received its greatest impetus from anti-war sentiment concerning American involvement in the Vietnam conflict. For while admitting that a good deal of this anti-war sentiment originated on the campus and was supported by academics, the critique points to the large amount of

support the war received from the academic community; both ideological support and personnel recruited by the government to play various roles in the conduct of the war.

Here it is most germane to examine what the critique has to say about the ideological similarities between the social science community and the ruling class: the peculiarities of recent American domestic or foreign policy will not be detailed.

Robin Blackburn has described the shared ideological orientation leftist critics identify as common to both social science and the ruling class, the bourgeois ideology.

This ideology . . . consistently defends the existing social arrangements of the capitalist world. It endeavours to suppress the idea that any preferable alternative does, or could exist. Critical concepts are either excluded (e.g. 'exploitation,' 'contradiction') or emasculated (e.g. 'alienation,' 'class'). It is systematically pessimistic about the possibilities of attacking repression and inequality: on this basis it constructs theories of the family, of bureaucracy, of social revolution, of 'pluralist' democracy all of which imply that existing social institutions cannot be transcended. Concepts are fashioned which encapsulate this determinism (e.g. 'industrial society') and which imply that all attempts to challenge the status quo are fundamentally irrational (e.g. 'charisma'). In short, bourgeois social science tries to mystify social consciousness by imbuing it with fatalism and by blunting any critical impulse. Those aspects of this social science which are not directly aimed at consecrating the social order are concerned with the techniques of running it. They are providing vocational training for future market researchers, personnel managers, investment planners, etc. . . . Moreover the systematic complacency of bourgeois social science about its own society and its instinctive pessimism about the possibility of creating a civilization which avoids its own misery and servitude blinds it to any understanding of the revolutionary stirrings within the advanced capitalist world itself.

(Blackburn, 1969:163-164)

In addition to lending support to the status quo, "bourgeois" social science research is "conducted from the vantage point of the dominant class in society and devotes its energies to gathering information about the underlying population which is useful to the ruling class and institutions." (Nicolaus, 1973:45) This behavior is camouflaged, however, by the claim of the social sciences to objectivity and value-neutrality, a claim which creates a smokescreen for those attempting to determine either 1) the biases of social science research (preservation of ruling class dominance), or 2) the implications of this research (continued lower class subordination and manipulation). The claim of objectivity, according to the leftist critique, obfuscates the interest-serving nature of social science research, and in so doing misleads both the consumer and those standing to be affected by the research.⁸

The second thrust of the leftist critique addresses the involvement of social science with American foreign policy, particularly vis-à-vis military research and policy formulation. Here, intellectuals (especially social scientists) are seen as the "new Mandarins"--technocrats recruited to facilitate and legitimize the imperialistic policies of an immoral government. A leading statement of this critique is Noam Chomsky's American Power and the New Mandarins. Chomsky argues that in recent years intellectuals have attained new levels of power, partially as a result of their "claim to possess the technique and understanding

required to manage our 'postindustrial society.'" (Chomsky, 1969:125) Zbigniew Brzezinski concurs in his analysis of the changing role of the intellectual:

. . . the largely humanist-oriented, occasionally ideologically-minded intellectual-dissenter, who sees his role largely in terms of proffering social critiques, is rapidly being displaced either by experts and specialists, who become involved in special government undertakings, or by the generalists-integrators, who become in effect house-ideologues for those in power, providing overall intellectual integration for disparate actions.

(Brzezinski, 1968:22)

Working hand-in-glove with government policy makers and other members of the elite, the new mandarins (drawn largely from universities, where they have often been educated in the social sciences), no longer act as critical observers, but as integral members of the ruling class. Their "objectivity" is supportive of a normless pragmatism; the governing tenet becomes adherence to the status quo. The resultant threat to scholarship is twofold. First, scholarly work is subverted by the demands and control of the ruling class, as scholars and the governing elite become less distinguishable. Secondly, scholarship as a whole, but especially the social sciences, may become discredited through association with bankrupt policies, whose failures will serve to taint all those associated with them. Chomsky adds:

What is more, the subversion of scholarship poses a threat to society at large. The danger is particularly great in a society that encourages specialization and stands in awe of technical expertise. In such circumstances, the opportunities are great for the abuse of

knowledge and technique--to be more exact, the claim to knowledge and technique.

(Chomsky, 1969:25-26)

Turning to the "liberal" or "moderate" interpretation of the relationship between scholarship and the ruling class, here once again close cooperation is noted, but the motives and implications of the cooperation are interpreted as benign. There is no attempt to demonstrate a conspiratorial partnership, designed to suppress the lower sector of society. In fact, from the moderate point of view, scholars do not have enough power to influence policy-makers, who would benefit from their advice and counsel but who often refuse to take them seriously.

Horowitz and Katz (1975) discuss the relationship between scholars and policy-makers, highlighting certain difficulties in the relationship, principally in the domain of inter-personal contact between the two groups. Scholars, the authors point out, suffer a certain loss of prestige and isolation from the academic community if a federal agency becomes their primary career association. In addition, when working with, or for, policy-makers, they must often respect limitations on publication and other forms of control which are anathema to academic traditions. Furthermore, policy-oriented research is disinterested in theory-building, so that here again the scholar must compromise in research. Horowitz and Katz say:

. . . federal bureaucrats operate with a concept of application that often removes theoretical considerations from research. Designing the future out of

present-day hard facts, rather than analyzing types of actions and interests and their relations in the present, comes to stand for a limited administrative utopianism and creates the illusion that demands for theory . . . have been met. . . . Social scientists come to suspect that their work is weighed for efficiency and applicability to an immediate and limited situation. The ability of the social system to confront large-scale and long-standing problems is left out of reckoning.

(1975:150)

Finally, the scholar must share responsibility for policy mistakes, and is subject to congressional inquiry and forms of harassment not generally suffered within the academic community.

At the same time, the "moderate" interpretation emphasizes other disharmonies: those originating in policy-makers' attitudes toward academic scholars. Here, the policy-making bureaucrats complain about the elitist treatment which academics expect to receive while in government service. Two aspects of this elitism are particularly annoying to the federal bureaucrat: academic scholars' insistence on access to the highest levels of administration, and their ability to cultivate outside activities and sources of income while working within the bureaucracy. This is grating to regular bureaucrats, who must depend fully on their jobs as the sole source of income. Further, there is some doubt among policy-makers of the value of the scholars' contributions, often seen as "ivory tower theorizing" ultimately irrelevant to pressing problems of policy.⁹ James N. Rosenau says:

One other consequence of the nonscientific character of foreign policy research needs to be noted, namely, that it commands little respect among those in positions to apply its concepts and findings to the actual conduct of world affairs. While the advice of foreign policy specialists is . . . sought by and provided to the agencies and personnel of government, the resulting interaction would not seem to be a typical client-expert relationship. . . . Plainly, neither officials nor governmental leaders defer to the foreign policy expert's judgement on the adequacy of an international posture. . . .

(Rosenau, 1968:202)

The emphasis, within the "moderate" critique on discord between scholars and policy-makers may eliminate the possibility of a conspiracy between the two groups to suppress society's lower classes, but it does not deny the existence of a common ideological orientation. Generally speaking, it is expected that scholars doing research for government agencies' use in policy-making will share the goals and orientations of the government. Here, Lincoln P. Bloomfield notes:

It may thus happen . . . that the government is most likely to choose relatively 'safe' researchers--men who are known to accept the basic premises of ongoing policy and be generally in tune with the Establishment's view of the world. The private scholar is encouraged to range freely through alternatives--but the sponsoring agency believes it will not be betrayed. Even if the latter is receptive to more than just a new and more elegant justification of the current policy line, it trusts that fresh ideas will uproot accepted doctrine only marginally.

(Bloomfield, 1968:190-191)

Thus, the discord existing between scholars and policy-makers stems from interpersonal career contacts, causing frictions and irritations, rather than from fundamental ideological disagreements. While not "hand-in-glove," the

two groups are cooperative and share similar goals, values, and methods.

According to the "conservative" interpretation, both policy-makers and scholars are ideologically to the left of the population as a whole, and therefore are mutually cooperative, reinforcing, and perhaps conspiratorial. They work together, not to suppress the lower classes, but to foist socialism on the country, pursue a soft line vis-à-vis communism in foreign policy, and sabotage the free enterprise system with excessive welfare programs. The critique is not widely disseminated within academic circles, but presumably has an audience equivalent to the ideologically conservative population within the United States.¹⁰

Political Development Research and American Foreign Policy

The principal research case examined in this study is political development research: therefore, it is important to determine the relationship between scholars of political development and policy-makers in the related "ruling class" activity, foreign policy-making. Existing literature addressing the relationship is consistent with the three interpretations described above. That is, the "New Left" interpretation sees a conspiracy of ideology, supported by self-interest, between political development researchers and foreign policy-makers. The "moderate" interpretation sees both groups as mutually governed by the liberal tradition. The "conservative" interpretation sees

in both groups members of the same clique of disproportionately powerful leftists.

Presenting the perspective of the "New Left," the Africa Research Group has published a strident attack on the "collusion" between social scientists and government policy-makers in matters concerning Africa. Many of the social scientists examined in this document are political development researchers. In their publication, The Extended Family, the Africa Research Group says:

. . . we will document the existence of a heretofore unstudied TRIBE--an extended family of interconnected and incestuous 'experts' who, while living off Africa, serve a system pitted against its needs. They are American 'social scientists' comfortably ensconced in the institutional architecture of the American 'intellectual' environment. Nurtured by foundation and government grants, they operate under the cover of the false 'neutrality' of academic scholarship which permits them to camouflage their ideological biases and the strategic-policy implications of their work.
(1969:1)

The publication goes on to identify ideological biases as a product of self-interest, supportive of a neo-colonial policy of imperialist domination toward African nations. The implications of these biases are also discussed, principally that the policies of the ruling class are reinforced and legitimized by the like-minded policy advice contributed by political development researchers.

A. R. Dennon, in a milder, but critical interpretation of the problem, entitled "Political Science and Political Development" accuses the political development literature of being "consciously or unconsciously

prostituted to the goals of American foreign policy. While not precisely stating that the scholars in this case are a tool of the ruling class, that is plainly implied.

(Dennon, 1969:285)

The "Moderate" interpretation of the relationship between political development researchers and foreign policy-makers is stated in great detail by Robert Packenham (1973). The author traces the liberal roots of both foreign policy doctrines and political development theories, noting both their similarities and differences. Packenham himself feels that social scientists were able to maintain "a great deal of autonomy, though they did not use this autonomy very much as a vantage point for basic criticism in the political development field." (Packenham, 1973:298)

In other words, the two groups shared similar goals and ideas, thus they were not at loggerheads. Their cooperation was not designed to impliment policies which would perpetuate their ruling class power and suppress the lower classes: they simply both shared a common ideological ancestry, the liberal tradition.

Unfortunately, a search of the literature does not reveal a "conservative" interpretation of the particular relationship between political development researchers and foreign policy-makers. The reader is referred to the works cited earlier (Goldwater, 1963; 1970; Buckley, 1970; 1973) for a general discussion there of the intellectual-bureaucrat relationship. It could be expected that the conservative

interpretation is consistent: that political development researchers are seen as ideologically compatible with foreign policy-makers. Both presumably are seen as unrepresentatively far to the left, therefore their goals and policies are not reflective of the majority's ideological orientation.

As predicted, the above review of various interpretations of the relationship between scholars and the ruling class has not produced an answer to the question: Is the intelligentsia the tool of the ruling class? The review serves only to clarify the positions taken on the question. In the end, it is a question whose answer nests in the very political ideology it explores.

Ideology and Dominant Disciplinary Norms

Thus far, we have discussed only the relationship between ideology and research. It remains to examine the relationship between ideology and the two other factors discussed in Chapters Two and Three respectively:

1) dominant disciplinary norms and 2) the academic reward structure.

Unfortunately, the nature of the relationship between ideology and dominant disciplinary norms has not been subjected to study. Therefore a discussion of it here is purely conjectural. One might guess, however, that scholars' individual ideologies exert some influence on dominant disciplinary norms at any given time. The

prevailing ideological orientation within any community affects the standards to which that community adheres: there is no reason to expect the relationship not to hold in the case of the ideological orientations of the members of the scholarly community.

In addition, one might expect the relationship to also be characterized by the reverse flow of influence: from dominant disciplinary norms to individual ideology. The scholar is socialized into certain disciplinary norms during professional training; then, depending on personal ideological inclinations, rebels against these norms, wholeheartedly supports them, or takes any number of positions in between. There is little doubt, however, that at least a portion of the disciplinary orientations taught to the scholar as a student will at some point have some affect on his or her personal ideological orientation.

Ideology and the Academic Reward Structure

How does personal ideology relate to the other factor in this model of the causes of research trends, the academic reward structure? Here again, the paucity of hard research on the question necessitates a speculative response. Certainly it is a prominent "New Left" argument that the academic Establishment will not tolerate the entrance of a novice whose ideological inclinations tend to be extreme. The "New Left" critique is concerned principally with cases of leftist scholars punished by the

academic reward structure, arguing in most cases that the punishment addressed the scholar's ideology rather than professional competence. Without accepting this position (in the absence of hard data, or even attainable evidence), it is still reasonable to credit it with some validity. If the academic reward structure does punish ideologically extreme scholars, the implications are twofold. First, prudent scholars with an eye to future career success will modify their own tendencies toward ideological extremism. Secondly, the academic reward structure may, occasionally, have to modify its ideological standards (presuming here that it does hold such standards) in order to accommodate a major change in ideological orientation on the part of the community. Thus, the flow of influence is two-way between the two factors, ideology and the academic reward structure.

The final factor influencing the rise and decline of research trends is the funding structure; the public and private dispersion of assistance to scholars, designed to facilitate research and teaching. The following chapter will discuss this structure in detail, surveying the literature concerning its nature and operations. In addition, in-depth interviews, conducted in order to supplement information available within the literature, will be reported (referenced by anonymous number) throughout the chapter.

CHAPTER III

NOTES

¹Max Scheler has said:

The Age of Enlightenment saw, very one-sidedly, only the conditioning of society by knowledge. It was the great discovery of the nineteenth century and the part of the twentieth century which has elapsed to perceive also that knowledge is conditioned by society.

Max Scheler, "The Sociology of Knowledge: Formal Problems," in James E. Curtis and John W. Petras (eds.), The Sociology of Knowledge (London: Gerald Duckworth and Company, Ltd., 1970), p. 183.

²The initial use of the word "ideology" is usually ascribed to the French philosopher, Destutt de Tracy. Describing de Tracy's work, Gould and Truitt have said:

The earliest consistent use of the term 'ideology,' by Destutt de Tracy, referred to the investigation of the natural sensory origin of ideas, how ideas come to be formed in the mind, and how a general science of ideas (an ideology) could be developed. Also included in de Tracy's conception of ideology was the determination of human nature based on the study of thought, since man alone is a thinking being. This determination, it was hoped, would provide a means for defining general social laws and institutions most conducive for human betterment. The early French conception of ideology was critical and revolutionary in that it was aimed at undermining the traditional religious and idealistic conception of mind with its belief in the soul and its claim that man was essentially a spiritual being. De Tracy advocated a materialistic and biological method that would rid the philosophy of mind of all its metaphysical and supernaturalistic elements.

(1973:7)

³Position on the ideological scale is usually determined by the question: How would you describe yourself politically? The respondent is presented a scale of options ranging from extremely conservative to extremely liberal. An additional measure of ideological position is occasionally used--party voted for at a particular time. In that

case, it is necessary to make certain assumptions regarding 1) consistency of party platforms and 2) voter information.

⁴It should be noted, however, that while the APSA is the principal organization of American political scientists and its membership is surely adequately representative of the discipline, a survey of its membership is not a survey of all American political scientists.

⁵For a discussion of the process of childhood political socialization, see: Kenneth P. Langton, Political Socialization (New York: Oxford University Press, 1969); Robert D. Hess and Judith V. Torney, The Development of Political Attitudes in Children (Garden City, New York: Doubleday and Company, 1968); and Richard E. Dawson and Kenneth Prewitt, Political Socialization (Boston: Little, Brown and Company, 1969). For an individual scholar's reconstruction of his own socializing experiences, see Gabriel A. Almond, Political Development (Boston: Little, Brown and Company, 1970), 5-14. Almond's account is a fascinating integration of childhood, student, and professional socialization experiences, tracing the influence of these experiences on his later scholarly work.

⁶For a discussion of the relationship between professional specialization and personality factors, see: Rollo Handy, "Personality Factors and Intellectual Production," Philosophy of Science, 23 (October, 1956), 325-332; Conrad Joyner, "Political Party Affiliation of University Administrative and Teaching Personnel," Southwestern Social Science Quarterly, 43 (March, 1963), 353-357; Keith W. Pritchard, Sing-Nun Fen and Thomas H. Buxton, "The Political Leanings of College Teachers of Education in Eight Selected Universities and Colleges," Western Political Quarterly, 24 (Sept., 1971), 549-559; C. Wright Mills, "The Professional Ideology of Social Pathologists," The American Journal of Sociology, 44 (Sept., 1943), 165-180; and Anne Roe, "The Psychology of the Scientist," in Paul Obler and Herman Estrin (eds.), The New Scientist (Garden City, New York: Doubleday and Company, Inc., 1962), 82-92.

⁷Hartz explains conflict in American history as quarrels over phantoms or irrelevancies--not a challenge to the acceptance of the basic Lockean tenets. Harry V. Jaffa points out that Hartz holds (implicitly) that "quarrels which are genuine and profound are always quarrels in which the idea of equality is in competition with its opposite." Thus, Hartz is able to discount the high level of fundamental conflicts which have characterized the "consensual" Lockean American society. See: Harry V. Jaffa, "Conflicts Within the Idea of the Liberal Tradition," Comparative Studies in Society and History, V (April, 1963), 274-278.

⁸Two assumptions underlie the leftist critique: a) government policy makers are part of, or act in the interests of, the ruling class, and b) it is in the self-interest of the intellectuals to lend support to the ruling class, either 1) because it is part of it, or 2) because it aspires to share its power. The specific motivations of intellectuals are not presented in a consistent fashion across the critique. The proper role of the intellectual is "to speak the truth and expose lies," disregarding self-interest or ambition, and acting in the cause of the oppressed in society. (Chomsky, 1969:325) Chomsky goes on to say,

If it is plausible that ideology will in general serve as a mask for self-interest, then it is a natural presumption that intellectuals, in interpreting history or formulating policy, will tend to adopt an elitist position, condemning popular movements and mass participation in decision-making, and emphasizing rather the necessity for supervision by those who possess the knowledge and understanding that is required (so they claim) to manage society and control social change.
(Chomsky, 1969:72)

Two interesting pieces discussing the role of bourgeois ideology in social science research in the areas of 1) peace research and 2) race relations, e: Herman Schmid, "Peace Research as a Technology for Pacification," Proceedings of the International Peace Research Association, Third General Conference (Assen, The Netherlands: Van Gorcum & Comp. N. V., 1970); and Robin Jenkins, The Production of Knowledge at the Institute of Race Relations (London: National Labour Press Ltd., 1971).

⁹For a case study of the exchange of respect between historians and policy-makers, see Louis Morton, "The Cold War and American Scholarship," in Frances L. Loewenheim, (ed.), The Historian and the Diplomat (New York: Harper and Row, 1967), 123-169.

¹⁰For the past decade, the conservative position has been best articulated by Barry Goldwater and William Buckley. See: Barry M. Goldwater, Why Not Victory? A Fresh Look At American Foreign Policy (New York: Macfadden-Bartell, 1963); Barry M. Goldwater, The Conscience of a Majority (Englewood Cliffs, N. J.: Prentice-Hall, 1970); William F. Buckley (ed.), Did You Ever See a Dream Walking: American Conservative Thought in the Twentieth Century (Indianapolis: Bobbs-Merrill, 1970); and William F. Buckley, Four Reforms: A Guide for the Seventies (New York: Putnam, 1973).

CHAPTER IV

THE ACADEMIC REWARD STRUCTURE: ELITE CONTROL

The study of sociology of science has both demystified the aura of absolute rationality and impersonality which surrounded the natural sciences and has exposed science as a social phenomenon: it has examined its "essential social character, its socio-historical development, its patterns of organization, the social images of science, social influences on the process of discovery, and the social responsibilities of science." (Barber and Hirsch, 1962:1) The findings of sociology of science have direct relevance to the study of the social sciences, for much of what has been found to be characteristic of the natural science community can also be generalized to the social science community. Thus, it is important that, while not losing sight of the distinction between the natural and social sciences, research from each be considered for possible insights into the causes of the rise and decline of research trends.

The most general proposition emerging from the sociology of science is that scientific eminence and recognition are not so rationally and objectively bestowed as the public has been led to believe. While objective standards of scientific excellence play an important role,

another criterion competes for prominence as a determinant of scientific recognition and eminence. That criterion is the scientist's position in the stratified social structure of science, a structure characterized by elite dominance, and elite control. The same social structure is also influential in the emergence of research trends.

In the following discussion, the natural sciences and the social sciences will not be distinguished except where such a distinction reveals important differences between the two types of science. Certain shared characteristics are assumed: both natural and social scientists of interest here work in an academic setting, the social structures of both communities are similar with a few exceptions, dissemination of information in each community features similar communications networks, each is characterized by entrance requirements, a socialization process, and a period of apprenticeship.

Although the scientific social structure is interesting at several different levels of analysis (individual faculty, regional group, etc.), most research has examined the national scientific community, for it is at the national level that each discipline is most visible to the public, bestows its most important prestige and recognition (with the rare exception of international recognition), and targets the readership for its major journals and other publications. Conventions and professional meetings tend to

be organized for participation by the national scientific community, albeit perhaps those of the national community whose specialty lies in some particular field.

The following discussion will be divided into three parts. The first will deal with the nature of the scientific community's social structure, and the role of this social structure in the assignment of eminence and recognition. The second section will examine and critique the control which the elite of the scientific community exercises and the third will discuss the influence of the scientific social structure on the life cycle of the research trend of political development.

The Social Setting of Academic Science

The most well-known studies of the social structure of the academic setting (Veblen, 1918; Znaniecki, 1940; Wilson, 1942; Lazarsfeld and Thielens, 1958; and Coser, 1965) agree that the academic community is stratified. The upper stratum, or elite of the community, is characterized by older, established, eminent scholars; the lower stratum is characterized by young apprentice scholars, as yet unrecognized and with little prestige. However, this simple stratification is complicated by several intervening factors. A full professor with a history of low productivity and little recognition, who is associated with a low-prestige college or university may have lower status than a young scholar, a graduate of an elite educational

institution, and known to be the student of a famous scholar. A scholar assigned to one stratum on the basis of rank, eminence, prestige of academic affiliations, and other established criteria may be more accurately assigned to another stratum on the basis of personal attractiveness, teaching reputation, or friendship associations. Thus, while there is widespread agreement that the academic science community is stratified, the basis for the assignment of a scholar to a particular strata is complex: the reader should be suspicious of facile categorizations.

Sociology of science has tended to associate the following characteristics with the elite of the scientific community; productivity, scholarly eminence, recognition, prestigious graduate training, prestigious academic affiliation, and the accumulation of awards, grants and distinctions. Measurement of many of the characteristics is shrouded in contradiction and ambiguity; for instance, productivity is usually measured by volume of publication, but occasionally it is also measured by quality of publication in addition to volume. Or, in another case, the concepts "recognition" and "eminence" usually distinguish two separate phenomena, but are sometimes used interchangeably.¹

Existing research concerning the social structure of the academic science community indicates a high level of congruence among the factors which sociology of science has associated with the elite of the community. Crane

(1965) found a strong relationship between productivity and recognition,² and noted the status of graduate school affiliation predicts better to productivity than does current academic affiliation.³ Axelson (1960) found a strong association between prestige of graduate school training and high productivity. Caplow and McGee (1958) found a relationship between prestige of institution from which the scientist received his doctorate and his later eminence. Crane found no such relationship, but found that recognition was related to the prestige of the scientist's current academic affiliation (1965:709). Cole and Cole (1967) found a high correlation between quality of research and recognition, and a slightly lower correlation between quantity of research and recognition.

Congruence among the indicators of elite status provides evidence of the existence of an elite strata within the academic community of science, but provides no information regarding the process which creates and renews the elite. Crane describes the process:

Thus the training of a scientist may be regarded as an increasingly selective process in which most of the best students are channelled into the best graduate schools and, in turn, the best of these are selected for training by the top scientists. This highly select group becomes the next generation's most productive scientists, most frequently chosen for positions in major universities. (1965:705)

More important than the accumulated sociology of science evidence of an elite is the significance of that elite to the practice of science. Why, to whom, and in

what respects is the elite's existence consequential? Certainly it is expected that the elite of any community receives homage and respect, but if the canons of science as they have been popularized--especially regarding the application of strictly objective standards to each piece of scientific knowledge produced--are indeed followed, then membership in the elite of the community (or in any other social strata for that matter) should make no difference in the assignment of recognition for scientific achievement. Our expectation, based on the supposed objectivity of scientific standards, is that membership in the elite is assigned objectively and must continually be justified by the production of work of the highest scientific excellence.

Sociology of science has disrupted this myth, however, by its exposure of two crucial aspects of the social relations among scientists. First, the elite of the academic community in science exercises important control over many aspects of the scientific career, which makes it a disproportionately powerful segment of a supposedly egalitarian group. Secondly, the existence of a halo effect, created by past achievements, distorts the objectivity of scientific judgement when applied to a member of the elite. This last dynamic has been called the "Matthew effect" and will be discussed later. At this point, a review of the sociology of science findings concerning elite control of the reward system will clarify

for the reader the importance of membership in the elite to the career of a scientist.

Elite Control of Rewards

The exercise of social control by the elite of the science community has been interpreted in two ways. By one interpretation, the use of social control by the established elite explains the high standards of scientific excellence which characterize the natural sciences and to a large extent accounts for their unusual rate of achievement. While this interpretation admits the procedures used to select and initiate the elite are selective, they are selective strictly on the basis of scholarship rather than any other criterion. Following this perspective, the scientific control system is defended as the most fair and efficient reward system available within academic circles at the present time.⁴

The alternative interpretation charges that the shroud of objectivity which surrounds the natural sciences is a myth and obscures from the public the truly subjective nature of the scientific selection and reward processes. This interpretation criticizes the middle class bias of the system, its cronyism, the subjective assignment of merit to scientific work, the competition for notoriety and credit among practitioners, its emphasis on conformity and on quantity rather than quality of research. Before detailing these criticisms, a review of the various types

of social control exercised by the elite will acquaint the reader with the mechanics of the scientific community's reward structure, insofar as that structure has been mapped by the sociology of science. The following discussion focuses on four types of social control: 1) control of access to elite institutions, 2) control of sponsorship of young scientists, 3) control of access to publication, and 4) control of academic promotion. The four areas of control are the major arenas of the "old boy network," a phrase used to describe a web of personal contacts which is the basis of much of the decision-making in these areas of control.

Access to elite institutions⁵ is critical to an academic career which aspires to eventual elite status. For the teacher, these institutions commonly offer a lighter teaching load than do minor universities and in addition, usually have better facilities for research, and are characterized by an atmosphere of greater intellectual stimulation from colleagues and students who are outstanding in their fields. To be brought onto the faculty of an elite institution can upgrade an academic career to a higher stratum, in many cases even if tenure is not subsequently secured. The assumption that these schools are able to choose the "cream of the crop" assures that those they hire benefit by association from the institution's prestige.

Hiring decisions in elite institutions are made by the faculty of the institution, which itself represents a segment of the elite of the science community. In this fashion, the elite controls access to elite institutions at the faculty level. At the graduate student level, the process of control is nearly identical. The best training, at least that which predicts to future success with the greatest accuracy, is to be found in elite institutions, where the elite of the community controls entrance decisions and in effect "screens" potential future members. For the student, admission to a major university can make or break future career success: in effect, when applying to an elite graduate school, the student is petitioning for entrance into the elite at some future date. Of course, there is always the possibility that the student may at some point be "screened out" and never attain that position in the academic social structure.

The "sponsor" is the student's principal advisor and can have an important effect on the socialization process of the young scientist.⁶ It can be strongly argued that the position in the social structure of the student's sponsor is an important predictor to the student's future scientific productivity. An eminent sponsor with a prestigious reputation may both give better guidance and training and also have a broader network of contacts to exploit on the student's behalf than does the less prestigious

sponsor. The elite of the science community, those whose prestige and contacts are greater than others in the community, thereby control the "odds" of certain novitiates' own ascendance into the elite of the future.

Diana Crane devotes a large section of her discussion of the scientist's socialization to the effect of the eminent sponsor on the career of a young scientist. She found no relationship among the prestige of the sponsor, the development of a lasting research interest in a problem area during graduate training, and the productivity of the student, but she does find that:

It could be argued that the prestigious sponsor is able to secure teaching positions for his students in high-ranking institutions, which in turn provide more favorable settings for later productivity. There is a relationship between having had a prestigious sponsor and being currently associated with a prestigious institution.

(1964:109)

Caplow and McGee (1958) had found similarly that good "contacts" made in the best schools are an important factor in the young scientist's future success. This mixed evidence allows only the cautious conclusion that sponsorship by a member of the elite is important to the future success of a young scientist.

Because publication is essential to academic achievement, access to publication is imperative for the young scientist. Control of access to the most prestigious journals rests in the hands of the elite of the science community. Therefore, here again the elite may screen young

scientists and help or hinder their advancement. In addition, the elite stratum also tends to publish in the most prestigious journals. In this sense, both the selection and the reward steps of the publication system are in the hands of the elite.

It has been suggested (DeGrazia, 1963) that journal editors are the "gatekeepers of science" insofar as they screen information which enters one of the most important channels of communication among scientists.⁷ An excellent and representative description of the procedures used in the review of manuscripts for publication is Austin Ranney's account which he published in the American Political Science Review, while its editor, in 1969.⁸ Ranney clearly states the objectives of the reviewing procedures: to select from among the manuscripts submitted those which are most suitable for publication,⁹ to work the manuscripts selected into the best possible shape, to give each author a serious critique of the work submitted, and to get a decision on publication within a short period of time. The agents or "referees" chosen to carry out these objectives are the experts of the discipline, according to the editor's judgement or the judgement of those who work with him. It seems obvious that in order to have achieved the status of "expert," one must be an established scholar, or at minimum have produced some scholarly work which gives one the glow of expertise.

Manuscripts are sent to reviewers, therefore, who are members of the academic elite. In most cases the author's anonymity is preserved by removing his name from the manuscript when it is sent out to the reviewer, and in this way the reviewer is prevented from exercising his own personal tastes for the scholar involved. He must read the manuscript "blind," theoretically not knowing if the author is a graduate student or a full professor. There is no constraint, however, on his freedom to exercise his scholarly prejudices. In her examination of three scientific journals, Crane found that:

As a result of academic training, editorial readers respond to certain aspects of methodology, theoretical orientation and mode of expression in the writings of those who have received similar training . . . In all three journals, the majority of authors and editors have degrees from major universities. This suggests that editors and contributors share common viewpoints based on training rather than on personal ties.
(1967:200)

Crane does not suggest that the similarity of viewpoints indicates any sort of a conspiracy among the graduates of major universities to limit access to elite or prestigious journals to those who also graduated from major universities. She merely points out that the shared mode of training leads to this end, without any intentional manipulation on the part of editors or referees. In her research, which sampled both natural and social scientists, she found that

Examination of the academic characteristics of contributors and editors of three scientific journals

indicates that the distribution of characteristics such as academic affiliation, doctoral origin and professional age of contributors to scientific journals is similar to the distribution of these same characteristics among journal editors. Anonymous evaluation of articles does not change this relationship.

(1967:200)

It follows logically from the similarity found among particular journal editors, readers and contributors that the more prestigious journals will be characterized by the most prestigious editors, referees and contributors. Not only do the elite of the science community have a better opportunity to publish in elite journals than do non-elite members, but it can be argued that they have a better chance across the board, in journals in general, to be published. Crane (1967) has found an association between membership in the elite of the community and "ease of publication." Doris West Goodrich (1945) has also found, in her study of the American Sociological Review, that the elite of the community have a better acceptance rate regarding journal publication. She bases this conclusion on her finding that those from "major" universities have a markedly higher rate of acceptance.

Tentative conclusions from the research reviewed above support the notion that the elite of the community both control access to publication, and enjoy ease of access to publication. There is no evidence of a conspiracy in the control, however. A more reasonable explanation points to a shared mindset, produced by mutual attendance at major or elite institutions at the doctoral level.

The final facet of elite control to be reviewed here is control over academic promotion. Historically it has always been the case in American universities and colleges that higher ranking professors pass judgment on the acceptability of lower ranking professors for promotion. A negative decision may be extremely harmful to an academic career since if, after a prescribed length of time, a junior faculty member is not given tenure--and usually an accompanying promotion--that person will be dropped from the faculty. Consideration for promotion is clearly a point at which the elite of the science community can exercise their power to screen those eligible for access to that elite. However, even consistent promotion is merely a necessary condition for membership in the elite of the science community, not by any means a sufficient condition. The rank of full professor must be attained at a major institution, and a large portion of the other elite characteristics mentioned earlier must be attained before elite membership is assured.

Ostensibly, the elite faculties of major universities place the highest value on number and prestige of publication in assessing a junior academic for promotion. It has already been demonstrated above that it is the same group which exercises control over access to publication. Therefore, in the particular case of promotion, the junior academic is simultaneously confronting two points in the

screening process for entrance into the elite stratum. At either of these points, as at the points of admission to an institution for graduate training or admission to the faculty of a major institution, the young scientist may either be discouraged from continuing or eliminated from competition for future elite status.

Two principal criteria underlying academic evaluation have been mentioned above: 1) objective standards of scholarly excellence, and 2) positive recognition of a shared perspective between researcher and evaluator which presumably creates some prejudice for that research on the part of the evaluator. A third criterion lies in the "old boy network," a form of sponsorship and "in-group" recognition on the part of those who are members of the elite. The nexus here is personal ties and contacts, and for that reason, it is difficult to research the functioning of this criterion. Historically, the phrase referred to a certain consideration or special recognition awarded mutually among those who attended the same elite university or college. The phrase "old boy network" now has expanded to include all favors or recognition given on the basis of friendship, acquaintanceship, or even mutual respect: it is a phrase so broadly used that its precise meaning is difficult to specify, and hence difficult to measure. While no conclusions regarding the relative importance of the "old boy network" as a factor in the exercise of elite

control can be ventured due to the paucity of research on its functioning, the term is, nevertheless, a universally recognized factor, and one which must be taken into very serious consideration in any analysis of elite control in the academic selection and reward system.

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The selection and reward system of science, as described above, has inspired passionate criticisms and equally passionate defense from varied sources. Defense of the system rests, as Merton (1957) notes, on the conviction that the scientific structure of technical and moral norms of operation implement the final objective, the discovery of knowledge. Many scientists see the mores of science as valid "not only because they are procedurally efficient, but because they are believed right and good." (Merton, 1957:552) The existence of "checkpoints" is defended as an efficient method of ridding the elite strata of those who do not meet its high standards. Thus, apologists for the system do not deny the role of screening in the determination of elite membership; they simply perceive it to be a fair, efficient, and egalitarian screening process, based on criteria agreed to by the community as a whole.

Criticisms of the selection and reward system of science focus on those inequities which defy the image of fairness and equal opportunity popularized by supporters

of the science community. One criticism, for instance, charges that the system has a built in middle-class bias which minimizes the chances of lower class students to eventually be admitted to the elite strata of the community: that it is middle-class students who are favorably placed so that they begin with a higher probability of entrance into the elite. As Peter M. Blau states it:

The findings suggest that academic institutions with superior reputations exhibit some class bias in faculty recruitment. One reason for such bias is that elite universities recruit their faculties primarily from graduates of elite universities, as a number of studies have shown, and middle-class students have a better chance to go to elite universities than others.
(Blau, 1973, p. 95)

Crane (1969) also found a middle class bias in academic recruitment.¹⁰ Lazarsfeld and Thielens (1958) found a relationship between productivity and social origin and noted that those from professional and managerial families were somewhat more successful than those who are not. Veblen (1918) discussed at length the advantages of entering academics with an independent inherited income, and the difficulties which befall the academic who does have such a supplementary income.¹¹

Another frequent criticism of the reward structure is that it overemphasizes quantity and speed of publication at the expense of quality. According to this argument, the pursuit of quantity is inherently contradictory to the development of quality in research. Lewis A. Coser says:

The career of the academician typically proceeds by a series of slow steps through the ranks up the academic ladder. This process may be an impediment to the achievement of intellectual excellence because the requirements for academic advancement and the optimal conditions for the advancement of knowledge do not necessarily coincide. (1965:281)

Coser goes on to explain that built-in pressures to publish may not allow for the ripening of long-term projects. Wilson earlier stated a similar position, but blamed the administrative apparatus of the academic community for the sacrifice of quality to quantity, citing administrators' efforts to stimulate productivity among the faculty as destructive to thorough research. (Wilson, 1942:19) Each criticism identified sheer scarcity of time as a serious impediment to excellence in intellectual activities.

Yet another object of criticism is the use, by journal editors, of "referees" (experts in a particular area of research) as consultants concerning the publication of research; the procedure described by Ranney (1969) above. Zuckerman and Merton (1971) discuss the significance of this system for individual scientists, scientific communication, and the development of science. The authors point to the dangers which inhere in the referee's anonymous status, as his identity is usually known only to the editor of the journal and to the author of the article being reviewed. Although the referees are, by objective standards of science, well-qualified to judge other scientific work in their field in that they are the leading

authorities in that field, Zuckerman and Merton point out that

The basic and, it would seem, thoroughly rational process of selecting experts as referees makes for its own stresses in the system. Some scientists have argued that it is particularly the experts who can exploit their fiduciary role to advance their own interests and so are most subject to possible conflicts of interest. (1971:97)

The referees could, in short, use the protective cloak of anonymity from the public to squelch other work in their field, to steal ideas before they are published, to "sit" on research in order to provide an opportunity for their own research or that of their students to reach publication first, or deny the validity of a piece of work in order to be the publicly acknowledged discoverer of a finding. The temptation to exploit the position of "referee" stems from the importance of priority in the assignment of recognition for scientific work: the scientific community usually recognizes and rewards only the first discoverer of an important finding or a theoretical breakthrough. Therefore, it is perhaps more accurate to criticize the dynamic which leads to corruption within the referee system than to criticize the referee system itself.

Elsewhere, Merton has discussed another criticism of the reward system in the science community; the operation of what he has called the "Matthew effect." (Merton, 1968) Based on the biblical passage "For unto every one that hath shall be given, and he shall have abundance:

but from him that hath not shall be taken away even that which he hath" (Matthew 25 xxix), the "Matthew effect" provides that the scholar who is more prominent will receive more attention and praise for his work than will a less eminent scholar for equal or better work. The result is that if a scholar or a series of scholars takes up an area of research, the work will receive attention in proportion to that scholar or scholars' standing in the academic community, although of course this is not the only factor determining the amount of attention a piece of work receives. As Merton says, when discussing the Matthew effect among the natural sciences:

Contributions made by scientists of considerable standing are the most likely to enter promptly and widely into the communication networks of science.
(1968:60)

In his interviews with Nobel laureates, Merton observed a common theme running throughout the interviews:

. . . eminent scientists get disproportionately great credit for their contributions to science while relatively unknown scientists tend to get disproportionately little credit for comparable contributions.
(1968:57)

Of course, were this a law-like statement, there could be no career-building for the young scientist: it is not inevitable that a contribution of high quality made by a young scientist will in every instance be ignored or passed over in favor of work done by a member of the established elite. However, Merton does emphasize the tendency for the disproportionate assignment of credit; a finding also reported by Crane (1967) and Cole (1970).

Political Development Research and the Academic
Reward Structure of Science

Here we shall treat the influence exerted by the academic reward structure on the trend of political development research, presenting to the reader concrete examples of the possible linkages between the reward structure and the life cycle of a research trend, as well as possible linkages among the four factors which cause the rise and decline of research trends. A sociology of science study might treat the relationship in detail, by rigorously tracing the career pattern of each scholar involved in political development research and relating this pattern to each scholar's publications, but such a study is not practical here: the following discussion will speak in generalities, and will lack detail, for it is designed simply to familiarize the reader with an illustrative case of the influence of the reward structure on a research trend.

Foremost among influences exerted by the academic reward structure is the pressure it exerts on scholars to "publish or perish." Publications are mandatory for membership in the elite, and "expected" at lower levels of the stratified community. Thus, there is a built-in career mandate for the academic to do research and to publish the results, which inspires an ongoing body of political science research publication, some of which would appear as a result of other factors, yet much of which is an artifact

of career pressures stemming from the academic reward structure.

A second critical factor in the life cycle of a research trend is the extent to which it is supported by the elite stratum of the academic community. In the case of political development research, for instance, it seems clear that the study of political development was adopted by the elite of the political science discipline. Indicators of elite support include: the support of political development research by prestigious universities and colleges, the publication of the research in elite journals, and the involvement of elite scholars in the field.

Prestigious institutions, such as the University of Chicago, Harvard University, and Princeton University, established institutes or designed programs specifically emphasizing the study of political development.¹² Though at first only a few elite scholars' field of specialization corresponded to the trend, they were complemented by another available group of elite scholars whose reputation for excellence in other fields of research had been established during World War II and the immediate post-war years. Some scholars of the latter group were recruited to the elite institutions where political development programs were gearing up, and in turn were to influence students, some of whom subsequently took up the study of political development. As described earlier, students of prestigious

sponsors are somewhat more likely to obtain the better teaching positions, and thus are favorably disposed to future entrance into the elite of the discipline. Therefore, adoption of political development research by the elite was to have consequences for the trend, not only at the time that the trend began, but farther into its life cycle, when the students of the elite went out into the community to teach.

Because the academic elite controls access to the more prestigious disciplinary journals, the content of these journals serves as an indicator of the type of research supported by the elite at a given moment. Thus, both the American Political Science Review and World Politics might serve as barometers of the standards which the elite apply to research, the first in political science as a whole, and the second in the fields of comparative politics and international relations. Both journals consistently published research on the subject of political development during the 1960's, thereby indicating support for this sub-field by the elite of the political science discipline.¹³

What was the role of the "Matthew effect," if any, in the political development research trend? Its influence seems to have been moderate; the history of the development of the trend, as described in Chapter I, has set out the reasons. Few established elite scholars were well versed

in the politics of the Third World at the time political development began to emerge as a research trend. Those having expertise in the area were highly visible and for a time, were disproportionately influential in political development research. However, much of the trend's most important literature was the work of young scholars who adopted the study of political development at a time when their research interests were originally being defined. Many of these young scholars were, at the time, in the "pipeline" into elite status; that is, they had obtained degrees from elite institutions and had--probably in many cases through the auspices of an elite sponsor--secured teaching positions in elite institutions. Their intellectual flexibility was still much in evidence and (as one suspects often happens in both the natural and the social sciences) they were actually to develop the ideas proposed by the elite older scholars who brought the subject to wide attention by dint of their attention to it. The case seems to be that the "Matthew effect" was fairly important at the early stages of the trend, then later diminished in significance.¹⁴

Presumably, the "Matthew effect" could work in reverse of the fashion described by Merton (1968). Indeed, that seems to be the case in political development research. Not only is there some qualified evidence that the adoption of the study of political development was advocated by a

visible elite of the community, giving the trend momentum and prestige at its onset, but evidence also exists that the withdrawal of support for political development by the elite lowered its visibility and prestige, with a concomitant reduction of its standing in the discipline. The decline of the political development trend, dated in Chapter I as characteristic of the early 1970's, seems at first blush to have been accelerated by the withdrawal of the elite of the community from this area of research. In the case of such a withdrawal, the "reversed Matthew effect" might explain how it is possible for a trend to decline in prominence while a large volume of research is yet being generated within that trend. In sum, both the exercise of elite control and the disproportionate visibility of elite research were influential factors vis-à-vis the life cycle of the political development research trend.

Comparing the visibility of neo-Marxist research on Third World politics with that of political development research in the traditional perspective, we find that the neo-Marxist perspective was not, during the 1960's, adopted by the elite of the community. Few well-known political scientists wrote from the neo-Marxist perspective: prestigious journals did not publish neo-Marxist research on development.¹⁵ Rather, journals such as Science and Society (which did feature such research) had low circulation and were relatively peripheral to the communication network of

political science. Prestigious institutions did not lend support to the neo-Marxist perspective, although they occasionally did support the work of individual scholars on the basis of their right to academic freedom, which when compared to the attitude toward research of the dominant perspective, can be summarized as the difference between tolerance and encouragement.

The academic reward structure exerts influence on other of the four causal factors related to research trends. Specifically, it influences 1) dominant disciplinary norms and 2) the funding structure. In these relationships the role of the "Matthew effect" is quite evident, at least in the case of political development research.

The elite of the political science discipline, due to their disproportionate visibility and prestige, are able to cause a shift in disciplinary norms, when in fact the number of members of the community committed to the shift is small. Such is the case in political development research. The influential article by Gabriel Almond (1960) proposing that political science adopt the structural-functional approach, discussed in Chapter I, caused a shift in research orientation within the discipline which certainly reflects, in part, the operation of the "Matthew effect." Then as now, Almond was a well-known and prestigious scholar: his intellectual proposals were given an attentive hearing, quickly entering the political science

communications network. Thus, the operation of the "Matthew effect" based on the academic reward structure which is stratified and characterized by a powerful elite, may be influential as a determinant of the selection of dominant disciplinary norms at a given point in the life cycle of a research trend.

Similarly, the elite of the political science discipline are able to exert influence over the allocation of funding for research purposes disproportionate to their actual numbers. Here again, the "Matthew effect" is the explanation, albeit the consequence is not disproportionate elite recognition, but disproportionate elite influence. Funding agencies are anxious to maximize their own investment and support the best scholarship available: often in order to identify the best scholarship these agencies consult with the academic community. Those within that community tapped as consultants to funding agencies are most often the elite of the community, members of the most visible and recognized stratum. In their role as consultant, the elite tend to apply disciplinary standards which reflect their own personal intellectual tastes. In this manner, they exercise disproportionate control over the allocation of research support within a discipline.

The importance of elite control over funding support depends on the degree to which supplementary funding is necessary in order for the research to be feasible. Because

foreign travel for field work was often necessary in the case of political development research, examples of elite control of funding decisions regarding political development research are numerous. However, a recitation of such examples would be relatively meaningless to the reader without an appropriate review of the funding structure which characterizes the academic science community: Chapter V will present such as a review, with emphasis on the funding structure of social science research.

CHAPTER IV

NOTES

¹Crane (1965) has measured recognition by the number of honors won, and seems to use recognition interchangeably with eminence, which is never clearly defined. Cole and Cole (1967) also measured recognition by the number of honors and awards won, but also included in this variable; 1) measures of prestige of department, 2) tenure/non-tenure status, and 3) level of familiarity of other scientists with one's work. Strauss and Radel (1969) measured eminence by counting the frequency of references to an author's work in widely used textbooks, a technique similar to the use of a citation index, employed by Cole and Cole (1967) to measure quality of research.

²The same finding is reported by Lazarsfeld and Thielens (1958:9), where a correlation is reported between productivity and eminence. The authors found that "other indicators of leadership are closely associated with productivity." (p. 8)

³A later study, however, found that the best predictors to later productivity are; 1) age at first publication, and 2) publication before the Ph.D. In this case, the sample was 2,205 holders of the Ph.D. in sociology, whose publications were examined for the years 1940-1970. (Clemente, 1973)

⁴For a statement of this perspective, see Donald T. Campbell, "Objectivity and the Social Locus of Knowledge," in R. S. Cohen and M. W. Wartofsky (eds.). Boston Studies in the Philosophy of Science. Dordrecht, Holland: D. Reidel, vol. 10, 1972.

⁵Elite institutions are usually considered to include the top twenty institutions, as ranked by standard sources, employing indicators such as 1) size of library, 2) number of faculty with the Ph.D, 3) number of graduate students, etc.

⁶One suspects that the notion of a one-to-one student-sponsor relationship is an over-simplification of the reality of both the natural and social sciences. In both cases, it seems equally common for the student to be

advised by more than one person, and therefore to lack a sense of having one "principal advisor."

⁷There is a sizable literature on the communication system in science which emphasizes the importance of the journal as the principal channel of communication among members of the same field of research. The explanation for the journal's importance lies in its ability to communicate recent findings to others in the field in a shorter time than required by the publication of a book. There are of course other means of communication which are even faster than the use of the journal: these include the informal exchange of papers, personal communication, and the exchange of information at professional meetings. For a discussion of the communication system in science, see: Howard F. Fehr, "Communication of Scientific Thought," in Paul Obler and Herman Estrin (eds.), The New Scientist: Essays on the Methods and Values of Modern Science (Garden City, New York: Doubleday and Company, 1962), pp. 199-124; Warren O. Hagstrom, The Scientific Community (New York: Basic Books, Inc., 1965), esp. pp. 42-68; Stephen Cole and J. R. Cole, "Visibility and the Structural Bases of Awareness of Scientific Research," American Sociological Review, 33 (June, 1968), pp. 397-413; Diana Crane, "The Nature of Scientific Communication and Influence," International Social Science Journal, 22 (1970), pp. 28-41; Diana Crane, Invisible Colleges (Chicago: University of Chicago Press, 1972); and Richard D. Whitley, "The Formal Communication System of Science: A Study of the Organization of British Social Science Journals," in Paul Halmos (ed.), The Sociology of Sociology (Keele, Staffordshire: University of Keele, 1970).

⁸Austin Ranney, "Procedures for Reviewing Manuscripts," American Political Science Review, 63 (March, 1969), pp. 168-169. Also see Harvey C. Mansfield, "Toward a Definition of Editorial Policy for the Review," American Political Science Review, 56 (March, 1962), pp. 129-138.

⁹Doris W. Goodrich (1945) asked editors of the American Sociological Review to formulate criteria for the acceptance or rejection of articles for publication. The criteria formulated were:

- 1) Relevance of subject matter to sociology
- 2) Degree to which material within the field of sociology is sufficiently advanced to be of interest to professional sociologists
- 3) Level of technical competence of manuscript as revealed by:
 - a) clarity, coherence and organization
 - b) proper use of terms, concepts and definitions
 - c) accuracy in the use of statistical and other methodological techniques

- 4) Membership of the author in the American Sociological Society
- 5) Representativeness of manuscript in terms of institutional affiliation of author by region and by type of institution
- 6) Representativeness of manuscripts in terms of the varied interests of the readers of the Review

¹⁰However, Wilson (1942) makes the statement that in academics there is much recruitment from below middle class ranks.

¹¹The early date of Veblen's book calls for a caution to the reader at this point.

¹²The Committee for the Comparative Study of New Nations, based at the University of Chicago, began its work in 1959-1960 under a planning grant from the Carnegie Corporation. The Center for International Affairs at Harvard University published its first annual report in 1957. The Center of International Studies of Princeton University has been influential throughout the period of political development research.

¹³Examples include: Leonard Binder, "National Integration and Political Development," American Political Science Review, 58 (1964), pp. 622-631; Samuel Huntington, "Political Development and Political Decay," World Politics, 17 (April, 1965), pp. 386-430; Seymour Martin Lipset, "Some Social Requisites of Democracy," American Political Science Review, 53 (March, 1959), pp. 69-105; Donald J. McCrone and Charles F. Cnudde, "Toward a Communications Theory of Democratic Political Development: Further Test of a Causal Model," American Political Science Review, 61 (1967), pp. 72-79; S. N. Eisenstadt, "Modernization and Conditions of Sustained Growth," World Politics, 17 (1964), pp. 157-181; Political Science Review, 61 (June, 1967), pp. 417-427; Gilbert R. Winham, "Political Development and Lerner's Theory: Further Test of A Causal Model," American Political Science Review, 64 (1970), pp. 810-818.

¹⁴Examples of scholars who came to the field of political development with established reputations are Merle Kling, Reinhard Bendix, Rupert Emerson, and S. N. Eisenstadt. Examples of the "new generation" of political development scholars are David Apter, Aristide Zolberg, Lucian Pye, and Leonard Binder.

¹⁵Exceptions occasionally arose, such as the work of Irving Louis Horowitz, which was widely disseminated during ¹⁵Except's, and an article published in World Politics in 1968: Ali A. Mazrui, "From Social Darwinism to Current Theories of Modernization," World Politics, 17 (1968), pp. 69-83.

CHAPTER V

FUNDING OF RESEARCH: SUPPORT vs. INFLUENCE

Thus far, three causes of fluctuations in academic research trends have been discussed: 1) dominant disciplinary norms, 2) individual and societal ideology, and 3) the academic reward structure. The influence these factors exert on academic research trends has been examined, using the case of political development research during the 1960's as an illustrative example. We now turn to the final explanatory factor in the causal model: the societal reward structure.

What is the societal reward structure and what is its influence? The phrase itself represents a system of reward-giving and reward-accepting between academic scholars and institutions and individuals outside of the academic community. There are a multitude of points of contact between the academic community and its larger society: here, only the most important contact points will be discussed--occasions of direct support of academic endeavor on the part of a sponsor outside of the academic community. "Direct support" may be in the form of money to facilitate research, or in the form of consultantships or other positions involving some remuneration, whether

in money or status. The importance of the support lies in the influence, direction, or pressure it may exert on the work of the academic community, which in turn partially determines the prevailing degree of academic autonomy.

Historically, two positions have been taken on the issue of academic autonomy. According to the first, loss of autonomy is perceived as threatening to the integrity of the academic community. To serve society, it is feared, is to become its servant and tool, specifically a tool of the power elite of the society. Irving Louis Horowitz states the position clearly:

When a breakdown of autonomy occurs, when policy questions or ideological requirements prevail, the deterioration in the quality of the social sciences is a certain consequence. Policy places a premium on involvement and influence; science places a premium on investigation and ideas. The issue is not so much what is studied, or even the way an inquiry is conducted, but the auspices and purposes of a study.

(Horowitz, 1972:392)

In order to avoid the loss of autonomy, academics may "boycott" all opportunities offered by outside institutions or individuals, thereby avoiding any influence on scholarship introduced by such opportunities. Or they may simply be suspicious of the motivations behind the awarding of money, and wary of the taint of "pressure" which may accompany it. The principal value underlying this position might be called academic self-determination.

On the other hand, the second position sees academic autonomy as a frivolous luxury, enjoyed by the academic community at the expense of the larger society.

From this point of view scholars are a national resource, which must be tapped by the society for their skills in societal problem-solving. Research done within the academic community should be relevant to societal needs: to be useful in policy matters should be considered an academic responsibility. Harold Orlans describes this position, as it was stated by Lyndon Johnson:

In his address on Government and the Critical Intelligence, President Lyndon Johnson (1966) took a strong stand on the public responsibilities of intellectuals. Because of their special education and talent, Johnson said, intellectuals have a special obligation to advise the government about what is right and wrong with its programs: 'Their judgement may be wrong, and they must live with that knowledge as other men do who have been chosen by their fellow citizens to exercise the powers of government. Their judgement may be right and still not be accepted. . . . That is a risk that they all take along with everyone else. But they must provide it; it is an obligation of responsible intellect.'

(Orlans, 1973:81)

While a certain degree of research restraint is the norm for the practice of research in many non-academic institutions (which depend on research contracts), the academic scholar is accustomed to setting his or her own research agenda, enjoying freedom to follow personal research interests, and retaining complete control of the final research product. Each of these conditions, as well as others, may be violated when the academic scholar "leaves" the confines of the community to conduct policy-dictated research.

Midway between the two positions just described is a moderate position: social science is a scientific endeavor with potential for usefulness to society which

should be exploited at every opportunity. However, the exploitation should be respectful of the requirements of the conduct of science. Primary among such requirements are, of course, a certain amount of research autonomy.

This normative debate about the importance of academic autonomy leaves unanswered the empirical question: How much influence, direction, or pressure is exerted on academic research by the "outside" society? How much do funding and consultantships outside of the university affect research within it? Here again there are two interpretations. According to the first, the increase in federal and private money for academic research following World War II has caused academe to lose control of its own research. Funding decisions are now made outside of the community, a particularly important development because research costs have risen dramatically as new methods became available making funding more crucial to research than in the past.¹ Because academic scholars are no longer determining their own research objectives, but now allow those to be determined by governmental and private agencies, research agenda-setting increasingly lies within the powers of decision-makers in those funding agencies. Further, as Irving Louis Horowitz says:

. . . big foundations and major universities are often policy extensions of federal agencies--if not directly then through special laws and rules governing the taxation of philanthropic agencies and universities. The source of funds for research tend to be exclusively

concentrated in the upper classes. The fact that the President can indirectly participate in the selection process of major foundations indicates the intimacy that exists between federal and private controllers of wealth despite legal niceties. This fusion of government and corporate wealth makes it difficult to bring about a countervailing pluralistic system of power with respect to social science funding.
(Horowitz, 1972; 378)

However, according to the second interpretation, academic scholars have "colonized" the funding agencies and have succeeded in diverting funds to meet individual career needs within the academic community. Thus, in essence, it is the funding agencies which defer to the research interests and judgements of the academic community. Witness the "peer review system" used by so many funding agencies; a system which passes research control from the hands of the funding agencies into the hands of the academic community.

The task of this chapter is to examine the societal reward structure as it relates to the academic community and attempt to establish the relative validity of each of the above interpretations. A two-step process will be followed. First, the nature of the funding structure will be described. Second, models of its interaction with the research community will be reviewed. Ultimately, our interest remains the same as in past chapters, to assess any influence exerted on academic research trends. As usual, special emphasis will be placed on the case study, political development research in the 1960's.

In describing the funding structure, the history and operations of private and public funding agencies will

be reviewed, including a discussion of relevant Congressional bills, hearings, and scandals. (See Appendix I for a chronology of events.) In order better to describe the funding structure and explain its influence, twenty-one in-depth interviews were conducted with persons involved at many stages of the academic funding process. These interviews will be reported within the body of the following discussion, each interview cited anonymously by the numbers one through twenty-one. Anonymity was assured the interviewees in order to create an atmosphere of trust; therefore, only the role of each interviewee can be reported in the interview citations. (See Appendix II.)

The interviews were conducted between May, 1973 and November, 1975, in Chicago, New York, Washington, and St. Louis, and vary in length from one hour to three hours. The average interview is 75 minutes long. The interviews' purpose was to gather descriptive material about funding operations and to generate hypotheses about the nature of power and decision-making within the system. Two principal goals were pursued: 1) to establish what criteria are utilized in the distribution of rewards (funding) and 2) to determine who the decision-makers in that distribution process are. Both issues ultimately address the problem of academic autonomy.

The Social Sciences and Federal Funding

Social science research is conducted in several distinct environments; the private research organization, colleges and universities, governmental agencies, industrial and commercial organizations, and other locations.² In 1959, Harry Alpert summarized the distribution of research among these various sectors:

. . . (1) more than three-fifths of the total national activities in social science research is supported by and conducted by industry and business; (2) the federal government provides about one-fourth of the funds available for social science research but conducts a little more than one-sixth of the total through its own facilities; (3) colleges and universities contribute just over 2 percent of the separately budgeted funds for social research but actually expend a sixth of the total on research that they themselves perform; (4) institutions of higher education are dependent upon the federal government and private foundations, and to a lesser extent upon industry, for support of the research they conduct.

(Alpert, 1959:74-75)

The rather small amount (25%) of social science research supported by the federal government in 1959 was actually a very large expenditure when compared with past federal support for the social sciences. A brief review of the development of federal funding of social science will place the Alpert analysis in context.

Pre-World War II political science, while principally concerned with moral and political philosophy, was passing into a period of increasing methodological sophistication, until it emerged today as empirically grounded observational and experimental science. One important

consequence of this developmental process has been the increase in the cost of social science research, which costs are largely related to expense associated with the systematic collection of data. Henry Riecken describes the nature of the rising costs:

. . . the costs of research in the social sciences consist principally of payment for the skilled (and often professional) labor of interviewers, observers, experimenters, coders, data analysts, and those who construct questionnaires and develop tests of various kinds. In recent years outlays for high-speed electronic computers and the professional and technical labor associated with their use have increased rapidly --a trend that seems likely to continue to grow as it becomes possible (and desirable) to work with larger and larger bodies of data. The travel and other expenses associated with cross-cultural or multi-national research are another factor in rising costs.
(Riecken, 1968:486-87)

The result of the increasing expense of social science research has been an increase in the financial support of this research above and beyond the personal resources of the scholar involved. As has been the case in the physical sciences, the dramatic rise in the expense of equipment, laboratories, and personnel necessary to conduct social science research in recent years has created a dependency within the social sciences on funding from outside of the academic community. Private foundations were first to come to the fore in funding social science research, during the period prior to World War II.³ During the War, however, the social sciences proved themselves a useful and productive source of information and policy-recommendations. Specifically, advances in

the field of social psychology occurred in research on troop morale, psychological warfare, and intelligence analysis. Anthropologists contributed concepts about the relations between culture and personality. (Lyons, 1969:8)

With these and other examples, the social sciences demonstrated their practical value to the nation. Prominent among those contributing to social research during World War II were officials of the Office of Strategic Services (OSS). During testimony before a 1945 Congressional committee examining the contribution of the social sciences in World War II, political science, economics, statistics, history, social psychology, sociology, anthropology, and geography were all identified as basic social sciences which contributed significantly to the intelligence activities of OSS.⁴ Thus, World War II was a major proving ground for the policy usefulness of the social sciences and proved an important catalyst in their development.

The interaction of increased need and demand for social science research on the part of the federal government and the development of social science itself (the growing emphasis on the collection of empirical evidence and growing sophistication in the analysis of that data) mutually reinforced the post World War II evolution of the role of social science in the federal government.⁵

The evolution was stimulated by two developments: 1) the creation of the National Science Foundation, and 2) organizational innovations within the federal government in

response to its need for social science research. Federal agencies after World War II began to support extramural research in the social sciences through grants to colleges and universities and research institutes associated with them, as well as increasing their pursuit of social research conducted intramurally and through specially selected or specially created research agencies.⁶ G. M. Lyons has reconstructed post-war governmental innovations in the support of research in the areas of defense and national security, as follows:

. . . organizational innovations in the area of defense and national security took several forms: the establishment of government laboratories, administered by federal agencies or under contract to private companies; the extension of the contract method in order to finance research, development, and analysis by industrial or university groups; and the creation of non-profit government-financed corporations like the RAND Corporation and the Institute of Defense Analysis (IDA). Such innovations in administrative technique have been extended to deal in a systematic and analytical way with problems of military strategy, international stability, and foreign economic and technical assistance, as well as with problems of weapons technology. Organizations like RAND Corporation and IDA have also been created by government agencies to deal with problems of urban reconstruction and social progress, including the Urban Institute and the Institute for Poverty Research. These semi-autonomous institutions, as well as the university and private research teams that operate under contract, have then served to promote the growth of expert staffs within the federal agencies that make use of their work.

(Lyons, 1969:13)

Increasingly, the social science research needs of the federal government have been met outside the federal agencies themselves, through developments such as those described above in the case of defense and national security

needs. Extramural social science research supported by the government is, to a large extent, applied research and thus tends to be tied to the specific missions of the federal agency involved. Only one federal agency, the National Science Foundation, is devoted exclusively to the support of basic research. The support of social science research by NSF was a major step forward for the social sciences, and was gained only after lengthy debate and stiff opposition.⁷

The National Science Foundation

Immediately after the conclusion of World War II, policy decisions had to be reached concerning the declassification of scientific findings amassed during the war and the future structure of government-science relations. In 1945, Vannevar Bush published a study addressing these problems. Science: The Endless Frontier was a synthesis and summary of the studies of four committees of scientists, each having taken responsibility for a post-war problem area and each charged with proposing recommendations in that area. One of the recommendations of the report was the establishment of a "National Research Foundation" to "assist and encourage research in the public interest." (Bush, 1945)

Bush limited his recommendation to the natural sciences, including biology and medicine, but soon the exclusion of the social sciences from his recommendations

became a hotly debated issue. President Truman spoke in favor of the inclusion of the social sciences in the scheme for future federal support of science, and Senator Thomas C. Hart, Republican from Connecticut, proposed an amendment to a bill sponsored by Democratic Senator Harley Kilgore of West Virginia. Hart's amendment proposed the inclusion of the social sciences in support specified under the proposed National Science Foundation. In 1946, the Hart amendment was defeated in the Senate by a vote of 46 to 26, with 24 members not voting. The debate over the role of the social sciences within the proposed National Science Foundation dragged on through the following four years, until in 1950 a National Science Foundation was finally established, with the provision that support for the social sciences was "permissive, but not mandatory." During the period between 1945 and 1950, several attempts were made to evaluate the exact contribution of the social sciences to the war effort, and their overall effectiveness in assisting with policy-making. The "usefulness" of social sciences became a key issue in all subsequent debates concerning federal support for social science and is the focus for the "problems" arising from the intensification of cooperation between the government and the social science community.⁸

One problem perceived by social scientists and others supportive of an autonomous social science was that the growing intimacy between social scientists and the federal

government may lead to the subjugation of social science research to government control. Fears that the government might exert an undue influence on the direction and nature of social science research underlay much of the opposition to its inclusion in the support offered by the National Science Foundation. In addition, the fear persisted that, in dealing with disputatious issues, it may be difficult to hold clearly in sight the line between fact and opinion. Of course, these concerns are somewhat alleviated by the basic science nature of NSF support: they are more acute and have further ramifications in the case of federally supported applied research.

Concerns specific to applied research relate directly to the mission-oriented nature of federal agencies and the conflict between the "fundamental research" interests of the social researcher and the need which the supporting agency has for "practical" results. Related to this concern is the fear that the federal sponsor may attempt to affect the outcome of the research effort, or to suppress research results which contradict its established policies. In addition, there is the understandable concern on the part of federal agencies with immediate, contemporaneous social problems, which may create a surfeit of research support in a given area for a short time, followed by a drying up of funds as interest shifts to another pressing social problem. As a result, research may be

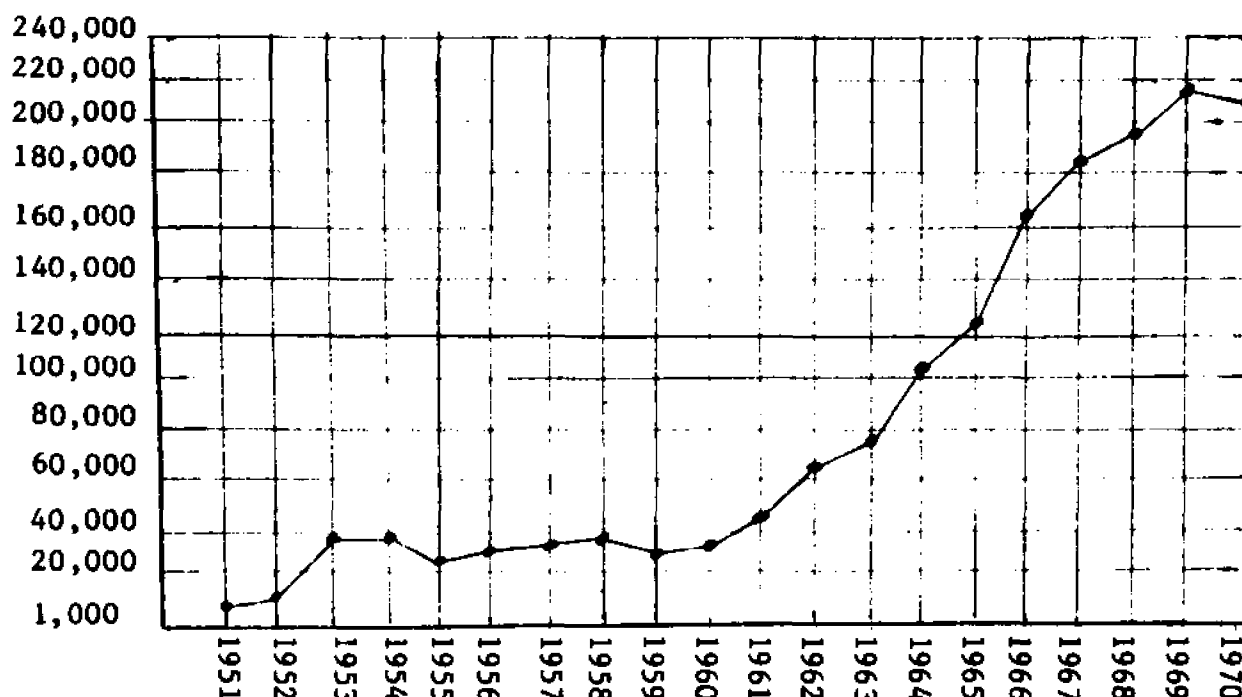
directed by the grantor's interests at the expense of those research topics valued highly by the social science community but ranking very low in the federal government's research priorities.

Post-War Federal Support of Social Science

During the 1950's and 1960's, government support of social science research continued to grow--amidst the debate discussed above. The following graph, prepared from National Science Foundation statistics, indicates the steady growth in federally supported social science research. In 1946 the Atomic Energy Commission and the Office of Naval Research had been created. In 1952 the Army created the post of Chief of Research and Development in the Office of the Chief of Staff, and by 1960 all the armed services had established positions of responsibility for research and development at the Assistant Secretary level. (Ellis, 1964) The new Department of the Air Force organized units to undertake and support research in problems of selection and training, group morale, organizational structure, and related social psychological and sociological areas. When the Research and Development Board was established in the Department of Defense, it included a Committee on Human Resources. (Alpert, 1959:81)

While the post-war period has been one of steady growth for federally supported social science research, the development has not been without its "starts and fits."

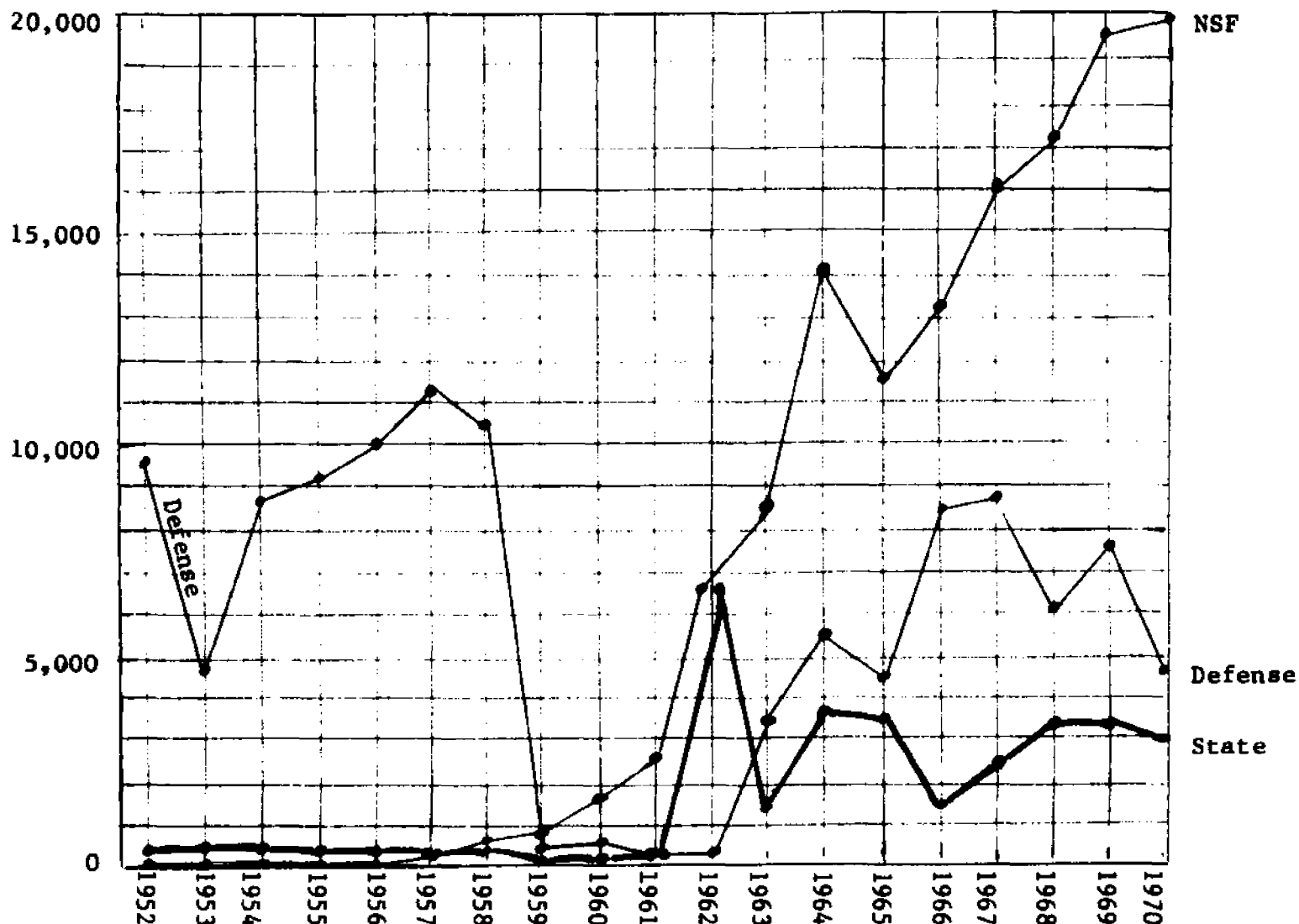
Graph 5.1: Federal Funds for Research in the Social Sciences, Fiscal Years 1950-51 Through 1970 (In thousands of dollars)



Source: National Science Foundation, Federal Funds for Research Development and Other Scientific Activities, NSF 69-31. Washington, D. C.: Government Printing Office.

While the number of federal agencies involved in social science research and the level of expenditure (both intramurally and extramurally) has shown a steady increase, programs often falter and suffer reversals, many times as a result of Congressional objections. The erratic nature of social science expenditures can be seen in the following graph showing comparative social science research support by three federal agencies, the Department of Defense, the State Department, and the National Science Foundation, for the decades of the 1950's and the 1960's.

Graph 5.2: Federal Funds for Social Science Research, by Agency: Comparison of the Department of Defense, the Department of State, and the National Science Foundation, for Fiscal Years 1951-52 through 1970¹ (In thousands of dollars)

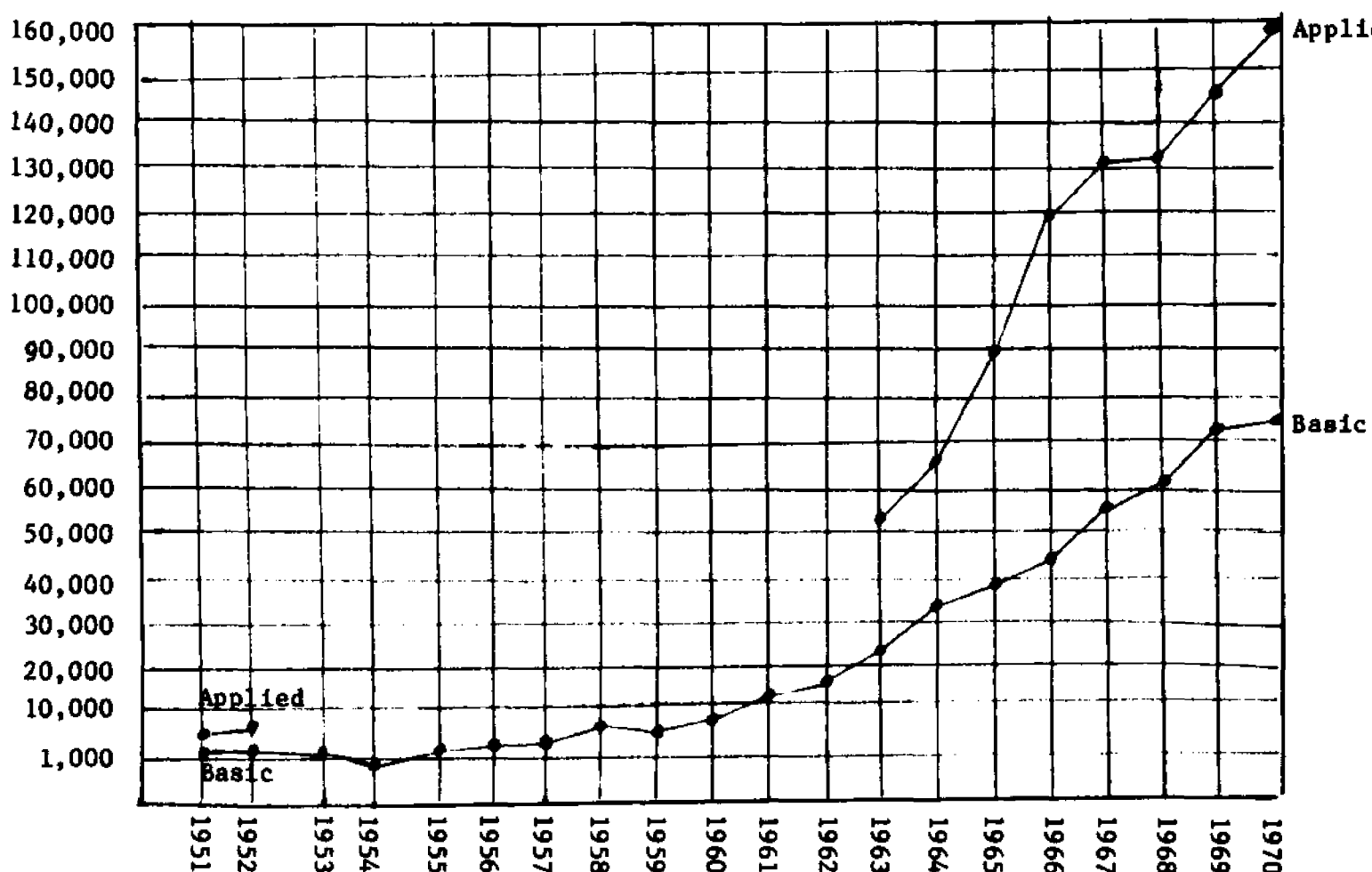


Source: National Science Foundation, Federal Funds for Research, Development and Other Scientific Activities, NSF 69-31. Washington, D. C.: Government Printing Office

¹Department of Defense figures for fiscal years 1956 through 1962 exclude pay and allowances of military personnel in research and development.

Further, federal support for social science research has been greater for applied research than for basic research. The following graph indicates the relative levels of support for each type of research for the decades of the 1950's and the 1960's.

Graph 5.3: Federal Funds for Basic vs. Applied Research in the Social Sciences, Fiscal Years 1950-51 through 1970 (In thousands of dollars)



Applied research: missing data, 1953-1962

Source: National Science Foundation, Federal Funds for Research, Development and Other Scientific Activities, NSF 69-31. Washington, D. C.: Government Printing Office.

The mission-oriented nature of federal agencies, with the exception of the National Science Foundation, makes the greater support of applied research predictable: it is noted here simply as a reminder of the greater "threat" which applied research potentially poses to academic autonomy in the social sciences. As argued earlier, mission-related research may be more susceptible to control by the agency commissioning the research. Because the potential for application of the results in policy-making by the commissioning agency gives the sponsor a high level of interest in the nature and results of the research, it is feared this may lead to unscientific data manipulation or the suppression of research results unfavorable to the policies of the agency involved, raising the question whether federal support necessarily means the loss of scientific independence and autonomy. This question, among others, was studied by a joint committee from the National Academy of Science and the National Research Council. In its report, entitled The Behavioral Sciences and the Federal Government (1968), the committee recommends a pluralistic system of support and methods of political and professional administration within which scientific independence could be protected. It had been argued earlier (Gordon, Parelius, and Marquis, 1967) that such pluralism in research support was necessary because the federal government tends to support projects which have more or less assured payoffs,

thereby neglecting the support of creative and innovative research.

On the other hand, this cautionary attitude toward the dangers of federal support for social science research has been disputed by those who argue that, despite the more imposing administrative structure of the federal government (vis-à-vis private funding agencies), federal funding agencies have shown considerable flexibility in their funding and have supported a higher proportion of innovative studies than have the private funding agencies. Because the federal government supports such a massive amount of research, it is seen as able to take the risk of supporting a number of "questionable" studies in the expectation that the payoff from a few of these studies will compensate for the failures.

The debate over the value of federally funded social science research and the constraints federal emphasis on applied research may exert cannot be settled here. However, in order to assist the reader in reaching a more informed conclusion on the debate, the following section will review the processes and personnel involved in the awarding of federal funds for social science research. In the discussion, special emphasis will be placed on the criteria applied in the awarding of funding and the identity of persons responsible for the selection of reward recipients.

Federal Funding Operations

Federal funding for social science research usually takes two forms: "grants" and "contracts." Contracts are initiated by the government; grants are initiated by independent scholars. The distinction is not an extremely precise one, however, as many contracts are actually initiated by social scientists. Amidst the complexities of indirect communication, the unrecorded phone call, and the "old boy network," it is nearly impossible in many cases to identify the initiator of a research project. The practice of second-guessing what federal agencies want researched often brings out the latent "entrepreneurial spirit" of academic researchers.

The distinction between grants and contracts remains helpful insofar as each type of funding carries with it a certain form of review and selection scheme. The schemes vary not only according to the grant/contract nature of the research, but also according to the federal agency involved, and in certain cases the type of research involved. Generally speaking, contracts are 1) awarded at the discretion of the agency involved, 2) usually commission applied research related to the agency's perceived information needs, and 3) are often awarded to non-academic research institutions. Grants are more often reviewed by non-government mediators, under what is known as the "peer review system." This system allows academic scholars to

play a role in the selection of projects for funding by acting as consultants to the federal personnel responsible for awarding grants.

The two greatest sources of controversy concerning the various methods of awarding federal grants and contracts are 1) the "closed" nature of the peer review system, and 2) the covert nature of some federal funding. The two issues will be discussed sequentially.

In hearings beginning July 22, 1975, the House Science and Technology Committee's subcommittee on science, research, and technology, chaired by Representative James W. Symington (D-Mo.) examined the peer review process used by the National Science Foundation in its awarding of grants for basic research. The role of the "old boy system" was carefully scrutinized, and the hearings provided a rare account of the internal working of the federal award system in the case of grants. A comparison of the award system at NSF and the National Institutes of Health (which also support basic research and use the peer review system), will give some insight into the variations possible within the peer review process:

NIH . . . assigns all grant applications to one of some 50 or 60 review panels, called study sections, of 12 to 15 members each. The names of each of the members are public, and a majority of applicants not only know to which study section their grant was assigned but also who reviewed it . . . In contrast to NIH, NSF handles only a small portion of its grant applications exclusively by review of an official panel. In 44 percent of the cases, individual reviewers are selected by powerful NSF staff personnel, called program

officers, who seek peer review comments by mail. These peers are chosen on an ad hoc basis, and never meet together in person (NIH study sections each meet 3-4 times a year). Their written comments on a given grant proposal go back to the program officer, he never knows what the reviewers said.

The names of individuals on the few permanent review panels in NSF are available to the public . . . The only place they are listed is in the foundation's annual report.

(Cullerton, 1975:535-36)

Criticism of the peer review system (especially the protection of the reviewer's identity by NSF) focused on possible favoritism on the part of the reviewers towards colleagues whom they know personally. Representative John B. Conlan (R-Arizona) cast his objections to the anonymity of NSF peer reviewers in terms of the opportunity the system affords for in-group monopolization of federal funds. In his testimony before the Symington Committee, Conlan said:

It is common knowledge that NSF program managers can get whatever answer they want out of the peer review system to justify their decision to reject or fund particular proposals. I know from studying material provided to me by NSF that this is an 'Old Boy's System,' (sic) where program managers rely on trusted friends in the academic community to review their proposals. These friends recommend their friends to reviewers.

(Quoted by Walsh, 1975:435)

Conlan went on to charge that when an NSF program director does not receive "rave reviews" from the academic consultant, reviews are sometimes either discarded or altered to conform with the program manager's predisposition. Such charges led to the much debated Conlan recommendation that the peer review system be required to operate in an

environment of total openness, meaning that verbatim reviews and the names of reviewers should be available on request to the principal investigators who submit grant applications. Opponents of this policy maintain that participation in peer review is broad-based, not confined to an academic elite chosen by "old boy" ties. Those taking this position also often express concern over the disclosure of reviewers' names, fearing that this will water down the quality of reviews, and create an atmosphere of caution and inhibition which would lower the willingness of reviewers to express their opinions with complete honesty.

Officials of NSF deny that the peer group review system provides a rubber stamp from the academic community for funding decisions made by the agency itself. From the point of view of NSF administrative personnel, the peer review system allows academic standards to be the final arbiter in funding decisions. According to NSF, that research is funded which the academic community feels deserves to be funded. (Interview no. 6)

Academic members of NSF social science panels seem to agree with the NSF interpretation of their role in the awarding of NSF grants. In two interviews conducted with current members of NSF social science panels, each respondent felt completely free to apply personal standards of academic excellence in his assessment of proposals sent to him by NSF. (Interviews nos. 1 and 14) There was no perception of guidance or pressure from the NSF program

director: in fact, in one case the respondent complained of a lack of direction from NSF.⁹ However, such evidence (which supports the NSF contention that the peer review system is "open") is not sufficient to vindicate NSF vis-à-vis Representative Conlan's charges. We still do not know what exactly becomes of the academic scholars' recommendations once sent to NSF, nor do we know the degree of favoritism shown to personal acquaintances by academic reviewers.

Charges of cronyism in the awarding of research funds are serious for those agencies and academics involved, but address a specific problem not extremely difficult to remedy. Far more serious and large scale was the exposé surrounding the "Camelot affair." Here, the academic community was faced with charges of unethical conduct, including misrepresentation of the research sponsor to foreign colleagues. The scandal itself, and the soul-searching which followed it, raise questions central to the debate over the proper role of the research sponsor in the academic community.

"Project Camelot"

In 1964, the United States Army's Special Operations Research Office (SORO) received clearance to initiate the largest single grant ever allocated for a social science project, titled "Project Camelot." Funds of over five million dollars were earmarked for the project, whose stated

purpose was:

. . . to determine the feasibility of developing a general social systems model which would make it possible to predict and influence politically significant aspects of social change in the developing countries. Somewhat more specifically its objectives are: first, to devise procedures for assessing the potential for internal war within national societies; second, to identify with increased degrees of confidence those actions which a government might take to relieve conditions which are assessed as giving rise to a potential for internal war and; finally, to assess the feasibility of prescribing the characteristics of a system for obtaining and using the essential information needed for doing the above two things. (Project Camelot communication, dated Dec. 4, 1964, quoted in Horowitz, 1967:4-5)

The potential for the research described above to serve as intelligence information for the counter-insurgency activities conducted by the U. S. Department of Defense is clear. There seems little doubt that intelligence material is what the Army and the Department of Defense hoped to glean from the large investment involved in Camelot.¹⁰ As will be discussed later, however, there was a good deal of misinformation about the exact purpose of the project given out by the Camelot staff. Thus, the academic community was given strong cues (despite the straightforward statement quoted immediately above) that the project was directed toward the advancement of basic research in the social sciences, and that the policy implications involved were minimal.

A substantial proportion of the Camelot funds were to be expended in the collection of primary data in foreign countries, principally in Latin American countries. It

was hoped that foreign and American social scientists could be recruited to the effort, and with that in mind, the staff of Project Camelot began to recruit the necessary academic talent. Such well known scholars as Professors James S. Coleman, Lewis Coser, William Kornhauser, Neil Smelser, S. N. Eisenstadt, Gino Germani, W. J. Goode, Jessie Bernard, Harry Eckstein, Thomas Schelling, Gordon Tullock, and William Riker served on, or were consultants to, the project in its initial stages.¹¹ Major responsibility for the research design was undertaken by Professors Nehnevasja, Coleman, Bernard, and Robert Hefner. The project's Director was Rex Hopper, assisted by Associate Director Ted Gude.

Several tensions plagued the project from its earliest days. As mentioned above, the exact purpose of the project was misrepresented, or more accurately, represented in varying ways according to the audience addressed. For instance, in order to secure military support, Camelot staff members continuously stressed the military bonuses inherent in the project (e.g., intelligence material, policy guidelines, tactical improvements in counter-insurgency programs, etc.) when reporting on the project to the Army or Department of Defense. When addressing academics, these same staff members abandoned military jargon and stressed the potential of the project as a boon for pure research. (Jacobs, 1967) This Janus-headed hypocrisy on the part of staff members was a natural product of their position--

suspended between the interests of two disparate groups, needing to please both, but also aware of their mutual distrust. The academic associates were particularly suspicious of the involvement of the military; cooperation with the armed forces poses ethical problems and can result in damage to academic reputations. To protect themselves, academic scholars involved with the project demanded and received assurances that all research would be publishable at completion of the project. (Interview no. 11)

The project's most problematic tension, however, stemmed from an interagency rivalry between the Departments of State and Defense. While the Department of Defense was officially sponsoring Project Camelot, there was strong feeling within the State Department that, in doing so, Defense was superceding its legitimate parameters, since traditionally foreign area research was handled within the State Department. An additional tension arose from the commitment of each Department to a different style of research: the Department of State to the more traditional research characteristic of the area studies approach; the Department of Defense to a more modern style of research, quantitative in orientation and heavily dependent on computer technology. (Interview no. 11) Because the more "modern" approach dominated Camelot research, State Department resentment of the project grew as research plans progressed.

Why would academic scholars step into this seeming hornet's nest? What could explain a desire to become involved with a project so fraught with tensions and ethical conundrums? The radical critique charges that scholars were attracted to the project for the salary it would net them; also, that ethical considerations were of no concern to those scholars favorably disposed to the Army's counter-insurgency activities, who acted in good conscience to perfect its operations.

In some cases, the radical critique was, undoubtedly, accurate: the overall motivations of the Camelot personnel, however, were far more complex. Irving Louis Horowitz has reconstructed six "points of consensus" shared by many of those associated with Project Camelot. Taken in toto, they demonstrate the naiveté, mistaken judgement, and illusions which characterized much of the thinking on Project Camelot.

First, according to Horowitz, those who worked for Project Camelot felt the need for "a more appropriate big-range social science." (p. 6) Horowitz probably means that many saw the project as an opportunity to conduct fundamental research with relatively unlimited funds at their disposal and hoped the result would be a giant step forward for interdisciplinary social science. Indeed, they may have been encouraged to see the project in that light by staff members who had stressed such possibilities to them in presenting the project.

Second, Horowitz notes that a number of those affiliated with the project were attracted to its non-academic setting, feeling that there was more opportunity to do fundamental research under the sponsorship of the government in a non-academic setting than in an academic setting. Many supported this view by pointing to the fundamental research conducted at the RAND Corporation, a government research agency, during the 1950's. The argument goes: fundamental research was possible within RAND during this period when it was not possible within the university setting--therefore, the government may offer the optimal arena for such research.

Third, while military sponsorship of the project disturbed many of the academic affiliates, they often reacted by agreeing to participate "to educate the Army." Implicit in that notion is a belief that the Army can be a force for social good, if only used in the appropriate way.

Horowitz' fourth and fifth "points of consensus" highlight the optimism and patriotism of many of the academic scholars associated with Project Camelot. According to Horowitz, many scholars were attracted to Camelot research because of their 1) belief in the perfectability of mankind, and 2) opposition to revolutionary holocaust and belief in Pax Americana. For these scholars, the project's goals were acceptable, indeed admirable, and in keeping with their own ideological orientations.

Horowitz' sixth point of consensus addresses the problem of distinguishing intelligence from research.

Horowitz found that:

. . . what became apparent from speaking with Camelot personnel is that none of them viewed their role on the project as spying for the United States government or for anyone else. . . . So far were Camelot people from thinking of their work in cloak-and-dagger terms that none of them interviewed were even convinced that the armed forces would take their preliminary recommendations seriously (even though that remained their hope).

(Horowitz, 1967:8)

Horowitz' analysis of the complex motivations of Camelot personnel seems a more accurate formulation than that of the radical left. Whichever is correct--whether Camelot personnel were innocent, malevolent, or somewhere in between--events conspired to bring the project to a sudden halt. Less than a year after its initiation, the project was cancelled by Presidential order, amidst scandal. A review of the salient events will place the "death" of Project Camelot in perspective.

In the Spring of 1965, with a small but growing staff, a group of academic consultants, and with the bare rudiments of a research scheme formulated, Camelot was "gearing up." The following summer, a research institute was to be held, marking the beginning of the major thrust of the project. Meanwhile it was decided that some "feelers" should be put out in Latin America in order to determine

- 1) possible problems with access to information, and
- 2) Latin American colleagues who might be tapped to serve

on the project. Chile was chosen as the trial country, although the reasons for the choice are unclear in retrospect. To choose Chile may have been a non-decision, sparked primarily by the offer on the part of one Dr. Hugo Nuttini to go to Santiago to determine the feasibility of Camelot research there.

Nuttini, an Assistant Professor of Anthropology at Pittsburgh who was an American citizen, but formerly had been a Chilean citizen, may have initiated the trip himself.

Former members of the Camelot staff recall the association with Nuttini as tenuous and not particularly welcome to the project staff. (Horowitz, 1976:12) When Nuttini arrived in Santiago and began to make contact with Chilean social scientists about Project Camelot, he misrepresented the project's sponsorship. At the time, Dr. Johann Galtung, a Norwegian sociologist who had been invited to join the Camelot staff, was in Santiago. Galtung heard of Nuttini's failure to report the military sponsorship of Camelot to the Chileans he was recruiting, and exposed Nuttini's misrepresentation.¹² Less than a month later the United States invaded the Dominican Republic and Chilean nationalist feelings ran high over the act of "imperialist aggression." It then seemed obvious that the misrepresentation of the role of the Army in Project Camelot was yet another side effect of American interference

in the internal affairs of Latin American countries, and the issue became a cause célèbre in Chile.

The Chilean parliament reacted to the Nuttini scandal by appointing a commission designated to study both Project Camelot and another incident involving an American graduate student but no military money. The appointment of the commission reflected two intense Chilean concerns: the deceit by Nuttini, and the "moral responsibility for results." The latter concern focused on the superior power of the United States and on speculations over its intentions in sponsoring research into the internal affairs of a weaker country. (Landsberger, 1965:85) The activities of the Commission received heavy newspaper coverage in Santiago and the scandal grew in proportion until it became a source of embarrassment to the U. S. State Department, and the U. S. Ambassador to Chile, Ralph A. Dungan. Events then shifted to Washington, where the State Department - Defense Department feud intensified and the White House became involved in assigning blame for the adverse publicity. The result was the cancellation of the project, less than a year after its initiation, amidst Presidential, Congressional, and public outcry.

In the wake of the Camelot fiasco, the social science community and U. S. policy-makers assessed two aspects of the events: the ethical problems revealed by the scandal (the principal concern of the social science community), and the steps necessary to prevent a recurrence (the principal concern of Congress and the Executive

branch). The following review of the "aftermath" of Camelot will discuss each aspect in sequence.

Ethical Implications of Project Camelot

In many respects, Project Camelot was ethically "clean"--certain features of the project were completely acceptable to even the most fastidious academic scholar. Theodore R. Vallance of SORO outlines some of the Camelot "pluses." For instance:

. . . the project was from the start planned as a basic research effort, with no requirements to deliver a product that would have application to anything but further research and development activity. . . . Second, clearly deriving from the open nature of this project, it is evident that no 'espionage,' not even the development of a technology for espionage, was envisioned during or even as an outcome of the project. . . . Third, any research to be conducted in foreign countries would have been conducted only with the full knowledge and consent of the local government and with the fullest possible cooperation of local scholars, universities, and other resources.
(Vallance, 1967:205-206)

Additionally, while Camelot must bear the responsibility for the scandal it caused and its ethical shortcomings must be acknowledged if they are to be prevented in the future, the project was to some extent the victim of American social science research practices which preceded it. Kalman H. Silvert says:

. . . the ground for today's disgrace was well prepared by the ethical incomprehensions, cavalier attitudes, and tolerance of ignorance manifested by American universities and scholars for many years. The sum of these saddening shortcomings bore heavily on Latin America as it became a lucrative and thus intellectually attractive field after the Cold War came to the Caribbean. . . . (Silvert, 1967:81)

It is only fair to place a part of the blame for the Camelot scandal on academic and governmental research on Latin America conducted before Camelot was ever conceived.

The explosion over Camelot was not, however, entirely the result of past research practices: it was also caused by the ethical ambiguities of the project itself. Although Theodore Vallance of SORO can point to several aspects of the Camelot project which do strictly conform to ethical standards, as he did in the quote excerpted above, many others are equally quick to point to the project's ethical shortcomings. The two most serious and frequently mentioned ethical problems with Camelot itself are 1) the misrepresentation of the project's sponsorship by Nuttini, and 2) the imperialistic and militaristic potential of the research itself. The first point is straightforward and self-explanatory: there is almost universal agreement that deceit concerning research sponsorship constitutes unethical academic conduct. But on the second point, the imperialistic nature of the project, there is a great deal of debate. Here the ethical problems become very subtle, for if a scholar believes that research of the nature planned in Camelot is detrimental to the country being studied, he or she would necessarily have to withdraw from the research. On the other hand, if the scholar could see no potential harm to the host country in the research but only benefit to his own country,

would it be necessary to withdraw from participation in the research? A third possible dilemma exists. If the research is to be conducted in any case, does the responsible scholar have an obligation to accept participation (with an enlightened eye and a careful ethical awareness) if the alternative is that the research will be done badly by a person with less vision?

Whatever the scholar's answer in each case of research, there are certain dangers which lurk in government sponsored research of all types. First, government-commissioned research is more likely to be designed with certain boundaries of government-supporting results in mind. A second danger is the possible loss of academic freedom should the government sponsor apply any political pressure, as such pressure might affect both the content of the research involved and its very subject and direction. A related danger is the government's insistence on secrecy in some of its research. Security clearances, signing the Official Secrets Act declaration, and locked libraries are part of government sponsored research in some cases. Along with this type of security, an additional provision is sometimes made: the research results cannot be published.¹³

Freedom to publish is a long-standing cause within the academic community. It is often recommended as a yardstick to be used in making decisions about the ethical value of a research project. The famous question goes:

Can I publish the results so that they will be available to all? In fact, one of the recommendations of a study conducted by the American Anthropological Association designed to examine some of the problems posed by the Camelot scandal was:

Classification of research and research findings should be eliminated except where national security is clearly involved. Precautions should be taken to prevent invocation of national security merely to protect the agency from possible criticism.

(Beals, 1969:157)

The report goes on to recommend,

The highest standards of protection of privacy and concern for the welfare of individuals and groups studied should be employed in the conduct and reporting of research. This involves the basic professional standards of honesty and integrity, including full disclosure of sponsorship, the purposes of research, and the consent of the individuals or group studied. This is doubly important in research outside the United States where violations by government-employed or sponsored researchers may result in closing the country or area to all social science research.

(Beals, 1969:157)

Unfortunately, these two recommendations are a bit like favoring motherhood and apple pie. They bespeak noble intentions with which no one would disagree, yet offer little in the way of concrete proposals to correct past indiscretions. Several more specific recommendations came out of the Camelot scandal, and in a few cases these recommendations were carried through, resulting in policy changes. The following section will review these outcomes of the scandal.

Repercussions of Project Camelot

Following the uproar in Chile caused by the unmasking of Nuttini's misrepresentation and the cancellation of the project, American scholars began to feel the effects of the scandal in their own cross-national research. Relations between American scholars and their foreign colleagues became strained to the point where access to data in foreign countries became noticeably more difficult. The pattern was typically low-key: American scholars were simply distrusted and were discouraged from pursuing research in foreign countries, especially in the social sciences. In at least one case, however, formal ties between a foreign group and an American university were severed--as a direct result of the Camelot scandal. In that case, the Brazilian group which had been involved in a joint project with Cornell University gave as its reasons for withdrawal from the project both the objections of Brazilian students to "American interference" and the Camelot affair. The Brazilian group explained:

These two facts create a picture in which minute data or nuance lose meaning and from which it is impossible to escape. How can one maintain and justify a relationship with an institution--the university in the United States--which permits itself to be transformed into the instrument of a security agency which today is internationally known as the instigator of dictatorial coups?

(Langer, 1967:1584)

Foreign suspicions and hostilities were further heightened by several events following the Camelot explosion.

The increasing American involvement in the Vietnam war, for one, caused intense apprehension among many less developed countries that they might be the next site of American protection. The American CIA, throughout the 1960's, was increasingly perceived in the Third World as a threat to national self-determination and progressive social change. Its image grew more odious rather than bouncing back after the Camelot scandal. The publication of several articles exposing the work of the CIA within American universities in Ramparts magazine in 1967 received considerable attention in Third World countries. The Ramparts evidence supports well-established foreign opinions of the CIA's work.

It was common knowledge that Camelot was not an isolated incident of government-sponsored research, conducted within the academic community, which was of questionable benefit to the foreign countries studied. In 1969, for instance, M.I.T.'s Center for International Studies carried out numerous sponsored studies dealing with Communist China, the U.S.S.R., North Vietnam, guerilla warfare, and other topics. Some Ph.D. students at M.I.T. worked on classified material and could only be supervised and examined by professors with the necessary "clearance." (Oppenheim, 1969:330)

The distrust of foreign academic colleagues was further aroused by revelations that Camelot research was resurrected, in varied form (a short time after the project's

cancellation), in Colombia and Peru: nationalist sentiment again became perturbed in these countries as it had in Chile. According to the New York Times of February 7, 1966, the projects, termed "Operation Simpatico" in Colombia and "Operation Task" in Peru were again sponsored by SORO of American University, received the financial support of the Defense Department, and involved American university professors. (Johnson, 1966:1016) Yet another source of distrust on the part of foreign colleagues was a growing suspicion that research of the Camelot type was being conducted in Africa, more discretely than was the original project's research. In two interviews conducted with American scholars of African studies, both respondents volunteered their own observations of counter-insurgency research, funded by the American government, conducted by academic scholars in Africa. (Interviews nos. 12 and 13) In a March, 1967 letter to Transaction magazine, Pierre L. van den Berghe (himself a well-known American scholar of Africa) reported pressure and harassment from a government agency for his refusal to participate in such research. (van den Berghe, 1967:63-64)

Of primary concern to the government, however, was not the difficulties created by Camelot for American academic researchers conducting research abroad, but the embarrassment the scandal caused to the American government itself. After the Chilean uproar, the Congress became

very concerned with "closing the barn door" and creating safeguards against a repetition of the international humiliation. In two important sets of Congressional hearings, the issues raised by Project Camelot were aired and examined with an eye to the creation of future safeguards.

Congressional Hearings and Resultant Preventive Measures

As Project Camelot was being approved and staffed, a Subcommittee of the House Committee on Foreign Affairs was conducting a continuing investigation of "ideological operations in foreign policy." The studies produced by this investigation are concerned with the role played by the social and behavioral sciences in U. S. foreign policy. As a result of the first three studies produced by the Subcommittee, certain innovations were made in research review processes within government agencies. In 1964, for instance, the Department of State took steps to strengthen its External Research Staff. In that same year, an inter-departmental Foreign Area Research Coordination Group (FAR) was established to improve communication on the working level among the departments and agencies involved in research relating to foreign affairs. Later in the same year, the U. S. Arms Control and Disarmament Agency established a Social Sciences Advisory Board to provide counsel to the Agency on matters relating to social science research.

When, in 1965, Project Camelot was cancelled and public attention was directed to U. S. research on foreign

areas, the same Subcommittee turned its investigation to the events of the Camelot affair. Hearings were held and a report, proposing several recommendations for the prevention of a future Camelot, was issued. The report, entitled Behavioral Sciences and the National Security was published together with the hearings which led to its conclusions, Part IX of the Hearings on Winning the Cold War: the U. S. Ideological Offensive. The stated purpose of these hearings was:

. . . to explore the extent of governmental involvement in research relating to foreign policy operations, to assess the effectiveness of the arrangements for the coordination of such activities, and to determine what improvements may be necessary to strengthen our Government's effort in this field.

(Behavioral Sciences and the National Security,
1965:1R)

As a result of its investigation the Subcommittee found that the bulk of research concerning foreign policy sponsored by the government was conducted by the "Military Establishment" rather than the State Department. It recommended a constant surveillance of such research by "authorities entrusted with the responsibility for the conduct of U. S. foreign policy." The Subcommittee's most concrete recommendations, however, were threefold.

First, government departments and agencies should develop an effective system for a coordinated determination of research needs and priorities, so that each department and agency is no longer free to go its own way, responding only to its own needs and inclinations.

Second, an Office of the Behavioral Sciences Adviser to the President should be established as a focal point for a sustained and fruitful collaboration with private scholars and the academic community.

Third, the Subcommittee recommended the convocation of a White House Conference on Behavioral Sciences to examine the national effort in these fields, and "to bring behavioral science knowledge to bear on government policy."

Concurrent with the Subcommittee's hearings was the establishment of the Foreign Affairs Research Council, designed to monitor all government sponsored research concerned with foreign affairs. The Council was initiated as a result of a letter from President Johnson to Secretary of State Dean Rusk, dated August 2, 1965, instructing the Secretary of State to establish procedures to insure that the U. S. Government not undertake any foreign area research which, in the Secretary's judgement, would adversely affect U. S. Foreign relations.¹³ The Council, as established, is chaired by the Director of the Bureau of Intelligence and Research, and includes a member of the Deputy Under Secretary's Office (Politico-Military Affairs), and, as necessary, representatives of the State Department's geographic bureaus. The functions of the Council include a review of all government-sponsored research relating to foreign affairs, the exercise of veto power over individual projects, and the formulation of Department of State

policy with respect to the criteria, the priorities, and the implementation of this type of research. (Behavioral Sciences and the National Security, 1965:4R)

One other set of hearings, this time in the Senate, is related to the Camelot affair. At the time of the project's cancellation, some thought a remedy for such a scandal might lie in the creation of a National Social Science Foundation, reasoning that what was needed was a federal agency to support social science research which was completely free from fear of Congressional reaction to conclusions that research might reach. Two years after Project Camelot's cancellation, Senator Fred R. Harris (D-Oklahoma), Chairman of the Subcommittee on Government Research, Committee on Government Operations, conducted hearings on the feasibility of establishing a National Social Science Foundation. In those hearings, the mistakes of Project Camelot were analyzed, advice was sought from the academic community, and a case was made for the need for the new agency. In the end, however, several bills before both the House and the Senate failed to be passed: the proposed National Social Science Foundation was never established, and the social sciences continue to look to NSF for support of basic research.

* * *

Scandals such as Project Camelot evoke a predictable reaction in Congress and the White House. Federal support

of research becomes more cautious, more rigid, more bureaucratized, and perhaps slightly reduced--all to prevent a reoccurrence of the scandal. That seems to be the case in the aftermath of Project Camelot and other lesser scandals which followed it. During the 1960's, federal support of the social sciences rose rapidly; thus, Camelot did not lead to a reduction of support. It did, however, lead to an increase in red tape and bureaucratic oversight and restrictions, as well as a decrease in the personal relationship which often existed between grantee and federal bureaucrat. (Interview no. 13)

To escape the red tape and ethical dilemmas posed by federally-supported research, some scholars turn to the private foundations for research sponsorship. We will now turn our attention to the "private sector" and examine the foundations' history and operations.

The Private Foundations: Historical Development

Private charitable foundations grew in number in the United States at the beginning of the 20th Century, largely as a result of the accumulation of wealth in private hands which resulted from industrialism under a capitalist system. The accumulation of wealth was both a blessing and a curse for the successful industrialist: a blessing in that wealth served as an indicator of success and self-worth, but a curse in that money alone was not considered the key to moral worth but must be accompanied

by good deeds. According to the historically influential Protestant Ethic, wealth for its own sake was not the formula for rewards in the afterlife, and in fact if accumulated through greed or dishonesty, may disqualify a man from being considered virtuous.

In establishing a charitable foundation, the wealthy industrialist could combine personal merit and good business practice. By dispersing his wealth in a fashion designed to benefit the society as a whole, he need no longer fear for lack of "good deeds" in his lifetime. At the same time, because the money was assigned to charitable purposes, the federal government was willing to make tax concessions which made the dispersion of his money nearly painless to the donor. The formula was ideal for earlier industrialists, and apparently has proven so for wealthy donors ever since, as the number of foundations in the United States has grown steadily since the turn of the century until now there are over 26,000 private foundations in existence.

The federal government has smiled on this proliferation of foundations by granting foundations a tax-exempt status, and by disallowing gifts to foundations from the taxable income of the donor. Thus foundations hold a unique place in the private sector, in that they are relatively free of government regulation and control, yet benefit from governmental assistance in the form of tax breaks.

In return, foundations are expected to remain true to the ideal of service for the public welfare, and are intended to remain voluntarily accountable to the federal government which has blessed their existence.¹⁵

In fact, this harmonious relationship has often broken down, as periodically Congress becomes alarmed over some aspect of foundation behavior and threatens to curtail or eliminate their favorable treatment. In 1915, during the Progressive Era, Congress held the Walsh Committee Hearings into the activities of foundations. At that time, Congressional concern was aroused over the ultra-conservatism of foundations, fearing that they were gaining too much power and merely served as an arm of monopoly capitalists.

In 1940, Congressional concern came from a different ideological direction: this time an unsuccessful Congressional amendment took aim at foundation liberalism, and the perceived interference of foundations in national politics. In 1952, during the McCarthy period when communism was seen as the subverting influence behind all "un-American activities," Congress established the Select Committee to investigate Tax-Exempt Foundations and Comparable Organizations, chaired by Representative Eugene Cox of Georgia. Cox and his committee looked for pro-communist activities and infiltration within the foundations, but the Committee hearings ended in acquittal. The foundations, it seems,

had steered clear of any associations which the Committee could label "communist."

In spite of the acquittal, one member of the Cox Committee, Representative Brazilla Carroll Reece of Tennessee, persisted in suspecting the foundations of pro-communist, subversive, and pro-socialist activity. Congressman Reece became particularly obsessed with the activities of the Ford Foundation, and in 1954 he opened hearings on foundation activities. Once again, no legislation resulted. However, the Committee's Report (House Report No. 2681) remains a classic statement on the public responsibility of foundations. An excerpt from the Report states the "public responsibility" argument succinctly:

For these great foundations are public trusts, employing the public's money--become so through tax exemption and dedication to public purposes. Foundations are permitted to exist by the grace of the public, exempted from the taxation to which private funds are subjected, and are entitled to their privileges only because they are, and must be, dedicated to the public welfare. The public has the right to expect of those who operate the foundations the highest degree of fiduciary responsibility. The fiduciary duty is not merely to administer the funds carefully from a financial standpoint. It includes the obligation to see that the public dedication is properly applied. . . .

(House Report No. 2681, 1954:20)

Though the foundations had been somewhat shaken by the attack by the Reece Committee, they had emerged relatively unscathed from that encounter, with their tax-exemptions and freedom of activity fully intact.¹⁶ A formidable foe waited in the wings, however. Congressman Wright Patman of Texas, in 1962, launched a one-man

investigation into foundation activities, which proved the most thorough Congressional investigation, and the most momentous, to date.

Congressman Patman was not interested in the political bias of foundations; as was true of the 1915 Walsh Commission, his concern with the foundations addressed their economic implications. The Walsh Commission investigation had been motivated by a fear of big business and a resentment of the activities of the robber-barons. Patman's variety of Populism wasn't far afield of the motivations of that first Congressional investigation, as Patman opposed a concentration of economic power (including big banks as well as foundations) and accused the multi-million dollar foundations of having replaced monopoly trusts broken up during Theodore Roosevelt's administration.

As a result of Patman's investigation, which lasted for eight years and included hearings before a subcommittee of Patman's Committee on Small Business, the Treasury Department became more resolute in its policing of the foundations. Until the 1960's, the foundations had been so loosely supervised by the Internal Revenue Service of the Treasury Department that an accurate and complete list of existing foundations could not be obtained from the Treasury Department when Patman began his investigation. Spurred in part by Patman's activities, the Treasury Department in 1965 proposed reforms which would correct

some of the foundation abuses uncovered thus far by Patman, but the foundations fought the reforms, which in any case were doomed by the opposition of the Secretary of the Treasury himself, C. Douglas Dillon.

The foundations may have wished they had supported the Treasury Department reforms, for in 1969 they were more severely curtailed by the 1969 Tax Reform Act, a product of both the Patman investigation and hearings held by Congressman Wilbur Mills, Chairman of the House Ways and Means Committee. Mills was the author of the 1969 Tax Reform Act, and the person responsible for finally bringing legislation to bear on Congressional concern over foundation activities, but his investigation was less objective, less scholarly, and more political in motivation than Patman's investigation had been. Mills' 1969 Tax Reform Act was in response to public agitation for a more fair tax structure: it has been suggested that he was looking for a scapegoat, to avoid taking on the more powerful political fiefdoms, such as the church or the oil lobby. (Whitaker, 1974:113)

Whatever the motivation of that portion of the 1969 Tax Reform Act which relates to foundation activity, the foundations can undoubtedly live comfortably with the provisions of that Act, for it limits their activities only slightly. The 1969 Act imposes a ban on attempts by foundations to influence legislation, prohibits expenditures

for both political lobbying and electioneering, as well as grants to public servants. Foundations are permitted to communicate with any member or employee of a legislative body only for "educational" purposes, such as providing technical advice or information based on objective surveys or studies. The Act continues to allow foundations to support voter registration drives under certain circumstances, and does not outlaw voter education programs or television presentations that are truly educational. (Reeves, 1970: 23)

In addition to these controls, the private foundations lost some of their tax-exemption privileges with the 1969 Tax Reform Act, though not enough of these benefits to seriously threaten their status.¹⁷ The legislation was enough, however, to startle the formerly placid foundations into paying closer attention to their activities and budgeting. Several of the larger foundations have hired executives whose sole job is to monitor the foundation's conformity to the 1969 Act. (Interview no. 5) It is not at present possible fully to assess the effect of the 1969 Tax Reform Act on foundation activities. Undoubtedly, one side-effect, intended or otherwise, will be an increasing conservatism on the part of foundations vis-à-vis their tendency to support social innovations. Another side effect may be an increase in bureaucratization within the foundations themselves, as accountability and "public responsibility" must increasingly be documented.

What ramifications could result from increased bureaucratization and more detailed accounting procedures within the private foundations? One possibility is that the foundations may lose some of their powers of innovation.

Private foundations have traditionally been more flexible, hence better able to support innovative or challenging programs in areas where the federal government "dare not tread." A popular interpretation of the role of foundations in the greater national security sees it as a proving ground for societal programs too progressive for the federal government to support at the time.¹⁸ It is the greater flexibility and lack of regulation which gives the foundations this ability: to curtail that flexibility may be to curtail the foundations' traditional role.

While not all of the programs supported by foundations (now or in the past) are "progressive" vis-à-vis federal programs, enough have been so to earn the foundations a reputation as "liberal." Irving Louis Horowitz describes the liberal orientation of many American foundations:

In referring to the liberal outlook of major foundations, we are not denying that liberalism is multifaceted and complicated by historical mutations. But here we need only say that foundations are mainly associated with a liberal constituency--with academic intellectuals holding attitudes that have been opposed by political groups showing markedly right or left wing characteristics. For example, the foundations favor United States involvement with foreign nations on the grounds that all parts of the world are interdependent, that wealthy nations like the United States have responsibilities to the rest of the world and

that contact between the United States and other nations provides an opportunity for benevolent exchange. The foundations disdain the nationalistic isolationism of the Right, as well as the left-wing suspicion of American motives and behavior abroad.
(Horowitz, 1972:439)

Of course, even the "liberal interpretation" of foundation activity does not often deny the existence of some foundations which do support or engage in right-wing activities: one common example of right-wing foundations are those supported by Texas millionaire H. L. Hunt. Apparently some private foundations have also served as conduits for CIA funds destined to support such causes as a Cuban counter-revolution or pro-American propaganda on Latin American radio, causes which are not "liberal" by any means. (Sherrill, 1970:133)

Private Foundations and the CIA

It was Congressman Wright Patman who originally uncovered the link between the CIA and certain private foundations it was using as "fronts" in order to funnel money to clandestine projects. Patman's investigation was curtailed, however, when he received "a hint that I had better not touch this because it involves foreign operations of the CIA." At that point, Patman's inquiry became confidential and was completed in executive session.
(Sherrill, 1970:134)

The "trail" was then picked up by Group Research, Inc., a fact-finding organization based in Washington, which

was able to trace CIA funds as they flowed through several foundations, being "laundered" on their way to support of as the American Friends of the Middle East (an anti-Zionist, pro-Arab organization) and the Cuban Freedom Committee. Since that time (1966) the use of private foundations by the CIA, as a channel for funds for foreign and domestic operations, has been researched in detail, until it is now possible for The Washington Post to publish a sophisticated chart of CIA supported projects. (see chart in Whitaker, 1974:158-59)

The complexity of the laundering process must not be underestimated. In the case which caught Patman's attention, CIA funds flowed through seven foundations on their way to their ultimate destination. In some cases, funds are laundered through several additional foreign agencies and foundations before finally reaching the intended source. But above and beyond their laundering function, what is the attraction of the foundations for the CIA? Ben Whitaker (1974) explains that there are three principal "covers" used by CIA agents in their work abroad; posts as journalists, as employees of a foundation, or as employees of a multi-national business. Each role provides the agent with a reason for being in the country and for dispersing funds to local people there. Clandestine work requires alibis and explanations; association with an American foundation often supplies both conveniently.

Most important for foundation autonomy is the question: What effect has the penetration of foundations by the CIA had on foundation activities? One obvious danger is the stifling effect on innovation which could accompany government involvement in foundations. In fact, the public may be convinced of the impotence of foundations to reach conclusions which run counter to government policy as it learns of more and more CIA penetration. Certainly it is the case in Third World countries, where American foundations are now thoroughly distrusted not simply as a result of the Camelot scandal discussed above, but also because of subsequent revelations of complicity between the federal government's intelligence agencies and private American foundations. Scholars in the Third World are now doubtful of the intellectual objectivity of Americans (especially academics) who are involved with private foundations. In the eyes of many Third World scholars, and many Americans, the foundations have been compromised in the area of their greatest strength--their independence from government regulation and their freedom to pursue conclusions in opposition to existing government policy. (Langer, 1967:1583-84)

Foundation Funding Operations

Foundations are established 1) by charter, or
2) by deed of trust; in either case the founder determines

whether the charter or deed of trust will be "general purpose" or "specific purpose." Foundation officials overwhelmingly prefer the "general purpose" charter or deed of trust, as it allows for greater freedom for the foundation to establish its own priorities of action after the donor's death. The example often used to illustrate the plight of the "specific purpose" foundation is the endowment to provide red flannel underwear for divinity students in Boston. When divinity students no longer need flannel underwear, it may be possible to change the foundation's purpose, but to do so usually involves a complicated and costly legal action. (Kiger, 1954:30)

Private foundations are governed by a Board of Trustees and run by a group of salaried officers--the president, vice-president, further officers often called directors, who are in charge of specific branches of the foundation's program, and certain general officers such as the treasurer, secretary, and controller. Supplementing and assisting these officers is a staff of foundation personnel. The officers, directors, and foundation policy making staff are known as the "philanthropoids"--those who administer the foundation's operating programs. (Weaver, 1967:104)

Falling within this general description, the number and range of foundation personnel vary greatly, from extremely small, "specific purpose" foundations to large

"general purpose" foundations, such as the Ford Foundation, the Rockefeller Foundation, the Carnegie Foundation, and the Sloan Foundation.

Whether small or large, each foundation's Board of Trustees is ultimately responsible for foundation policy and activities. Trustees tend to be white, middle age, middle class men, successful in business, education, or a profession, very often from the New York-New England area, and very often politically conservative. They are thus usually men firmly within the Establishment, a "group of absolutely top-flight persons--men of experience, of special talent, of wisdom, and of integrity." (Weaver, 1967: 106) The staff of the foundation, on the other hand, tends to be recruited from academic circles, especially in the case of larger foundations; they are often generalists with broad-ranging abilities and some demonstrated administrative talent.

In recent years there seems to be an effort on the part of the foundations to diversify the make-up of their Boards of Trustees and staffs, partially in response to criticism from the Left, and partially in response to pressure from Congress. For instance, in a confidential study of the Russell Sage Foundation, commissioned by the Foundation and conducted by a group known as the Radical Consultants, Russell Sage was criticized for its overwhelmingly upper-middle class, white, protestant, male staff, and the

consultants recommended, among other things, that the Foundation "desegregate" and "decredentialize" the full- and part-time staff. (Schulman, Brown, and Kahn, 1971:129)

Congressional pressure for affirmative action hiring policies, aimed at correcting sex and racial inequalities within the society as a whole, are being felt within the hiring policies of the foundations, although as late as 1972, Arnold J. Zurcher and Jane Dustin were able to describe foundation hiring as "informal and unstructured." (Zurcher and Dustin, 1972:66)

Although somewhat dated, Joseph C. Kiger's study of private foundations makes a useful distinction in operating procedures regarding foundation fund-dispensing activities. Kiger distinguishes between foundations which operate on "tight lines" and those which operate on "loose lines." He describes each of these procedures as follows:

The former ("tight" operating procedures) insist on formal applications for those seeking grants, usually in writing; in order to be considered, institutions must conform to certain predetermined standards; these foundations are unlikely to make grants out of line with their established programs of activity. The latter ("loose" operating procedures) do not insist on all the foregoing points, and, in general, the grants are smaller.

(Kiger, 1954:37)

Further complicating the "tight"- "loose" distinction is a second distinction in foundation operating procedures, that among "institutional," "operating," and "fellowship" foundations. In this case, the distinction addresses the recipient of foundation monies rather than the method of

application to the foundation. "Institutional" operating procedures allow the foundation to make grants primarily to other institutions or agencies for general or specific purposes. The "operating" foundation pays salaries to permanent or semi-permanent members of its own staff for the performance of specified tasks. Finally, the "fellowship" foundation makes grants primarily to individuals outside the foundation for varied purposes. (Kiger, 1954:38)

To set out the distinctions themselves is merely to categorize foundation operations descriptively--more interesting is the ramifications of each operating procedure. Specifically: What is the outcome of adopting each operating procedure in terms of 1) foundation accountability, 2) foundation direction, 3) foundation efficiency, and 4) foundation fairness?

In terms of accountability, certainly the "institutional" foundation is the most desirable, in that when foundation activities are carried out by an outside institution or agency, that agency must absorb a large part of the responsibility for its activities, leaving the foundation relatively free of culpability regarding specific projects or programs. The "operating" and "fellowship" types of procedures, however, place the burden of execution, evaluation, and implementation of programs squarely on the foundation itself, making the foundation solely responsible for results. Similarly, "tight" operating procedures

are also more desirable in terms of foundation accountability, in that "tight" procedures allow for less individual discretion in the dispersing of money, hence the selection of recipient individuals or institutions is more standardized and more easily justified to critics and to the public.

Foundation control of the direction of activities it supports is clearly somewhat diminished under "institutional" operating procedures, for by allocating the selection and execution of programs to an outside agency, the foundation must, of necessity, surrender some of its decision-making autonomy to that institution. Similarly, the foundation loses some degree of its control over program direction when it opts for "loose" procedures in soliciting applications for funding. Insofar as the applicant has been required to specify his or her plans in detail, that person can be held to those plans, thus be controlled by the foundation providing support funds.¹⁹

Each type of operating procedure is defended as the most efficient by those foundations employing it, making it nearly impossible to determine the rank ordering of the procedures on the criterion of efficiency. For example, foundations using "loose" procedures in combination with an "operating" foundation format claim to be flexible and decisive, insofar as informal solicitation of grantees, combined with self-contained decision-making allow a foundation

enough freedom and latitude to recruit the "best and brightest" and to apply their talents to the most pressing current problems, thereby "striking while the iron is hot." (Interview Nos. 2 and 5) On the other hand, when a foundation uses "tight" operating procedures, in combination with an "institutional" format, its administrators claim that in this fashion the best talent is recruited by open competition, and the program is carried out with maximum efficiency because the institution administering it is specialized and experienced in a particular area. (Interview Nos. 3 and 4) Only rigorous evaluation of the efficiency of foundations employing each procedure could settle this debate.

In terms of fairness, certainly a heavy dependence on "old boy" connections in the recruitment of funding recipients is discriminatory, in that "old boy" connections are not always grounded in talent and ability. Thus, foundations which use "tight" operating procedures--and open competition for funding support--seem to have a greater claim to fairness in their practices than do those which rely heavily on personal recommendations in allocating funds to individuals or institutions.

No clearly superior set of operating procedures emerges when each foundation "style" is examined on the criteria of accountability, direction, efficiency, and fairness: each type of operating procedure has certain advantages and disadvantages and a particular foundation's operating style is a response to its individual needs and

goals, as well as the prejudices of the administrators who make its policy.

Equally debatable is the value of the use of consultants by foundations. Most of the larger, more progressive foundations (such as Ford, Rockefeller, and Carnegie) use the device of employing consultants, or panels of consultants, on a full- or part-time basis. Several different purposes can be served by the use of consultants. First, consultants may serve a public relations function for the foundation, in that their use may amount to a gesture designed to maintain cordial relations with the particular group affected by foundation decisions. An example would be the use of academic consultants in advising fellowship-granting programs. Or, second, the advice of consultants may serve an indispensable service in identifying the best and most promising talent or the best research proposal, if special knowledge is necessary in order for that decision to be made. Consultants are particularly helpful when it is not feasible for the foundation to maintain persons on its staff equipped with such special knowledge. Finally, consultants may be used as an objective, outside source of criticism and evaluation, giving the foundation an opportunity to see itself through dispassionate eyes.

Usually consultants employed by foundations serve more than one of these three possible functions. How

carefully consultants' advice is heeded is a central question in evaluating their worth, but one which, at this point, has not been researched. One would expect that, as with other aspects of foundation operating procedures, the use of consultants is idiosyncratic to each foundation and is determined by the foundation's own needs and goals.

The Private Foundation and Social Science

Private foundations in the United States have been credited with being "primarily responsible for the development of the social sciences in the United States for fifty years" and serving for many years as the "main source of funds for basic research." (Beals, 1969:144) During the McCarthy Era, when social scientists were being attacked as Socialists, the foundations were simply considered guilty by association. A quote from the general counsel to the Reece Committee illustrates the anti-social science mentality of the period:

Social scientists may be said to have come to constitute a fourth major branch of the government. They are the consultants of government, the planners, and the designers of governmental theory and practice. They are free from the checks and balances to which the other three branches of government . . . are subject. They have attained their influence and their position in government mainly through foundation support; and this support, in the past, has been chiefly given to persons, institutions, and ideas of a progressive-liberal, if not Socialist, coloring.

(Wormser, 1958:84-85)

Soon after the end of the McCarthy Era, the federal government's support of the social sciences began to attain

such proportions that it soon displaced the foundations as the major supporter of social science. Thus, today, a shift has occurred: the larger foundations are still active in their support of the social sciences, but are outstripped by the federal government.

Examples of private foundations' support for social science are abundant; for instance, the Ford, Rockefeller and Carnegie Foundations have all, at certain times in their history, supported the Social Science Research Council. In addition, these three foundations have consistently supported action programs in the social sciences, as well as providing the "freest source of funds" and permitting "the greatest independence" to the researcher. (Beals, 1969:145) In addition to the "big three" foundations, smaller foundations have assisted the social sciences consistently over a long span of time. The Russell Sage Foundation has supported research in sociology, the Wenner-Gren Foundation anthropological research, while the Brookings Institution and the Twentieth Century Fund each have supported and advanced economics.

At the present time and in the recent past, the larger foundations have provided most foundation support for social science research; in 1960, for instance, four-fifths of the \$10 million provided by private foundations for basic research in the social sciences came from the twelve largest foundations. (NSF, 1964:10) Strong support

for social science within the "big three" foundations (Ford, Rockefeller, and Carnegie) is indicated by the fact that, in 1966, the Ford Foundation granted \$12 million, or 61 percent of its total for that year, to the social sciences. (NSF, 1969:26)

As foundation support for the social sciences indicates, relations between the larger private foundations and academic social scientists have traditionally been symatric: while social scientists tend to be slightly isolated within the ivory tower of the university and philanthropoids tend to be preoccupied with protecting themselves against Congressional criticism, the two groups need the services each can provide, and have worked out a mutually agreeable "partnership" usually beneficial to each party. The private foundations provide funds for the "Advancement of social science" and the academic social scientists perform research "for the betterment of social science and society as a whole."

* * *

Thus far, we have identified two major actors within the societal reward structure: the federal government and the private foundations.²⁰ Each actor comes in contact with academic social scientists on occasions of direct sponsorship of the academic's research. ("Direct sponsorship" has been defined as money for research or consultantships, and other positions involving remuneration in money

or status.) As mentioned before, the importance of this support rests in the influence, direction, or pressure it may exert on the work of the academic community, thus reducing academic autonomy.

Two questions are suggested by support of the social sciences by actors within the societal reward structure: How do the actors within the societal reward structure relate to each other? and, What influence, direction, or pressure does the societal reward structure exert on academic scholarship? The first question will be discussed below. The second, addressing the linkage between the societal reward structure and academic research, will be discussed in Chapter VI, where findings of a case study of political development research will be reported.

* * *

Competing Models of the Societal Reward Structure

Two principal models of the societal reward structure compete for general acceptance. According to the first, private foundations are the free-wheeling counterpart to government agencies, supporting innovation where government agencies cannot, acting as society's critical gadfly, and serving as a counterbalance to the status quo orientation of the federal government. According to the second interpretation, the foundations are the handmaidens to government agencies (hence, their favorable tax situation), providing legitimization for government programs

whenever the government requests (or gently guides) them to do so.

The first interpretation seems to have gained widest acceptance within the academic community, perhaps because the academic community works closely with the private foundations and likes to think that they enjoy a high level of autonomy. Thus, most academic studies of the private foundations and their relationship with the federal government reflect this interpretation and stress the freedom, flexibility, innovativeness, and activism of private foundations. (See, e.g., Reeves, 1970; Weaver, 1967; Beals, 1969; and Whitaker, 1974) It follows from this interpretation that the foundations would offer a refuge for those scholars who wish to surrender as little academic autonomy as possible, while still accepting research support from an actor outside the academic community.

The second interpretation has been summarized by Irving Louis Horowitz:

- . The model that I believe is most empirically verifiable goes as follows: (1) policies are decided upon by some department of the legislative or executive branch of government; (2) these policies are arrived at through assumption of the needs of some mass or elite constituency externally; and (3) the need for any new Administration or ambitious member of the House or Senate to define their uniqueness in the political heavens leads them to search out what Gaetano Mosca long ago called the political formula, that is, "the New Frontier," "the War on Poverty," "black capitalism," and so on, ad infinitum; (4) once the political course is set, then there is a frantic search for precedent in the past, justification in the present and rationalization in the future; (5) in order to justify decisions made without any reference to the empirical

world, social scientists are called in to do "feasibility studies," "demonstration effects" and "stimulation analyses" that prove beyond a shadow of a doubt the legitimacy of the course of decision making decided upon in some political backroom or congressional cloakroom, or even in some presidential "state of the nation" report.

(Horowitz, 1972:415)

Here foundations and government work in a partnership, collaborating in using social science research to legitimize programs which social scientists had no real role in selecting. The model is disturbing to those who wish to retain autonomy for the academic community, for if the role of the social scientist is to serve as "mandarin" to the federal government (and to the government's servant, the private foundations), surely the points of contact at which the academic scholars are thus exploited are the points of direct support of academic endeavor--precisely the instances of funding support and consultantships we have discussed above.²¹

Further research is needed in order to argue convincingly for either one of the two models of the societal reward structure. Chapter VI will report such research, designed to shed some light on the internal relations of the societal reward structure, but with primary emphasis on measurement of influence and direction exerted by that structure on academic research. As mentioned earlier, academic autonomy is a central value sacrificed by the surrender of research direction to actors outside the academic community. How much of a threat to academic autonomy

is posed by either the federal government or private foundations? How much autonomy do these actors extract from the academic community through their support for academic research? In what specific ways is their influence felt and in what direction is it exerted? Finally, how might the interaction between the societal reward structure and the academic community be altered so as to either 1) decrease the threat to academic autonomy, or 2) increase the accountability of social science research to public needs?

No facile answers to these questions are appropriate. In the complex interaction among the actors within the societal reward structure, and between it and the academic community, even the most careful case study cannot represent a complete analysis. Chapter VI will report a questionnaire study designed to begin the difficult chore of answering some of the questions posed in this chapter.

CHAPTER V

NOTES

¹For a discussion of the growing costs of scientific research and the subsequent need for funding to the scientific community, see: Gerald Gordon, Ann Parelius and Sue Marquis, "Public vs. Private Support of Science," American Behavioral Scientists, 10 (1967), 29-32.

²The older term "social sciences" is still employed by the Department of Defense and the National Science Foundation, as distinct from "psychological science," whereas "behavioral sciences" has been adopted by the National Academy of Sciences to embrace both areas of knowledge. The State Department prefers the term "social and behavioral sciences."

³For example, the Social Science Research Council, supported by the Ford Foundation, was established in the early 1920's as a source of intellectual and financial support for social scientists in universities. See the discussion later in this chapter of the development of private funding of the social sciences.

⁴Summarized by Alpert (1959:81) in reference to, Hearings on Science Legislation (S. 1297 and Related Bills). Hearings Before a Subcommittee of the Committee on Military Affairs, U. S. Senate, 79th Congress, 1st Session (Washington, D. C.: U. S. Government Printing Office, 1945 and 1946).

⁵Documentation of the specific nature of the evolution of cooperation between the social sciences and the federal government can be found in: The Behavioral Sciences and the Federal Government (Report of the Advisory Committee on Government Programs in the Behavioral Sciences. Publication 1680, National Academy of Sciences National Research Council, Washington, 1968), Knowledge Into Action: Improving the Nation's Use of Social Sciences (Report of the Special Commission on the Social Sciences of the National Science Board, National Science Foundation, Washington, D.C. 1969), The Behavioral and Social Sciences: Outlook and Needs (Report of the Behavioral and Social Sciences Survey Committee, National Academy of Sciences and Social Science Research Council, Washington, 1969), and Gene M.

Lyons, The Uneasy Partnership: Social Science and the Federal Government in the Twentieth Century (New York, 1969), sponsored by the Russell Sage Foundation.

⁶Three distinctions used throughout this chapter are those between applied and basic research, intramural and extramural research, and grants and contracts. Applied social science research is the application of what is known about human behavior to a specific problem for the purpose of achieving a desired research goal. The goal often relates to, or is determined by, societal needs. Basic or pure research is conducted solely for the purpose of advancing knowledge, is usually more theoretically oriented, and aims to establish universal laws of human behavior. The agenda of research is determined by the criterion of "potential new knowledge" rather than by societal needs. Intramural research is simply research conducted by the agency needing or desiring the research results: extramural research is conducted outside of the agency needing the results, although that agency may control the research and have possession of the research results. The distinction rests on whether the research is conducted by the same agency which commissions and uses it. For a discussion of the distinction between grants and contracts, see p.178 of this chapter.

⁷For a detailed analysis of the debate over the inclusion of the social sciences in the support offered by the National Science Foundation, see: Gene M. Lyons, An Uneasy Partnership: Social Science and the Federal Government in the Twentieth Century (New York, 1969) sponsored by the Russell Sage Foundation, pp. 126-136; Harry Alpert, "The Social Sciences and the National Science Foundation: 1945-1955," American Sociological Review, 20 (1955), pp. 653-660; and Harry Alpert, "The Social Science Program of the National Science Foundation," American Sociological Review, 22 (1957), pp. 582-585.

⁸For discussion of the relationship between various social science disciplines and federal agencies supporting research (especially the National Science Foundation) see: Philip Sapir, Julius Segal, and Marcus S. Goldstein, "Anthropology and the Research Grant and Fellowship Program of the National Institute of Mental Health," American Anthropologist, 65 (1963), 117-132; Harry Alpert, "Anthropological Research and the National Science Foundation," Bulletin of the American Anthropological Association, 3 (April, 1955), pp. 332-333; and Harry Alpert, "Social Science, Social Psychology, and the National Science Foundation," The American Psychologist, 12 (February, 1957), pp. 95-98.

⁹For a discussion of the standards applied by academic scholars in their role as panelists for federal agencies, see: Ernest M. Allen, "Why Are Research Grant Applications Disapproved?" Science, 132 (Nov. 25, 1960), 1532-34.

¹⁰One instance of the expression of doubt concerning the intelligence aims of the Army is found in David Riesman's letter to the editor of Transaction, dated January, 1966. Riesman speculates:

. . . I wonder if Horowitz is right in assuming that the Army actually had policy ends in view? The support of research, as of other good things, usually springs from mixed motives. Maybe some people in the Army had to pretend even to themselves that there was a policy bonus for the Army in order to legitimate their support for and interest in social science.

David Riesman's letter is reproduced in "Feedback from our Readers," Transaction, vol. 3 (1966), p. 2.

¹¹A complete list of consultants can be found in: U. S. House of Representatives, 89th Congress, Second Session, House Report No. 1224. "Behavioral Sciences and the National Security," Report No. 4, together with Part IX of the Hearings of "Winning the Cold War: The U. S. Ideological Offensive" by the Subcommittee on International Organizations and Movements of the Committee on Foreign Affairs, House of Representatives. Washington, D. C.: Government Printing Office, 1965.

¹²Johann Galtung had been contacted by the staff of Project Camelot in December of 1964 and invited to join the project. His response explained his refusal: he could not accept the role of the Army as the sponsoring agency, nor the Army's pretensions to being an agency of change in the underdeveloped world, nor the "imperialistic features" and "asymmetry" of the project.

¹³For a more complete discussion of the dangers posed by direct government sponsorship of research, see: A. N. Oppenheim, "Knowledge for What? the Camelot Legacy," British Journal of Sociology, xx, no. 3, 1969, 326-336. An interesting general discussion of the ethical dilemmas inherent in Project Camelot is: Gideon Sjoberg, "Project Camelot: Selected Reactions and Personal Reflections," in Gideon Sjoberg (ed.), Ethics, Politics, and Social Research (Cambridge, Massachusetts: Schenkman Publishing Company, Inc.), 1967, 141-161.

¹⁴The text of President Johnson's letter is reproduced on p. 107 of the report Behavioral Sciences and the

National Security, which includes Part IX of the Hearings on Winning the Cold War: The U. S. Ideological Offensive (1965). For a discussion of the review process which resulted from President Johnson's letter, see John Walsh, "Foreign Affairs Research: Review Process Rises on the Ruins of Camelot," American Psychologist, 21, no. 5, 1966, 438-440.

¹⁵For details of the state and federal laws establishing and regulating the activities of private foundations, see Marion R. Fremont-Smith, Foundations and Government (New York: Russell Sage Foundation, 1965).

¹⁶A complete account of Congressional "attacks" on the private foundations from 1915 through the McCarthy Era is found in: John Lankford, Congress and the Foundations in the Twentieth Century (River Falls: Wisconsin State University, 1964).

¹⁷For a detailed account of the 1969 Tax Reform Act as it pertains to foundation activities and finances, see: Merrimon Cunningham, Private Money and Public Service (New York: McGraw-Hill Book Company, 1972), 190-216.

¹⁸The interpretation of foundations which focuses on their innovativeness often explains the capitalists' tolerance of this innovativeness as the "risk capital concept" or the "venture capital concept." For an explanation of the "risk capital concept," see: Richard Colvard, "Risk Capital Philanthropy," in George K. Zollschan and Walter Hirsch (eds.), Explorations in Social Change (Boston: Houghton Mifflin Company, 1964), 728-748.

¹⁹The term "robinhooding" has been coined to describe the broadening of research plans as originally funded to include research the scholar would like to perform, but which would be unlikely to receive funding support. The success of the deception depends on the degree of monitoring conducted by the funding agency. As monitoring usually declines as the scholar's status and prestige increase, the greatest amount of "robinhooding" occurs among senior scholars. (Interview no 16) See: Peter H. Rosse, "Researchers, Scholars and Policy Makers: The Politics of Large Scale Research," in Elisabeth T. Crawford and Albert D. Biderman (eds.), Social Scientists and International Affairs (New York: John Wiley and Sons, 1969), 85-91. See p. 90.

²⁰Three other actors in the societal reward structure will not be discussed here; the university or college, the non-profit research institute and the "for profit" research institute. The university is principally a

"middle man" between the federal government and the private foundations and the academic scholar. As such, its role is primarily passive and will not be examined in the discussion of the societal reward structure. The reader is referred to several works on the subject of the role of universities in the societal reward structure: Warren Weaver, U. S. Philanthropic Foundations (New York: Harper and Row, 1967), chapter 12; Robert S. Morrison, "Foundations and Universities," Daedalus, 93, no. 4 (1964), 1109-1141; Harold Orlans, The Effect of Federal Programs on Higher Education (Washington, D. C.: The Brookings Institution, 1962); Charles V. Kidd, American Universities and Federal Research (Cambridge: Harvard University Press, 1959); Merle Curti and Roderick Nash, Philanthropy in the Shaping of American Higher Education (New Brunswick: Rutgers University Press, 1965); and David Horowitz, The Universities and the Ruling Class: How Wealth Puts Knowledge in its Pocket (San Francisco: Bay Area Radical Education Project, 1969).

The two remaining actors in the societal reward structure, the "for profit" and the non-profit research institute, do not generally make grants to individual scholars for support of their research, nor do these institutions usually employ academics as consultants. Rather they hire academics to work within the institute on a full-time basis, thus removing the scholar from the academic community. For a discussion of the role of these institutions in the societal reward structure, see: Harold Orlans, The Nonprofit Research Institute (New York: McGraw-Hill Book Company, 1972).

²¹For a discussion of the role of social scientists as "mandarins" to the federal government, see: Noam Chomsky, American Power and the New Mandarins (New York: Random House, 1969).

CHAPTER VI

INFLUENCE OF THE FUNDING STRUCTURE

In the previous chapter we reviewed the potential of the societal reward structure as an influence on academic research, but did not discuss the linkages between it and the other factors in the model. The omission was intentional, for those particular linkages have been researched in a questionnaire study sampling scholars prominent in both "conventional" political development research and "neo-Marxist" political development research in the 1960's. The results of that study will be reported in this chapter.¹

The questionnaire study addresses a limited part of the societal reward structure, as did our discussion of that concept in Chapter V. Two points of contact between the societal reward structure and academic research are examined: the funding of research by an agency outside of the academic community, and the recruitment of academics to serve in (or for) outside agencies. The two points of contact have been designated "the funding structure." (See Chapter V.)

The questionnaire study represents data collected systematically in one case, political development research,

and will serve as the basis of speculations concerning the linkages among concepts of the model. The process is obviously inductive: the case study will not serve to test existing theory, but rather to order and guide speculations concerning such a theory. The process of generating theory from data has been called "grounded theory" by Barney G. Glaser and Anselm L. Strauss (1967), who aptly summarize the dilemma of logico-deductive theory-testing vs. the discovery of grounded theory. According to the authors, social science desperately needs the latter, but is currently oriented toward the practice of the former. Thus, the discovery of grounded theory is currently neglected, although it "provides us with relevant predictions, explanations, interpretations, and applications." (Glaser and Strauss, 1967:1)

* * *

Three linkages between the funding structure and other factors in the model will be discussed in this chapter; first, the influence exerted on academic research by the funding structure, second, the influence the funding structure exerts on dominant disciplinary norms, and third, the influence the funding structure exerts on the academic reward structure. In some cases, the influence will flow in both directions. The basis for the theoretical speculations proposed will be the questionnaire study conducted for that purpose; a brief description of that study will acquaint the reader with it.

The Case Study

The case study approach carries with it several advantages and several liabilities. Its obvious advantages lie in the greater detail, texture, and depth offered by a thorough examination of one case: its obvious liability is the inability to generalize from one case. An additional disadvantage of the case study is the difficulty of choosing the "best" case to examine. How can the reader be sure that the case study has not been chosen simply because it promises the researcher the results he or she is anxious to obtain?

In response to the necessity for justifying the choice of case, we are able to point to several aspects of political development research which make it an ideal research trend to serve as the case study. First, the study of political development cuts across the somewhat artificial boundaries of academic disciplines: scholars from political science, sociology, economics, and history have been prominent in the field. By choosing the trend of political development research as the case study, rather than an area of research strictly within one discipline, the generated hypotheses should have slightly better generalizability across social science disciplines.

Second, Glaser and Strauss recommend the use of comparative analysis in the generation of grounded theory,

thus requiring a sample of at least two research trends. (Glaser and Strauss, 1967:21-160) Political development research, however, has the advantage of being one substantive area of inquiry characterized by two approaches, so dramatically dissimilar to each other as to create two separate trends of research within one substantive area. Thus political development research offers an opportunity to practice comparative analysis within the framework of the case study.

In addition, the two approaches to political development are dissimilar on theoretically relevant dimensions. As the discussion of political development research in Chapter I indicates, the two approaches vary on three of the four factors discussed in the theoretical model; ideological orientation (ideology), methodological approach (dominant disciplinary norms), and the status of scholars involved (academic reward structure). Unfortunately, variance on the fourth factor, the societal reward structure, cannot be determined from a reading of the research: it is impossible to tell, in most cases, what role factors outside of the academic community have had on research without additional information above and beyond what is available within the body of the research itself. For instance, in the points of contact between research and the societal reward structure of interest in Chapter V, the funding structure, it is necessary to ask the scholars

involved what consultantships they held, or what research support they received, which may have (when aggregated) affected the rise and decline of the research trend of political development.

It is a matter of general knowledge that funding for social science research was abundant during the decade of the 1960's, and that foreign area research was heavily supported by both government and private agencies. A questionnaire study of political development scholars will allow us to generate hypotheses concerning the influence of the funding structure on research trends. The study will also allow us to formulate hypotheses about the theoretical linkages between the funding structure and other factors in the model. The case study will then have served its purpose: to help generate theory which can then be tested at a later time in a logico-deductive fashion.

The Samples

Two samples were drawn from political development researchers: the first from the body of scholars prominent in the "conventional" approach to political development; the second from scholars of the "neo-Marxist" approach. The goal was not to sample all political development scholars, but only those truly prominent and influential in the field--scholars whose work was visible and respected--thus confining the samples to those who actually created the research trend and its "alternative" approach rather

than sampling all those who may have worked within it at one time or another.

Prominent scholars of the "conventional" approach to political development research were chosen by consulting ten reviews of political development literature (listed in Appendix III). In order to be sampled, a scholar's work must have been cited in seven of the ten reviews consulted.² This sampling technique yielded an n of 31 respondents.

The "neo-Marxist" sample was drawn through a search of "neo-Marxist" political development literature. The sampling technique itself was dependent, to some extent, on personal judgment, as the scholarship of this approach is limited and very often obscurely located. Publications of "alternative" or "leftist" presses, periodicals, and journals (especially The Monthly Review Press, Science and Society, and The Monthly Review) were examined for the years 1960-1969 and relevant scholars sampled. Others were included as a result of a general bibliographic search for scholars employing the "neo-Marxist" approach to political development. This sampling method yielded an n of 16 respondents.

The Questionnaire Study

In all, 47 scholars were asked to respond to a confidential questionnaire concerning their 1) academic career, 2) funding support, 3) contact with funding agencies,

4) publication record, 5) relationship with colleagues, and 6) personal views of the proper role of funding agencies. The questionnaire's 15 questions were both fixed-choice and open-ended, in a ratio of 11 fixed-choice to 4 open-ended questions. (See Appendix IV for cover letters and questionnaire.)

Several factors mitigate against a high response rate and lead us to expect a relatively large amount of missing data. First, busy scholars are often unable to answer all the requests for information they receive. Thus, several respondents replied that "the spirit was willing," but time to complete the questionnaire was simply not available. Others filled out the questionnaire only partially, thus boosting the response rate while still aggravating the problem of missing data.

Second, the subject of funding support has become a sensitive one for some academic scholars, partially as a result of the vehemence of the leftist critique of academic involvement with the military establishment surrounding the Camelot Affair and the Vietnam War. Thus, the questionnaire was occasionally perceived as a veiled accusation of corroboration with the military establishment, and flatly rejected with either defensiveness or hostility.

A third problem arose in locating several of the sampled scholars. Leaves of absence, sabbaticals, and trips to conduct field research occasionally made a scholar

unavailable to respond to the questionnaire. In addition, some scholars in the "neo-Marxist" sample were "marginal" within the academic community at the time their research was conducted, and, whereas many of these scholars are now at elite universities, enjoying the current popularity of their point of view, others have "disappeared" from the academic community and could not be located.³

In spite of these problems, the questionnaire response rate is quite respectable: 64% (30) of all respondents completed and returned the questionnaire. More specifically, 64% (20) of "conventional" scholars of political development responded to the questionnaire, and 62% (10) of the "neo-Marxist" sample responded. Certainly this response rate permits us to use the questionnaire survey for purposes of generating grounded hypotheses concerning the influence of the funding structure on other factors in the model of the causes of the rise and decline of academic research trends.

* * *

The linkages between the funding structure and other factors in the model will be discussed sequentially; in each case the discussion will involve three steps. First, those hypotheses from existing literature which are clearly useful will be discussed. Questionnaire results relevant to the linkage will then be reported, followed by additional hypotheses which emerge from the results reported.

The theoretical linkages between the funding structure and other causal factors within the model will be discussed in the following order: 1) the funding structure and political development research, 2) the funding structure and dominant disciplinary norms, and 3) the funding structure and the academic reward structure.

The Funding Structure and Political Development Research

Only one scholar in the existing literature has systematically examined the effect of funding on the emergence of social science research trends. In four different publications, James L. McCartney explored the proposition that trends in social science research are influenced by the availability of financial support for research. Specifically, McCartney formulates four hypotheses which may explain the nature of the relationship between the funding structure and social science research trends.

McCartney's four hypotheses, each originally formulated in his unpublished dissertation, are:

1) As funding support increases within an area of specialization, the area of specialization will increase in prominence relative to areas not so supported. (McCartney, 1965:55-56)

2) As research subject matter becomes more controversial, funding support for such research will decline. (McCartney, 1965:51)

3) Private foundations are more likely to support controversial research; federal agencies are less likely to support such research. (McCartney, 1965:53)

4) As research subject matter becomes more practical (i.e., applicable to social problems), it is more likely to receive funding support. Conversely, as research subject matter becomes less practical, it is less likely to receive funding support. (McCartney, 1965:52)⁴

McCartney, testing his hypotheses within sociological literature, finds support for each hypothesis, and notes that ". . . other factors must be considered in explaining trends in research, but it is apparent that economic factors cannot be dismissed as being inconsequential." (McCartney, 1965:194) Does the case study of political development research also support McCartney's hypotheses?

McCartney's first hypothesis, predicting that an increase of funding support in one area of specialization will lead to an increase in that area's prominence within the discipline, is supported by the case of political development research. Funding of the study of the politics of developing nations grew during the decade of the 1960's, along with the growth of social science support in general documented in Chapter V. Additionally, political development research benefited from its substantive relevance to comparative politics, area studies, and international relations, all heavily supported by federal and private funding

during the 1960's. The increase of funding support did, in this case, correspond to a rise in the prominence of political development research.

More interesting still is the fact that McCartney's second hypothesis, that as research subject matter becomes more controversial, funding support for that research will decline, is also supported in the case of political development research. Because "neo-Marxist" political development research was critical of the status quo, anti-regime in orientation, and favorably disposed, in many cases, toward revolutionary activity within Third World nations, it was extremely controversial, more especially so because of the delicacy of the U. S. involvement in the Vietnam War and the dissention that involvement created during the decade of the 1960's. The questionnaire survey reveals that research in the "neo-Marxist" approach to political development was poorly supported in relation to "conventional" political development research.

Scholars whose research reflected the "conventional" approach to political development were very heavily supported in their research by private and public funding agencies, relative to those scholars whose research was of the "neo-Marxist" approach. Specifically, 17 of the 20 respondents (85%) whose research was "conventional" received funding support from government and private agencies during the decade of the 1960's, either individually

or through their university. In the case of the 10 respondents whose research was "neo-Marxist" in orientation, only 4 of the respondents (40%) received funding support for their research from federal and private agencies during that period.

TABLE 1: Political Development Scholars' Receipt of Research Funding Support from Agencies Outside the Academic Community During the Decade of the 1960's by Approach to Political Development Research

	Conventional Scholars		Neo-Marxist Scholars	
	n	%	n	%
Scholars individually funded or funded through universities	17	85	4	40
Unfunded scholars	3	15	6	60
	<u>20</u>	<u>100%</u>	<u>10</u>	<u>100%</u>

Further, funding support for "conventional" political development research was far larger in volume than was research support for "neo-Marxist" political development scholars. Whereas 8 (26%) of all supported "conventional" scholars responding to the questionnaire received total research support of \$100,000 or more, none of the "neo-Marxist" scholars received support of such magnitude. Thus, there is a significant discrepancy, in both the occasion of funding support and the quantity of funding support, between the two samples.

TABLE 2: Level of Research Funding Support Received by Scholars of Political Development from Agencies Outside the Academic Community During the Decade of the 1960's by Approach to Political Development Research^a

Level of Research Funding Support	Conventional Scholars		Neo-Marxist Scholars	
	n	%	n	%
\$100,000 and above	8	50	0	0
\$50,000 - \$100,000	1	6	1	11
\$10,000 - \$50,000	2	13	0	0
under \$10,000	2	13	2	22
unfunded	3	18	6	67
	<u>16</u>	<u>100%</u>	<u>9</u>	<u>100%</u>

^aFour "conventional" scholars and one "neo-Marxist" scholar were excluded from the table because, while reporting research funding support received indirectly from agencies outside the academic community through their universities or colleges, these respondents failed to report the level of that support.

Do political development scholars agree or disagree that funding support is related to the controversiality of research? The answer corresponds closely to the respondent's research identification. Of 17 "conventional" political development scholars responding to the question

In your opinion, do you think it is likely that research which is controversial or unorthodox by the standards of your discipline would be supported by private foundations, public funding agencies, or quasi-public agencies?

13 (76%) answered that they do believe controversial research would be supported. On the other hand, of the 5

"neo-Marxist" political development scholars responding to the same question, all 5, or 100% answered that they did not think controversial or unorthodox research would be supported.

TABLE 3: Political Development Scholars' Expectation that Controversial or Unorthodox Research Would be Supported by Funding Agencies, by Approach to Political Development Research

	Conventional Scholars		Neo-Marxist Scholars	
	n	%	n	%
Expect controversial or unorthodox research to be supported	13	76	0	0
Expect controversial or unorthodox research not to be supported	4	24	5	100
	<u>17</u>	<u>100%</u>	<u>5</u>	<u>100%</u>

In a question designed to probe the above question more specifically, respondents of both samples were asked to identify the type of funding agency thought to be most likely to support controversial or unorthodox research. The fixed-choice question limited the respondent to one of four answers; private foundations, mission-oriented government agencies, basic research government agencies, or quasi-public agencies.

Respondents who replied that they did not believe funding support could be obtained for controversial or

unorthodox research (100% of the "neo-Marxist" sample) were not probed on this question. However, of the 13 (72%) "conventional" respondents who answered the question, 10 (77%) identified the private foundation as the agency most likely to support controversial research. The same number (10) responded that mission-oriented government agencies were least likely to support controversial or unorthodox research. Two respondents from the "neo-Marxist" sample, who answered the question in spite of their belief that controversial research would not be supported, identified the private foundations as the agencies most likely to support such research. Respondents to the political development questionnaire study obviously agree with McCartney's hypothesis that private foundations are more likely to support controversial research than are federal agencies.

While the case of political development research lends support to McCartney's first three hypotheses, support for the fourth hypothesis is equivocal. McCartney suspects that, as research subject matter becomes more practical (i.e., applicable to social problems), it is more likely to receive funding support. In the case of political development research, the "practicality" of a piece of research is difficult to determine, since there is disagreement on the usefulness of political development research to policymakers. For instance, Robert Packerham argues that:

The extent of direct use of political development theories by policymakers was very low. In terms of indirect use, which is harder to assess, theories may have been slightly more influential, but the general situation appears to be similar.

(Packenham, 1973:284)

On the other hand, speaking of the "conventional" approach to political development, Herbert Spiro maintains that:

The shortcomings of (this approach) are not restricted to academic analysis but also affect concrete policies that politicians and governments are likely to recommend and pursue.

(Spiro, 1970:147)

Until the debate over the usefulness of political development research to policymakers is resolved, its "practicality" cannot be assessed. It should be pointed out, however, that the "neo-Marxist approach certainly produced research less practical to policy makers than did the "conventional" approach, simply due to the system-challenging nature of "neo-Marxist" research, for research which conforms to the assumptions of existing policies and programs is bound to be more readily applicable to those policies and programs than is research which calls for a basic rethinking and change of direction. To the extent that the former type of research, "conventional" political development research, was more heavily supported by funding agencies outside of the academic community than was neo-Marxist" research (85% vs. 40% of each sample received support, as reported above), McCartney's hypothesis is supported by our case study.

Two further findings of interest emerge from the questionnaire data. First, most scholars of political development, regardless of approach, feel that they have not been influenced by the availability or deprivation of research funding. In response to the question

When you recall your research and writing during the 1960's, do you feel that the development of your research interests was pulled in one direction or pushed away from another direction as a result of funding?

13 (87%) of the 15 scholars of the "conventional" approach responding to the question answered that they had never been influenced by the availability of funds for research. Similarly, 3 of the 4 "neo-Marxist" scholars responding to the question also felt they had not been influenced in terms of the direction of their research by the availability of research funding support. The findings indicate a high level of belief among academic scholars in their own intellectual autonomy.

A second finding of interest is that, of the 4 political development scholars of the "neo-Marxist" approach who did receive funding support for their research during the decade of the 1960's, 3 of those scholars expressed cynicism about the grantors' intentions. Two excerpted quotations will convey the opinions of these scholars:

Occasional mavericks like myself were 'funded' in the post-Castro NDFL grant wave, as the Establishment sought to beef up its pool of multi-lingual 'brains' for cooling the fires of revolt in the Third World. Rather than be grateful for the years of support and education these grants made possible for poor folk

like me, these mavericks 'opportunistically' sought to find out the truth and to serve the people. That is how I got a Ph.D. and successive firings from five university teaching positions. You can quote me on it.

My opinion (just that) is that Ford and other foundations offered money to American radicals for studies in the Third World precisely as intelligence gathering, and also to channel energies away from critical studies of the USA.

It would seem that belief in individual academic autonomy is so strong in these cases that the respondents believe "dirty money" can be turned to good use by a right-thinking academic. Hence, the receipt of funding is disassociated from the package of intentions attributed to the grantor--a position which invests the funding institution with very little ability to influence research, should the scholar choose not to conform to funding agency goals.

The last two findings lead us to generate one hypothesis as a result of the questionnaire study of political development research: The more controversial the research subject matter, the greater the scholar's awareness of outside influences on his work.

In the case of political development research, this hypothesis was, somewhat surprisingly, not supported. "Neo-Marxist" scholars, whose research was more controversial than that of the "conventional" approach, demonstrated a strong belief in their own intellectual independence. The findings, however, are not strictly clear and merit further exploration, as several questions remain

intriguing. For instance, does the perception of academic autonomy on the part of those conducting controversial research extend to their assessment of others who are conducting less controversial research? One suspects not: that "controversial" scholars believe "noncontroversial" scholars are influenced by the availability of research funds. Conversely, do those scholars who are conducting noncontroversial research believe those conducting controversial research are free of any influence from funding research availability? Here, one suspects that they do, assuming that research of a controversial nature is of little interest to funding agencies, and that such funding is of little interest to "marginal" or "alienated" scholars.

Further research is necessary in order to confirm or undermine these sorts of speculations. The hypothesis generated from the questionnaire study does, however, serve to raise the interesting issue of academic scholars' concept of their own intellectual independence.

The Funding Structure and Dominant Disciplinary Norms

Once again, James L. McCartney has made the principal contribution in existing literature concerning the linkage between the funding structure and other factors in our theoretical model. McCartney has formulated two hypotheses:

1) The more statistical the research, the more likely it will receive funding support. (McCartney, 1965:55)

2) Research utilizing interviews and questionnaires is more likely to receive support from funding agencies than is research which does not utilize these techniques. (McCartney, 1965:55)

The hypotheses address the ways in which funding support encourages the development of social research in certain directions through support of certain approaches and methodologies. There are two underlying assumptions: that funding agencies will tend to support research techniques which promise to yield social science research with a high degree of usefulness to policymakers, hence often support quantitative research, and that funding support will permit a researcher greater latitude in the use of statistical techniques in the collection and analysis of data.

McCartney finds evidence within the sociology literature to support his hypotheses in two separate studies. (McCartney, 1965; McCartney, 1970) His evidence is not conclusive, however, because he was not able, in either study, to examine the content of research which was refused support by funding agencies. An examination of rejected research proposals is necessary in order to determine definitively that non-statistical research and research based on library data sources was discouraged by lack of funding support. The crucial question is: Is research of a non-quantitative type discouraged by a lack of support from funding agencies?

The questionnaire study of political development researchers does not, unfortunately, solve McCartney's problem of missing information. We do know that statistical analysis and the use of interviews and questionnaires was on the increase in the social sciences prior to and during the dramatic influx of funding support into social science research discussed in Chapter V. The case study of political development research does not support McCartney's assertion that funding agencies tend to support research which will yield quantitative data of use to policymakers, for the "conventional" approach to political development (by far the more heavily funded of the two approaches) is only occasionally characterized by the use of questionnaire and interview studies. But the question remains: When the funding agencies facilitate research using statistical techniques and interview and questionnaire methods, are they merely "riding with the tide" of a trend toward statistical analysis in the social sciences, or are they actually changing the direction of the social sciences by discouraging research of a non-statistical nature because it is unacceptable to their criterion of usefulness?

In order to adequately test McCartney's hypotheses, evidence must be obtained from the files of the funding agencies themselves. Proposals submitted to funding agencies, but denied funding support, should be examined for their methodological orientation. The results of the

analysis would indicate whether or not research was systematically denied funding which would not have employed statistical techniques.⁵ Four funding agencies were petitioned for access to their files in the early stages of this dissertation, in order to systematically examine those proposals concerning political development which were refused funding support. In all four cases, the Rockefeller Foundation, the Foreign Area Fellowship Program of the Ford Foundation, the Social Science Research Council, and the National Science Foundation (the only federal agency petitioned for access to its files), access was denied. As a result, the case study yields no hypotheses concerning the relationship between the funding structure and dominant disciplinary norms above and beyond those formulated by McCartney.

All four institutions cited the privacy of individuals as the reason for denial of access. The legality of such reasoning is open to some question, and a test in the courts would prove most interesting. A central issue in a legal challenge to the funding agencies would be the status of unfunded proposals as public or private documents, a classification which would determine their availability for public inspection under the Freedom of Information Act. In the case of the Rockefeller Foundation, the Foreign Area Fellowship Program, and the Social Science Research Council, the three private foundations petitioned,

a classification of the documents as public documents may be legally justified by the foundations' privileged tax status. In the case of the National Science Foundation, a federal agency, there seems a still stronger legal foundation for the classification of documents in its files as public documents. Since it is the taxpayers' money which is being distributed in the act of funding research, it may not be unreasonable to expect that the taxpayer would legally be assured access to the documents involved in that distribution of funds.

The Funding Structure and the Academic Reward Structure

Existing literature yields no hypotheses concerning the relationship between the funding structure and the academic reward structure, yet intuition suggests that a relationship does exist. It seems reasonable that when a scholar obtains research support, his career benefits to some degree. Thus, we might hypothesize: As a scholar receives a greater amount of funding support, he is increasingly likely to be successful in his career. The hypothesis is based on an assumption that research support at minimum defrays research costs, and at maximum, makes certain types of research possible: the completion of research is greatly facilitated and the scholar is almost certain to experience some incremental advancement in career status through publication of the research.

Political development scholars of the "conventional" approach support the hypothesis that funding serves to advance academic careers in their response to the question:

Do you feel that your development as a scholar has been helped or hurt by the availability of research support from funding agencies?

Of 16 scholars responding to the question, 12 (75%) noted that the funding support which they received helped their development. Several cited research opportunities made possible by large grants; several others mentioned that funded research resulted in publications helpful to their careers.

Positive response to the question, as just noted, was not unanimous. Two scholars responded that funding support did not affect their development, and two others responded that funding support was harmful. In the latter two cases, the termination of funding support before the completion of research-in-progress was cited as harmful to scholarly development. One respondent noted that research performed under contract early in his career was ultimately harmful to his development due to publication restrictions on research results.

The beneficial influence of the funding structure on the academic career of supported scholars is recognized by "neo-Marxist" scholars of political development, as well as by "conventional" scholars. Three of the four "neo-Marxist" scholars funded by agencies outside the

academic community responded to the question, all stating that funding was helpful to their careers, citing research and travel the support made possible. One unfunded "neo-Marxist" scholar noted that a lack of funding support has limited his ability to travel. Thus, it would seem that there is widespread agreement among social science scholars of political development that one's career does indeed benefit from research support from funding agencies, regardless of their ideological orientation or personal opinion toward funding agencies.

A final hypothesis emerges from the questionnaire data concerning the role of consultantships in the academic reward structure. An interestingly large number of "conventional" scholars reported having served as consultants to funding agencies on a regular basis: in fact, 15 of the 20 "conventional" scholars responding to the questionnaire responded that they had served as a consultant at least once. Indeed, one scholar had served in six such positions during the 1960's. The mean number reported was 3. (The standard deviation of the number of consultantships reported is 1.4).

The frequency of respondents' consultantships begs a closer look at the point of contact: the foundation or federal agency. Surely such consistent consultations with the funding agencies by academics has implications for the academic reward structure. Though no formal hypotheses have been formulated, several popular notions exist regarding

the nature of those implications: one of which is that the frequent consultation of certain eminent scholars by the funding agencies is an arena where the "old boy network" might operate. Funding agency consultantships, as this idea goes, provide opportunities for a scholar to generate support for colleagues and graduate students. As a hypothesis, it might be stated: The selection of academic consultants by funding agencies increases the likelihood of career advancement for colleagues and students of those scholars selected who have a similar substantive and methodological orientation.

It seems reasonable that academic consultants to funding agencies will promote work which mirrors their predispositions through their judgments of submitted proposals. This might be perceived as another dimension of "old boy" control. To the extent that research support helps to advance an academic career, those young scholars whose work is respected by established scholars would be relatively more certain of career success than would those whose work is considered too innovative, is unpopular with the established elite, or is shoddy in terms of fixed academic standards.

A final speculation on the data findings addresses the role of the funding agencies as facilitators of academic communication. As social science increasingly adopts a system of communication similar to that of the natural

sciences (that is, more extensive use of journals, research reports, and panel presentations at professional conventions as channels for speedy communication), consultantships to funding agencies may become more desirable as a function of the opportunities they afford for staying on top of recent developments in a scholar's field of research. Thus, the academic consultantship may become an established part of the communication system in academic social science: as such, it is a badly researched part of that communication system and needs further investigation.

* * *

The questionnaire study of political development scholars reported here explores only a small part of the theoretical model developed in this dissertation. Nonetheless, it clarifies certain aspects of three linkages within the model. These are the relationships between 1) the funding structure and academic research trends, 2) the funding structure and dominant disciplinary norms, and 3) between the funding structure and the academic reward structure. Ideally, all the linkages suggested throughout the previous chapters could eventually be tested: Chapter VI ends with the logical desire that we might have the opportunity to judge the real worth of a theoretical explanation by thoroughly testing it against reality.

CHAPTER VI

NOTES

¹Unfortunately, the choice of two samples excludes a group of scholars who are not highly visible, but may have been very influential in a specific speciality of political development research. These scholars might have been included in the sample, save that there is no systematic method for identifying them, and a systematic sampling technique is one of the requirements for "grounded theory" stipulated by Glaser and Strauss. (1976:1)

²Had the reviews of the political development literature been spread throughout the decade of the 1960's in a relatively even fashion, it would have been necessary to divide each scholar's "score" (i.e., the number of times he was cited in the reviews) by the number of years elapsed since the publication of the work cited. It was not, however, necessary to do so because the reviews cluster at the end of the 1960's and beginning of the 1970's; therefore, works published in mid- and late- 1960's are nearly equally likely to be cited in the ten reviews examined.

³Robert Packerham describes the "neo-Marxist" scholars of political development as "marginal," with the following clarification:

. . . such few radical challenges as did appear came not from the most prominent and influential American social scientists, but from social scientists at the margins of the American academic establishment . . .

Packerham then elaborates in a footnote:

The phrase 'margins of the American academic establishment' refers mainly to people teaching in the less prestigious American universities and/or scholars whose works were less widely read than those reviewed above. I do not mean to imply that these social scientists were marginal in this sense solely or even mainly because of their radical views. The roots of this marginality are manifold and too complicated for treatment here. This subject merits further study. A good brief discussion is C. Vann Woodward, "Our Own Herrenvolk," New York Review of Books, April 12, 1971, pp. 12-13.
(Packerham, 1973:290-291)

⁴McCartney's hypotheses have been slightly recast for the sake of clarity when presented here, in order to prevent the reader from becoming confused by McCartney's original formulations, which are not truly in the form of hypotheses.

⁵Admittedly, an examination of rejected research proposals would not indicate how many proposals are discouraged at a stage prior to submission to a funding agency. Among certain circles of scholars, funding agencies may have a reputation (justified or not) for funding research which is statistically oriented, in keeping with the "useability" criterion dominant within policy-makers circles. Such a reputation would suppress the rate of submission of research proposals of a non-statistical nature.

CHAPTER VII

SUMMARY AND CONCLUSIONS

The principal thesis developed in Chapters I through VI is that in spite of elaborate legal and social mechanisms designed to protect intellectual freedom, the American intellectual in the academic setting is subject to social and political controls which shape and direct his or her research. We have been particularly concerned with the influence these controls exert on the substance and quantity of research; that is, how the controls affect trends (sometimes called fads) in academic research. The central questions examined have been: What factors might cause a research trend to emerge?; What factors might cause the trend to decline in importance?; and, Do these factors also influence the content of the research trends?¹

Finding no satisfactory theory in which to nest the questions, it has been necessary to construct a theory. First, four existing areas of inquiry, each relevant to an explanation of academic research trends, were presented and discussed. Next, one concept was derived from each of the four areas of inquiry, and finally, all four concepts were brought together to form a theoretical model of the causes of the rise and decline of academic research trends.

As the reader will recall, the four areas, and the derived concept for each area, are:

<u>Area of Inquiry</u>	<u>Derived Concept</u>
1) Sociology of knowledge	1) ideology
2) Sociology of science	2) academic reward structure
3) Philosophy of science	3) dominant disciplinary norms
4) Politics of science	4) societal reward structure

Causal linkages among the concepts and between each concept and academic research trends have been posited. The result is a model, in the sense of a theoretical pattern showing the interrelations of the different concepts. A brief review of each chapter will recall the "high points" of the development of the model.

Chapter I described the research trend chosen to serve as a case study, political development research in the decade of the 1960's, and its counterpart, an area of research treating much the same subject matter in a dramatically different perspective, a perspective which emerged as a separate research trend at the beginning of the 1970's. In our discussion, the latter research trend was labeled the "neo-Marxist" approach to political development.

Chapters II, III, and IV each reviewed one of the derived concepts, examining its potential contribution to an explanation of social science research trends.

Throughout each chapter the concept was explored for its possible influence on research trends, often using the example of political development research to illustrate the influence the concept might exert. At the conclusion of each chapter the concept was linked theoretically with other concepts in the model.

Chapter V discussed the societal reward structure, itself one of the derived concepts. The discussion was limited to that part of the societal reward structure labeled the "funding structure." Specifically, concern was limited to research support and professional consultancies outside the academic community. The influence of the funding structure on research trends, and on other factors in the model, was explored in a questionnaire study of political development researchers, reported in Chapter VI.

* * *

No conclusions, as such, are appropriate here, for the principal contribution of this dissertation is its development of the skeleton of a theoretical explanation of the causes of research trends in the social sciences--in short, one version of a "political sociology of social science."

The value of our explanation of the causes of research trends rests in its actual verifiability. Each reader will judge individually the merits of the evidence presented here, collected from both existing literature

and original data obtained in interviews and by the questionnaire study. It must be noted, without intending to prejudice that individual judgment, that evidence that pervasive social and political controls influence academic research is growing and, with this dissertation, hopefully has begun to be integrated into a single theoretical model.

CHAPTER VII

NOTE

¹The normative question underlying the dissertation's central thesis is whether or not social and political controls should influence academic research. The question has been avoided here, in conformity with the logical positivist argument that we cannot know what should be done until we know what reality exists. However, even the most adamant advocate of logical positivism must admit that personal values on normative dimensions appear in subtle and illusive ways throughout the scholarly effort. In this dissertation, the reader may see evidence of a normative position "creeping into" the otherwise objective analysis, my own tentative preference for academic autonomy, based on a suspicion that 1) scholarship only suffers in quality when its practitioners lose control of it, and 2) the less autonomous the academic community, the less able it is to serve as social conscience and critic to the larger society. Obviously the latter position rests on a personal belief that the intellectual's role in society is to serve as both discoverer of knowledge and as societal critic and conscience. While the preference for academic autonomy is currently dominant in my own personal value structure, it is a tentative preference, for a competing argument, that academic autonomy allows privileged scholars to remove scarce resources from society without accountability to the population as a whole, is also persuasive.

APPENDIX I

CHRONOLOGY OF EVENTS AND LEGISLATION

- 1950 Establishment of the National Science Foundation by the National Science Foundation Act.
- 1954 Report of the Special (Reece) Committee to Investigate Tax Exempt Foundations. Report #2681, 83rd Congress, 2nd Session.
- 1958 National Defense Education Act, Title VI, Public Law 85-864, as amended.
- 1961 Mutual Education and Cultural Exchange Act, Public Law 87-256, widely known as the Fulbright-Hays Act.
- 1964 Project Camelot is cancelled.
- 1965 Subcommittee on International Organizations of the House Committee on Foreign Affairs publishes Behavioral Sciences and the National Security. Report No. 4, together with Part IX of the Hearings on Winning the Cold War: The U. S. Ideological Offensive, 89th Congress, 1st Session, 1965.
- 1966 Federal Support of International Social Science and Behavioral Research. Hearings, Subcommittee on Government Operations, U. S. Senate, 89th Congress, 2nd Session, 1966. Fred Harris (Chairman).
- 1966 Congress adds Title IX ("Utilization of Democratic Institutions in Development") to the Foreign Assistance Act.
- 1966 Bill to establish a commission on a White House Conference on the social and behavioral sciences, H.R. 15458, 89th Congress, 2nd Session.
- 1967 Bill to provide for the establishment of a National Foundation for the Social Sciences, S. 3896, 89th Congress, 2nd Session, 1967.
- 1966 Bill to establish a National Social Sciences Foundation, and for other purposes. H.R. 15459, 89th Congress, 2nd Session.

- 1967 The Use of Social Research in Federal Domestic Programs, Parts I-IV, A Staff Study, Research and Technical Programs Subcommittee, Committee on Government Operations, House of Representatives, 90th Congress, 1st Session, 1967. H.S. Reuss (Chairman).
- 1967 National Foundation for Social Sciences. Hearings, Subcommittee on Government Research, Committee on Government Operations, U. S. Senate, 90th Congress, 1st Session, 1967. Fred Harris (Chairman).
- 1967 Equitable Distribution of Research and Development Funds by Government Agencies. Hearings before the Subcommittee on Government Research of the Committee on Government Operations, 90th Congress. 1st Session, 1967. Parts I, II, and III. Fred Harris (Chairman).
- 1967 Bill to change the organization and operation of the National Science Foundation (to include social science in its specific charge). H.R. 5404, 90th Congress, 1st Session, 1967.
- 1968 The Behavioral Sciences and the Federal Government. Advisory Committee on Government Programs in the Behavioral Sciences. Washington, D.C., National Academy of Sciences. Report. D. Young (Chairman).
- 1969 Hearings Before the Committee on Ways and Means, Tax Reform. U. S. House of Representatives, 91st Congress, 1st Session.
- 1969 Tax Reform Act.

APPENDIX II

INTERVIEWS CONDUCTED

<u>Interview Number</u>	<u>Interviewee's Role</u>	<u>Length of Interview</u>
1	Professor of Political Science with a long history of funded research, Northwestern University	1 hour
2	Administrative Official, Rockefeller Foundation, New York City	1 hour 45 minutes
3	Administrative Official, Foreign Area Fellowship Program, New York City	1 hour
4	Administrative Official, Social Science Research Council, New York City	1 hour
5	Administrative Official, Rockefeller Foundation, New York City	1 hour
6	Administrative Official, National Science Foundation, Washington, D.C.	1 hour
7	Professor of Industrial Engineering with a history of funding. Northwestern University	1 hour
8	Associate, National Academy of Public Administration Foundation, Washington, D.C.	2 hours 20 minutes
9	Professor of Sociology, University of Illinois, Chicago Campus. Previously associated with the National Science Foundation.	2 hours
10	Professor, Northwestern University. Previously associated with the Russell Sage Foundation.	1 hour
11	Professor, Northwestern University. Associated with Project Camelot.	1 hour

<u>Interview Number</u>	<u>Interviewee's Role</u>	<u>Length of Interview</u>
12	Professor, Northwestern University. Involved in funding of African Studies.	2 hours
13	Professor, University of Chicago. Involved in funding of African Studies.	3 hours, 30 minutes
14	Professor, Northwestern University. Serving as an NSF academic consultant.	1 hour
15	Administrative Official, the Spencer Foundation, Chicago.	1 hour
16	Administrative Official, Office of Sponsored Projects Administrator. Washington University, St. Louis.	1 hour
17	Professor, Washington University, St. Louis. Prominent in political development research.	1 hour
18	Professor, now an Administrative Official, Washington University, St. Louis. Prominent in political development research.	1 hour
19	Professor, Rutgers University. Prominent in political development research.	3 hours
20	Professor, UCLA, Los Angeles. Prominent in political development research, previously a consultant to the Ford Foundation.	By mail
21	Professor, CUNY, New York City. Prominent in political development research.	By mail

APPENDIX III

REVIEWS OF POLITICAL DEVELOPMENT LITERATURE

- Apter, David and Charles Andrian, "Comparative Government: Developing New Nations," in Marion D. Irish (ed.). Political Science: Advance of the Discipline. Englewood Cliffs: Prentice-Hall, Inc., 1968, 82-126.
- Hah, Chong-Do and Jeanne Schneider, "A Critique of Current Studies on Political Development and Modernization." Social Research, 35 (Spring, 1968), 130-158.
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- Mayer, Lawrence C., "The Elusive Concept of Political Development," World Politics, 17 (October, 1964), 108-120.
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- Spiro, Herbert, "Political Development," in Herbert Spiro. Politics as the Master Science. New York: Harper and Row, 1970:139-159.
- Welch, Claude E., Jr., "The Comparative Study of Political Modernization," in Claude E. Welch, Jr. (ed.). Political Modernization. Belmont: Wadsworth Publishing Company, Inc., 1971, 1-16.

APPENDIX IV

COVER LETTER FOR "CONVENTIONAL" SAMPLE
COVER LETTER FOR "NEO-MARXIST" SAMPLE
QUESTIONNAIRE

NORTHWESTERN UNIVERSITY
COLLEGE OF ARTS AND SCIENCES
EVANSTON, ILLINOIS 60301

DEPARTMENT OF POLITICAL SCIENCE

SAMPLE COVER LETTER FOR "CONVENTIONAL" SAMPLE

Dear Professor _____:

I would like to enlist your help, as an expert in the field of political development and modernization, in a case study of the funding of research in this field during the decade of the 1960's. The study will test hypotheses developed as part of my dissertation in political science at Northwestern University. Because the effect of funding on academic research is subject to several rival interpretations and the literature on this subject leaves so many questions unanswered, I am depending on your knowledge of, and opinions about funding agencies to provide the basis for a more solidly grounded analysis than now exists.

The funding which is of concern to this study comes from sponsoring agencies outside of the university. That is to say, I am interested in fellowships and research grants or contracts which you received from 1) private foundations, such as the Ford Foundation or the Rockefeller Foundation, 2) government agencies, such as the State Department, the National Science Foundation, or the Department of Defense, and 3) quasi-public agencies, such as the Brookings Institution.

I would be extremely grateful if you would fill out and return the enclosed questionnaire. Your answers will be confidential.

Sincerely yours,

Jean H. Dose

NORTHWESTERN UNIVERSITY

COLLEGE OF ARTS AND SCIENCES

EVANSTON, ILLINOIS 60201

DEPARTMENT OF POLITICAL SCIENCE

SAMPLE COVER LETTER FOR "NEO-MARXIST SAMPLE

Dear Professor _____:

I would like to enlist your help in a case study of the funding of research concerning development and modernization during the decade of the 1960's. The study tests hypotheses developed as part of my dissertation in political science at Northwestern University. Because the influence exerted by funding agencies on research in the United States is largely unexplored, I am very much interested in the experiences and opinions of those who, like yourself, looked critically at conventional notions about development and chose to analyze Third World countries in terms of imperialism, dependency, and inequality. As you are aware, this perspective was popular among European scholars during the 1960's, but has only become widespread among American scholars during the 1970's: I am anxious to discover the encouragement or discouragement you received from funding agencies when your research was essentially "ahead of its time."

The funding which is of concern to this study comes from sponsoring agencies outside of the university. That is to say, I am interested in fellowships and research grants or contracts which you received from 1) private foundations, such as the Ford Foundation or the Rockefeller Foundation, 2) government agencies, such as the State or Defense Departments and the National Science Foundation, and 3) quasi-public agencies, such as the Brookings Institution.

I would be extremely grateful if you would fill out and return the enclosed questionnaire. If you have not had any funding experience which is relevant to this questionnaire, I would appreciate having your opinions concerning funding agencies (see last page of questionnaire). Your answers will be confidential.

Sincerely yours,

Jean H. Dosé

QUESTIONNAIRE

CONFIDENTIAL

Please return to:
Jean H. Dosé
Department of Political Science
Northwestern University
Evanston, Illinois 60201

Political Development Research Questionnaire

1. What was your principal occupational affiliation during the 1960's?
(If it changed, please give dates.)

<u>Principal Affiliation</u>	<u>Dates</u>
University or College (including centers administered by universities)	_____
Private research organization (including private foundations)	_____
Journal or publishing house	_____
Federal Government	_____
Other (specify)	_____

* * * * *

The following questions specifically concern your research in the field of political development, defined broadly here as any research concerning the nature, conditions, or consequences of modernization in emerging countries.

NOTE: Questions 2 through 5 are concerned with funding in the amount of \$1,000 and more. Please include post-doctoral fellowships in your answers to these questions.

2. During the 1960's how many separate research projects concerning political development did you have funded by either government agencies or private foundations (excluding special research grants from your university or college)?

Number _____

3. Please list those funding agencies which awarded you funding, the approximate dates during which the funding occurred, the college, university, or organization with which you were affiliated at the time you applied for the funding, whether the project was a joint or individual one, the size of the award, and whether the funding was in the form of a fellowship, grant, or contract.

Please indicate the size of the award by category:

(1) small: under \$10,000 (2) medium: \$10,000 - \$50,000

(3) large: \$50,000 - \$100,000 (4) very large: over \$100,000

Name of Agency	Approximate Dates	College, University or Organization at Time of Application	Joint or Individual?	Size	Fellowship, Grant or Contract?

Name of Agency	Approximate Dates	College, University or Organization at Time of Application	Joint or Individual?	Size	Fellowship, Grant or Contract?

4. During the 1960's, how many times did you submit fellowship or research proposals concerning political development to a funding agency and did not receive support?

Number of proposals submitted _____

Number of separate projects involved _____

5. Please list those agencies which did not award you funding, the approximate date of the application, the college, university or organization with which you were affiliated at the time you applied for the funding, whether the proposal was joint or individual, the size of the request, and whether the funding would have been in the form of a fellowship, grant, or contract.

Please indicate the size of the award by category:

(1) small: under \$10,000

(2) medium: \$10,000 - \$50,000

(3) large: \$50,000 - \$100,000

(4) very large: over \$100,000

Name of Agency	Approximate Date	College, University or Organization at Time of Application	Joint or Individual?	Size	Fellowship, Grant or Contract?

6. As a junior scholar, how helpful were each of the following to you in helping you to secure funding for research purposes? (If you have had contradictory experiences in the degree to which one group has been helpful to you, please indicate by checking all appropriate categories.)

Former Mentors or Teachers

_____ Extremely helpful
 _____ Helpful
 _____ No contribution
 _____ Somewhat obstructive
 _____ Obstructive

Other Academic Colleagues

_____ Extremely helpful
 _____ Helpful
 _____ No contribution
 _____ Somewhat obstructive
 _____ Obstructive

University Officials

_____ Extremely helpful
 _____ Helpful
 _____ No contribution
 _____ Somewhat obstructive
 _____ Obstructive

Funding Agency Personnel

_____ Extremely helpful
 _____ Helpful
 _____ No contribution
 _____ Somewhat obstructive
 _____ Obstructive

Others (Specify,
 e.g., personal
 friends)

_____ Extremely helpful
 _____ Helpful
 _____ No contribution
 _____ Somewhat obstructive
 _____ Obstructive

7. As a senior scholar, how helpful is each of the following to you in helping you to secure funding for research purposes? Please check all appropriate categories.

Former Mentors or Teachers

_____ Extremely helpful
 _____ Helpful
 _____ No contribution
 _____ Somewhat obstructive
 _____ Obstructive

Other Academic Colleagues

_____ Extremely helpful
 _____ Helpful
 _____ No contribution
 _____ Somewhat obstructive
 _____ Obstructive

University Officials

_____ Extremely helpful
 _____ Helpful
 _____ No contribution
 _____ Somewhat obstructive
 _____ Obstructive

Funding Agency Personnel

_____ Extremely helpful
 _____ Helpful
 _____ No contribution
 _____ Somewhat obstructive
 _____ Obstructive

Others (specify)

_____ Extremely helpful
 _____ Helpful
 _____ No contribution
 _____ Somewhat obstructive
 _____ Obstructive

8. How have the following been helpful to you in securing funds throughout your career?

Former Mentors or Teachers

Other Academic Colleagues

University Officials

Funding Agency Personnel

Others

9. During the decade of the 1960's, approximately what percentage of your twelve month academic salary was charged to fellowships or research grants or contracts of any kind? Please check the appropriate estimate for the two periods shown below.

	<u>1960-65</u>	<u>1966-70</u>
0%	_____	_____
Under 10%	_____	_____
10-19%	_____	_____
20-29%	_____	_____
30-39%	_____	_____
40-49%	_____	_____
Over 50%	_____	_____

10. Were you ever in your career a regular consultant to, or an employee of, a private or government organization which distributes research funds (including serving on review committees in your academic role)?

Yes _____ No _____

- 10a. If so, when did you serve, in what funding agency, and what position did you hold?

Name of Agency	Approximate Dates of Service	Position Held

11. During the 1960's, what percentage of the total number of articles which you submitted to journals for publication were eventually accepted by a journal and published?

Approximate percentage _____

12. In your opinion, do you think that it is likely that research which is controversial or unorthodox by the standards of your discipline would be supported by private foundations, public funding agencies, or quasi-public agencies (such as the Brookings Institution)?

Yes _____ No _____

- 12a. If you think that controversial or unorthodox research would be supported by the funding agencies, which type of agency do you think would be most likely to support it, and which type would be least likely?

	Most Likely	Least Likely
Private Foundations		
Mission-oriented Government Agencies		
Basic Research Government Agencies		
Quasi-Public Agencies		

13. When you recall your research and writing during the 1960's, do you feel that the development of your research interests was pulled in one direction or pushed away from another direction as a result of funding? Please describe briefly any instances which you recall when your research interests were affected by funding circumstances.

14. Do you feel that your development as a scholar has been helped or hurt by the availability of research support from funding agencies? In what ways were you helped or hurt?

15. What do you see as the optimal role of the funding agencies in academic research?

Thank you for your assistance.
Please return the questionnaire in the envelope provided. If convenient, please include a copy of your curriculum vitae.

2/12/75

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VITAE

Jean Hardisty Dosé

Date of Birth: June 18, 1945

Place of Birth: Washington, D.C.

Degrees: B.A. Northwestern University, 1967
M.A. University of Southern California, 1971
Ph.D. Northwestern University, 1976