ABSTRACT

THE IMPACT OF WORK ORGANIZATIONS ON A SCIENTIFIC DISCIPLINE

by Worth Cary Summers

One of the changes that reshaped the institution of American science in the first six decades of this century was the shift in the distribution of scientific manpower among different kinds of work organizations; in particular, a shift from organizations primarily concerned with creating and disseminating new information to those concerned with transforming it into some useable form. This dissertation deals with understanding how this shift in its organizational base may have affected a segment of the larger institution of science. It focuses on one relationship between the two principal components of science, scientific work organizations and scientific disciplines, dealing with the following question: in what ways can participation in different kinds of work organizations affect the extent to which scientists participate in their disciplines?

The institution of science was conceived as having two distinct but overlapping sets of social structures: the

organizations where scientists carry out various research, teaching, and administrative activity--the work organizations; and that larger arena of activity and association among scientists who share a common theoretical and substantive concern--the scientific disciplines--such as microbiology, mathematics, molecular biology, etc.

These two structures were viewed in terms of the _exchange_ of differentially valued social goods. As incumbents of work organizations scientists exchange their compliance with the directives of the organization for various organizational rewards; and as incumbents of the disciplinary structure they exchange scientific information with their colleagues in return for some form of recognition. It is because both structures are linked through the common membership a scientist has in each that a work organization can influence his disciplinary participation. An organization can do so in two ways: first, by influencing his _opportunities_ to _acquire_ information that would be exchangeable in the discipline by virtue of the adequacy of the organization's _means_, and by virtue of the adequacy of the _ends_ toward which research was directed. The second way in which organizations might affect a scientist's participation in his discipline is by influencing his _motivation_ to participate. Motivation was regarded as being affected in part by the nature of the organization's reward system, and in part by a scientist's estimate of his opportunities to acquire exchangeable information.

Five general hypotheses dealing with these relations were tested with mailed questionnaire data from a sample of 985 full-time employed microbiologists who were engaged in some research. Five measures of disciplinary participation were used, but only one, extent of professional publishing was retained for the complete analysis because it alone, probably due to certain defects in measuring the others, gave consistent interpretable results. For this one measure, however, the hypotheses were largely confirmed. An additional hypothesis which tested the assumption that these same organizational conditions affected participation in part by <u>changing</u> motivation was also, with several qualifications, confirmed when controls for initial motivation and selective mobility were introduced. One important unexpected finding was the very great importance of motivation to participate in the discipline relative to any separate impact of the organization on participation.

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TABLE OF CONTENTS

TABLE OF CONTENTS - Continued

LIST OF TABLES

CHAPTER I

INTRODUCTION

STRUCTURAL CHANGES IN 20TH CENTURY AMERICAN SCIENCE

The dramatic transformation of the United States from a rural agrarian to an urban industrial society could not have occurred without the relatively rapid shift from a craft to a scientific technological tradition.[1] By transcending the limitations of an empirically based technology, science helped to revolutionize agriculture, to create new industries and transform others, and to provide many radically new technological means for coordinating and integrating an ever more differentiated and complex society. The urban industrial transformation also would not have been possible without the concomitant development

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[1]Although craft knowledge and trial and error invention were responsible for many of the technological developments that made the industrial revolution possible, there were limits to how far manufacturing could develop while grounded in such empiricism. Not until scientific knowledge and procedures began to supplant intuition did machinery design, raw material, and power sources, begin to be efficiently exploited. A. Rupert Hall, "The Changing Technical Act," in The Technological Order: Proceedings of the Encyclopedia Britannical Conference, Carl F. Stover (ed.), (Detroit: Wayne State University Press, 1963), pp. 117-131.

of organizations devoted to expanding the scientific knowledge base, to training a scientifically skilled labor force, and to applying scientific knowledge to a variety of new and old situations.[2] For the United States today the importance of science and of a technology grounded in science scarcely needs to be pointed out.

Society has changed radically through its association with science, but so has science changed through its growing involvement with its increasingly receptive and accommodating host. With elaborate technical and organizational facilities, an expanding and well trained scientific work force has enormously extended and elaborated scientific knowledge and the range of its application. Yet changes in the social fabric within which science is created and used, in the long run, may be equally significant. Fostered by structural changes in the society that supports it, by its growing acceptance, and by the success of its practitioners, the social structure of science has undergone at least four important modifications

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[2] A. Hunter Dupree, Science and the Emergence of Modern America, 1865-1916, (Chicago: Rand McNally and Company, 1963). Dupree attributes the difference between the place of science and technology in the 19th and 20th centuries less to science dominating technology in the 20th than to the changed relation between the two. By the 20th century institutional devices for translating scientific knowledge into practical forms such as the industrial research laboratory, schools, government departments, philanthropic foundations, etc., were well established.

in the first half of the twentieth century: (1) the number of scientists has increased enormously; (2) science has become increasingly specialized and differentiated; (3) scientists have been concentrated increasingly into large scale organizations; and (4) the work organizational base of science has shifted massively away from academic toward governmental and industrial settings.

One important consequence of the closer symbiotic relationship between science and society has been the remarkable increase in scientific manpower in this century. Price estimates that the number of scientists is following an exponential growth curve and doubling every ten to fifteen years.[3] And although scientists are only one component of the more general growth in technical and managerial occupations that has accompanied technological development and the emergence of new organizational forms, they have increased relative to professional and technical workers and to the labor force as a whole.

> Between 1930 and 1960, while the civilian labor force increased by 42 percent and professional and technical workers by 126 percent, the number of engineers rose over 290 percent, and the number of scientists more than 625 percent. In the period from 1954 to 1963, engineers increased by 300,000 and scientists by 210,000--about 46 and 105 percent respectively.[4]

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[3]Derek J. de Solla Price, Little Science Big Science, (New York: Columbia University Press, 1963), pp. 1-6.

[4]National Science Foundation, Scientific and Technical Manpower Resources, NSF 64-28, p. 12.

In 1930 scientists and engineers comprised only about .05 percent of the civilian labor force, by 1963 they were about 1.86 percent[5] and had increased about 1.1 million to a total of nearly 1.4 million.[6]

Stimulated in part by the challenges and opportunities of wider acceptance, science has also become highly specialized and differentiated, a condition that is clearly reflected in the growth of scientific societies and journals. Of 176 major American scientific and engineering societies whose members were "primarily professional scientists actively engaged in research and teaching," one half have been established since 1920 and more than a quarter since 1940.[7] And "hard core" scientific journals have approximately doubled every fifteen years since 1899.[8]

The third major structural transformation of science is the concentration of scientists into large work organizations, a phenomenon which, because data are available there, is seen most clearly in industry. Apparently from

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[5]National Science Foundation, Scientific and Technical Manpower Resources, NSF 64-28, p. 14, computed from figures in Table 11-16.

[6]Ibid., pp. 10-11.

[7]National Science Foundation, Dues and Membership in Scientific Societies, NSF 60-55, September 1960, p. 2, Table 2.

[8]National Science Foundation, Office of Science Information Service, Characteristics of Scientific Journals 1949-1959, NSF 64-20, p. 2 and Figure I, p. 3.

the very beginnings of industrial research, larger concerns have been most inclined to use scientific research in their operations and to increase their research commitments. In 1921 only fifteen companies had research staffs of more than 50 and by 1938, 120 companies had staffs this large, an increase which was largely the result of expansion in existing labs.[9] In 1938 less than one percent of all companies with research staffs employed one third of all research workers and 8 percent employed two thirds.[10] And by 1958, fully 93 percent of all manufacturing companies with 5,000 or more employees had research and development staffs, while in companies with 1,000 to 4,999 employees this declined to 57 percent and to only an estimated 4 percent for companies with less than 1,000 employees.[11] This tendency for

[9]George Perazich and Philip M. Field, Industrial Research and Changing Technology, Work Projects Administration, National Research Project, Philadelphia, Pennsylvania, January 1940, p. 8. Though the surveys on which these two comparisons are based are not entirely comparable in total coverage they are probably nearly equivalent for the larger companies involved here. The conclusion is also based on data from 244 companies who were included in both surveys.

[10]Ibid., pp. 9-10.

[11]National Science Foundation, Industrial R & D Funds in Relation to Other Economic Variables, Surveys of Science Resources Series, NSF 64-25, p. 6. The 350 companies with 5,000 or more employees who performed research and development "represented o.1 percent of all manufacturing companies but accounted for 93 percent of all federally financed R & D performance in industry and 81 percent of all R & D performance." Ibid., p. 5.

the largest companies to expand at the expense of smaller concerns had not abated as recently as 1962; for while total scientific man-years doubled between 1954 and 1962 this increase was almost exclusively confined to firms with more than 5,000 employees.[12] No doubt similar trends have occurred in the Federal government and in educational establishments particularly since World War II with expanding efforts in defense and space and increasing enrollment in higher education.

The changing organizational setting of science is the fourth major structural transformation, and the one from which this dissertation derives its major impetus. The vast mobilization of corporate and governmental resources for translating scientific knowledge into usable forms so characteristic of the first half of this century has been accompanied by the employment of scientists in a great variety of work settings. Unfortunately, there are no trend data to adequately reflect changes in scientific employment patterns in this century; but, judging from the above noted rapid expansion of industrial science in this period, there has been a massive increase in the proportion of scientists employed in industry and government relative to those in colleges, universities, and foundations.

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[12]U.S. Department of Commerce, Office of the Assistant Secretary of Commerce for Science and Technology, <u>Studies in Scientific and Engineering Manpower, Part I and Part II</u>, Staff Report: 63-1, Figure IB, p. 21.

Again the trend is clearest in industry. Although industrial research laboratories were virtually nonexistent prior to the turn of the century,[13] in 1927, 927 companies had research staffs, by 1938 the number had nearly doubled to 1,722,[14] and twenty years later, in 1958, one reliable estimate places the number of companies performing research and development at 12,086.[15]

Current trends in the changing distribution of scientists, although probably much less marked than those occurring in the first five decades of this century, are discernable from National Science Foundation surveys conducted in the '50s and projected to 1970. While all scientists are expected to increase by nearly 75 percent in the eleven years between 1959 and 1970, the growth will be distributed unequally among the major employers--manufacturing, government, and academic institutions--who together employ approximately 90 percent of all scientists. Manufacturing, with a projected growth of nearly 85 percent,

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[13]Howard R. Bartlett, "The Development of Industrial Research in the United States" in National Research Council, National Resources Planning Board, <u>Research--A National Resource, II.--Industrial Research</u>, December 1940, pp. 19-77.

[14]Perazich and Field, <u>Industrial Research</u>. . ., p. 9.

[15]National Science Foundation, <u>Industrial R & D Funds</u>. . ., p. 6. Because the 1927 and 1938 surveys are not entirely comparable to the 1958 survey, these figures should be taken to represent only the general growth trends.

and government, with 81.2 percent, will exceed the overall rate, while colleges and universities, with a projected rate of 57.2 percent, fall considerably below it. If these trends materialize, then manufacturing concerns employing slightly less than 41 percent of all scientists in 1959 will increase their share to 43 percent by 1970. And the higher than average growth rate in government will raise the proportion of scientists employed there from 20.7 to 21.4 percent in the same period. But proportionately fewer scientists will work in colleges and universities in 1970 than in 1959 as their share drops from 28.1 to 25.2 percent.[16]

Two significant additional changes are associated with changes in the work organizational base of science. Norms, values, facilities, and the social relationships of scientific work influence and shape it in important ways. Hence any shift in the way scientists are distributed among different work organizations also means that the normative and resource profile of the entire institution has been altered, and that the kinds of influence brought to bear on the institution and on individual scientists have also been modified. Secondly, and related to this, these changes

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[16]Figures calculated from Tables A-17 and A-18, National Science Foundation, *The Long Range Demand for Scientific and Technical Personnel: A Methodological Study*, NSF, 61-65, p. 49.

also represent a dramatic shift in the scientific division of labor: a sharp decrease in roles necessary to create and disseminate scientific knowledge relative to roles for translating it into usable forms. Such fundamental changes in the institution of science have no doubt had many significant and far reaching consequences. This dissertation, however, is limited to considering only one of these; one that stems directly from the unique structural composition of the institution of science itself.

As we shall see in the following chapter, the institution of science consists of <u>two</u> distinct but overlapping social structures, scientific work organizations and scientific disciplines, both of which profoundly influence scientific work. Furthermore, a scientist's participation in a scientific discipline, as well as a discipline's influence on scientific behavior, largely depends on a scientist's position in the scientific division of labor and on a number of related characteristics of the organization in which he works. Thus changes in the organizational setting of science over time may have substantially changed patterns of participation in scientific disciplines, and, consequently, in the total institution of science itself.

It is from this background of historical change in the distribution of scientists within different work settings and its possible effect on behavior patterns in science that this dissertation derives its major focus. Unfortunately, the available data do not permit us to correlate historically the connections between changes in the organizational base of any scientific field and changes in the disciplinary participation of those in that field. What is attempted here is necessarily less ambitious; it is an attempt to clarify one important relationship between the principal structural components of the institution of science, work organizations and disciplines. In particular, this dissertation represents an effort to furnish some theoretical and empirical answers to the following question: <u>in what ways can participating in different kinds of work organizations affect the extent to which scientists participate in their disciplines</u>?

Existing theoretical frameworks do not deal both with scientific work organizations and with disciplines, let alone suggest how being involved in different work organizations may affect a scientist's participation in a scientific discipline. The theoretical portion of the answer, therefore, is necessarily rather extended, occupying Chapters II and III. Chapter IV provides a partial empirical test of hypothesized relationships between characteristics of work organizations and the extent to which scientists participate in their scientific disciplines. Chapter V focuses upon organizationally induced <u>changes</u> in the disciplinary participation or motivation of scientists. And finally, Chapter VI summarizes and evaluates the main theoretical and empirical results, and suggests lines for further research and analysis.

CHAPTER II

THE TWO SOCIAL STRUCTURES OF SCIENCE

Introduction

The institution of science is not easily studied. In large measure this is because it consists of two quite different structures, work organizations and scientific disciplines, though the discipline seems to contribute a disproportionate share of the difficulty. Sociologists have developed relatively effective theoretical and methodological tools for dealing with large-scale organizations and can bring these to bear on scientific work organizations. Moreover, work organizational membership is concentrated within reasonably compact settings; positions and roles are often designated by the organization, and it is fairly easy to establish first-hand contact with members and organizational processes; hence it is not too difficult to gain a concrete, wholistic picture of much of the organization's essential structure and operation. Scientific disciplines, in contrast, present more formidable difficulties to those who would study them: they are less neatly bounded than work organizations, their positions are less clearly demarcated, and their roles are more ambiguous. And while there are formal offices and functions, these apply only to what are, in some ways, the least important disciplinary features, being largely unrelated to the many contacts between individual scientists across organizational boundaries. And finally, their much larger membership, far from being conveniently located in a single setting, is found in many separate organizations throughout the North American continent and, increasingly, throughout the world. It is not surprising, therefore, that empirical studies in the sociology of science have, until quite recently, been overwhelmingly of work organizations rather than disciplines. How does one study such a diffuse social entity as a scientific discipline? What concepts or theories are appropriate? We sometimes forget that there is no _right_ way of viewing social phenomena; there are only the more or less useful or pleasing. And despite abundant evidence, we often overlook the fact that even though sociology has its orthodoxies, it is still in search of its "paradigms," Thomas Kuhn's apt term for the cohesive, tradition-giving models of theory, procedure, and epistemology in science.[1]

As with other intricate social phenomena, many perspectives or paradigms can be made to yield important and

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[1]Thomas S. Kuhn, _The Structure of Scientific Revolutions_, International Encyclopedia of Unified Science, Vol. II, No. 2 (Chicago: University of Chicago Press, 1962).

useful information. Yet as each contributes uniquely to our understanding, it also excludes other information: construing something one way necessarily means that we have not construed it in alternative ways; and insights from one perspective are not automatically yielded by another. One must, therefore, give some thought to adopting a point of view for looking at scientific disciplines, and before creating a "new" one, should seriously consider the advantages of existing alternatives. The remainder of this chapter will be devoted to such an assessment.

Two Views of Science

Roughly speaking, sociologists have tended to adopt one of two perspectives toward scientific disciplines. One, emphasizing values, I will refer to as the "traditional" position. And the other, emphasizing structure and situational variation, I will refer to as the "contextual" position, though in doing so I do not intend to imply that it represents a uniform and consistent theoretical stance, but only that there is similarity in emphasis and viewpoint.

The traditional view has been presented most forcefully and fruitfully in Merton and Barber's numerous writings in the sociology of science. Though they differ somewhat on precisely what values are involved, these authors see science in terms of distinctive norms and values that are morally binding on scientists.[2] The values are:

> transmitted by precept and example and reinforced by sanctions [and] are in varying degrees internalized by the scientist, thus fashioning his scientific consciousness or . . . superego.[3]

The scientist's "activities [are] devoted to definite moral values and subject to clear ethical standards."[4] In fact, "moral values are always present in the everyday working practices of scientists, however unconscious of them some scientists may be."[5] These values flow from and govern behavior directed toward the principal institutional goal of science, "the extension of certified knowledge", and from "originality", a prime criteri~~a~~on for determining whether knowledge is new.[6]

Despite their pervasive influence on scientific activity, however, these overarching values in the

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[2]Robert K. Merton, "Science and Democratic Social Structure," Chapter XVI in Social Theory and Social Structure (Glencoe, Illinois: The Free Press, revised and enlarged, 1957), p. 551; Bernard Barber, Science and the Social Order (New York: Collier Books, revised edition, 1962), p. 122.

[3]Merton, "Science and Democratic. . .," p. 551.

[4]Barber, Science and the Social Order, p. 122.

[5]*Ibid.*, p. 123.

[6]Robert K. Merton, "Priorities in Scientific Discovery" in Bernard Barber and Walter Hisoch (eds.), The Sociology of Science (New York: The Free Press, 1962), p. 463.

traditional view, are neither codified nor easily observed. Instead they must "be _inferred_ from the moral consensus of scientists as expressed in use and wont, in countless writings on the scientific spirit and in moral indignation directed toward contraventions of the ethos."[7] This same point is echoed by Barber, who adds that ceremonial gatherings also provide opportunities during which the values are more clearly displayed.[8] From observations of such exceptional circumstances, Barber concludes that behavior within a scientific discipline is shaped by the following values.

1. _Rationalism and emotional neutrality_.

2. _Universalism_, the belief that "scientific truth is not conditional upon the social or personal qualities of the individual scientist."

3. _Individualism_, an anti-authoritarianism in which the scientist is free to pursue his own investigations while submitting to the absolute moral authority of science.

4. _Communality_, a belief in the absence of secrecy and that "all contributions to the fund of scientific knowledge and conceptual schemes are community property."

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[7]Merton, "Science and Democratic. . .," p. 552, my emphasis.

[8]Barber, _Science and the Social_. . ., p. 123. Probably Merton and Barber's emphasis on underlying values which surface only under special circumstances is due in no small measure to their methodology. Both have relied extensively on historical material which is probably heavily biased toward reporting the exceptional circumstance, and toward commentaries by and about unusually articulate scientific statesmen.

5. Disinterestedness in which scientists "are expected by their peers to achieve the self interest they have in work-satisfaction and prestige through serving the community interest directly."[9]

Thus in the traditional view scientific behavior is shaped or flows from a specific set of values to which scientists, through prolonged and intensive socialization, feel morally bound. How adequate is this for dealing with the issues confronted in this dissertation?

One drawback of this traditional view is that it applies to only a narrow segment of roles comprising the scientific division of labor--those primarily concerned with extending and elaborating scientific knowledge and, particularly, with the elite of science who have attained great eminence in their fields. These values, which in the traditional view shape scientific behavior, may provide a very useful description of this group. However, though I cannot adequately demonstrate it without a lengthy digression, it is a far less adequate description of the behavior of scientists engaged chiefly in disseminating scientific knowledge, or in adapting it to some practical end-roles with which this dissertation also deals.

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[9]Barber, Science and the Social. . ., pp. 128-132. Merton's list of values, upon which Barber has based his, differs in only minor ways from this, somewhat longer catalogue. See, Merton, "Science and Democratic. . .," pp. 553-561.

There is another peculiarity of the traditional position that limits its usefulness in this dissertation: though it presumably attempts to characterize or explain scientific <u>behavior</u>, in actuality it describes shared but uncodified and possibly even unconscious <u>values</u>, the existence of which, as we have seen, can only be detected under rather exceptional circumstances. Why is this? I suggest that the position tacitly assumes that values are the most basic social datum: that regular behavior patterns emerge only from shared values, that behavior proceeds from the top down, so to speak. It is difficult to see how the traditional view can, by focusing on values as the principal explanatory device, account for behavioral variation within science. Such variation cannot be due to inadequate socialization, for as we shall see in later chapters, the variations are patterned and occur under specific situations: in certain types of organizations and work roles, for example.[10] Must we then posit other values to account for this deviation? Surely that would parallel the early fruitless attempt by some psychologists to reduce behavior to a set of needs,

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[10]For example, Krohn has shown that attitudes toward various aspects of scientific work vary systematically with work situation. Roger G. Krohn, "The Institutional Location of the Scientist and His Scientific Values," IRE Transactions on Engineering Management, EM-8, 3 (September 1961), pp. 133-138; and "Science and the Practical Institutions," <u>Proceedings of the Minnesota Academy of Science</u>, Vol. 28, 1961, pp. 163-172.

which obliged them to compile ever longer lists to account for the variety of human activity. By concentrating on value the traditional perspective is also unable to account for changing scientific behavior or attitudes, another consideration of this dissertation. If values account for behavior, then changes in behavior must be due to changes in values; yet the traditional view suggests no mechanisms by which values are changed.

Finally, the traditional perspective deals almost exclusively with only one component of the institution of science, the discipline, and neglects relations between disciplines and work organizations, and the effect of work organizations on scientific behavior, another focus of this dissertation. Barber appears to sense that the work setting has an impact on moral values when he observes that "scientists act somewhat differently in different kinds of organizations, in the university and in industry, say, with regard to such matters as secrecy in research and the patenting of discoveries."[11] And he goes on to comment that "the limits on some of these ideals [or values] are characteristically greater in 'applied' science."[12] But adherence to a set of uniform overarching

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[11]Barber, Science and the Social. . ., p. 134.

[12]Ibid., p. 136.

values as the principle explanatory device, and the neglect of situational factors, leaves the traditional position unable to explain why values are less effective in industry than in universities and in applied rather than basic research, though Barber suggests vaguely that somehow they are limited by organizational interests.[13]

In sum then, there are several reasons why the traditional position is inadequate for dealing with issues confronted in this dissertation. First, it focuses on a small, select group of scientists: those in basic research. Second, it tends to ignore actual concrete social behavior in favor of values abstracted from unusual circumstances. And third, because of this nearly exclusive reliance on explaining scientific behavior according to a uniform set of morally binding norms or values at the expense of concrete social and material circumstances, it is unable to account for departures from the behavior of the "typical" scientists, and, most seriously, is unable to systematically deal with the relationship between the two principal scientific social structures, the work organizations and the disciplines. Because it focuses on the relationship between different work organizations and disciplinary

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[13]Barber, Science and the Social. . ., pp. 136-137.

participation, this dissertation requires a theoretical perspective that can relate different concrete social and physical work environments to concrete behavior in a wide range of scientific work roles. The following quote by Blau, echoing many of the forgoing thoughts, provides a fitting critical summary to this discussion of the traditional theoretical position.

> A concern with social action, broadly conceived as any conduct that derives its impetus and meaning from social values, has characterized contemporary theory in sociology for some years. The resulting preoccupation with value orientation has diverted theoretical attention from the study of the actual association between people and the structures of their associations. While structures of social relations are, of course, profoundly influenced by common values, these structures have a significance of their own, which is ignored if concern is exclusively with the underlying values and norms.[14]

The remaining pages of this chapter will attempt to show that by emphasizing situational and contextual factors the dissertation's theoretical requirements can be more adequately met.

Those writers who have adopted what I call a contextual position toward science focus more on social relations among scientists, and, in particular, place much greater emphasis on the social and physical situation or context

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[14]Peter M. Blau, Exchange and Power in Social Life (New York: John Wiley and Sons, Inc., 1964), p. 13.

in order to explain their behavior. Aside from this similarity in viewpoint, however, they share no consistent theoretical framework with which to view the institution of science. We must look elsewhere then for a _theoretical_ framework that gives proper consideration to concrete social relationships between scientists in _different_ organizational settings, and which takes into account relationships between work organizations and disciplines. In the interest of parsimony I should also like to consider work organizations and disciplines, as well as essential relationships between them, in terms of a common conceptual and theoretical framework. The general concept of a _structure_ appears to meet these requirements.

Structures

The scientific work organization and the discipline, then, are to be construed as having structures of particular kinds. Now, "to exhibit the structure of an object," to use Bertrand Russell's deceptively simple dictum, "is to mention its parts and the ways in which they are interrelated."[15] So the task in the next few pages will be to "exhibit" the structure of both work organizations

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[15]Bertrand Russell, _Human Knowledge--Its Scope and Limits_ (New York: Simon and Shuster, 1948), p. 250.

and scientific disciplines in such a way that they have comparable "parts" and "interrelations."

The task of "mentioning" the parts and relationships present no special difficulties, although, as it will become subsequently clear, it requires judicious selection from among numerous possibilities. The "parts" will consist of social <u>positions</u>, and the "relations" will consist of what can be temporarily referred to simply as <u>behaviors</u>.

There is a growing awareness that "position" and "relation" must be defined with respect to one another.[16] Gross, Mason, and McEachern have graphically described this interdependence.

> The meaning of location /parts or positions in the present discussion/ is not . . . entirely self-evident. It is difficult to separate the idea of location from the relationships which define it. Just as in geometry a point cannot be located without describing its relationship to other points, so persons cannot be located without describing their relations to other individuals; the points imply the relationships, and the relationships imply the points.[17]

This nexus between positions, relations, and other positions has been made an explicit core feature of Sim's recent role

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[16]Francis Montgomery Sim, <u>An Explication of the Logical Model of Role Systems</u>, Unpublished Ph.D. dissertation, Michigan State University, 1966. Especially, p. 91.

[17]Neal Gross, Ward S. Mason and Alexander McEachern, <u>Explorations in Role Analysis: Studies in the School Superintendency Role</u> (New York: John Wiley and Sons, Inc., 1958), p. 48.

system axiomatization. Sim formally argues that social positions, at least in one of their most sociologically and theoretically interesting senses, consist of relations between other positions, and that roles are sets of relations between positions. All of this is succinctly stated in Sim's axiomatization in which "position" and "relation" are primitive terms.

> Consider any relation, R_i, and any two positions, P_j and P_k (which are not necessarily distinct), each having been selected from its appropriate set. Then R_i either joins P_j and P_k or it does not, and R_i either joins P_k and P_j or it does not. Taken together these two conditions result in four possible combinations which we could symbolize as follows, using R_i to show that the relation does not join the pair.
>
> 1) $P_jR_iP_k$ and $P_jR_iP_k$,
>
> or 2) $P_jR_iP_k$ and $P_j\bar{R}_iP_k$,
>
> or 3) $P_j\bar{R}_iP_k$ and $P_jR_iP_k$,
>
> or 4) $P_j\bar{R}_iP_k$ and $P_j\bar{R}_iP_k$.
>
> This display shows a feature . . . viz., that in general relations are taken to be "directed," which position comes first and which second must be specified. . . . In the following we will refer to an expression of the form "$P_iR_jP_k$" as a "predicate" and one of the form "P_j" (or "P_k") as an argument for ease of reading.
>
> 1) A role-sector is a collection of predicates whose first arguments are identical and whose second arguments are identical.
>
> 2) A focal role is a collection of predicates whose first arguments are identical.

3) A counter role is a collection of predicates whose second arguments are identical.

4) A role is a focal role and a counter role whose respective first and second arguments are identical.[18]

So as to forestall any misunderstanding concerning those role structures, one point deserves special emphasis. This framework is a formal analytical tool, neither right nor wrong in its present form. We can reasonably expect of such a scheme that it orders and assists in explaining important social phenomena. What we choose to regard as important phenomena is crucial, for it dictates, or should dictate what theoretical framework as well as what relevant observations should be made. I stress this point because the role structure model presented above is highly sensitive to the investigator's interpretation of the abstract and as yet empirically empty terms. <u>Any</u> relation or sets of relations can be selected to define a role sector or a position. As Sim observes:

> . . . the model does not preclude use of any set of relations whatsoever, . . . the model neither precludes nor requires treatment of any given relational structure as a role system. As a conceptual tool it may be used well or poorly, or it may not be used at all. <u>If</u> it is used, then the role concepts are applied to the chosen

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[18] Sim, <u>An Explication</u>. . ., pp. 95-96.

> content. _Whether_ it should be used must be decided in terms of substantive considerations of theoretical relevance, and these are not resolved by the logical form.[19]

Thus, while Gross, _et al_. have effectively used the class of "expectations" or "evaluative standards" as the defining relations of roles,[20] no inherent properties of Sim's formal definition requires this or, for that matter, any other relation. Hence, as an investigator I am free to select those relations that offer the best possibility for explaining the substantive conditions that I regard as important. These conditions may at some point be best explained using "expectations" as the defining relations, but there are no _a priori_ grounds for asserting that this is necessarily the most legitimate approach.

What relations fit the present situation? Another feature of the formal role structure model bears directly on this point. Sim's axioms clearly show that by varying the defining "collections" of relations, we can vary the number of roles and role sectors and hence the formal structure. Thus, just as we may freely choose to use relations that simplify theoretical and analytical tasks, we may also choose them to generate any structure that we have reason to believe would be useful in a given situation.

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19 Sim, _An Explication_. . ., p. 188.

20 Gross, _et.al._, _Explorations_. . ., pp. 58-64.

As in choosing appropriate relations, this must be done with an eye to both the empirical situation at hand and the formal apparatus we intend to apply to it. All of these considerations follow directly from Sim's Axiom of Relation, his definitions of role-sector, focal and counter role, and role: <u>the roles and hence the structure of some social systems are fixed just by selecting a collection of defining relations</u>.

Because the number of role sectors, roles, and positions depends on the choice of defining relations between positions, our theoretical task is somehow to come to an optimal balance between having, on the one hand, broad relations with fewer positions or role sectors, or, on the other hand, narrow relations with more role sectors. One advantage to broader relations and fewer roles is that the resulting structure is relatively simple. But against this, it might be argued that gains in structural simplicity may decrease sensitivity to empirical variations in behavior. Carried to its logical extreme, it would be theoretically possible to define a relation between two positions so broadly that any conceivable actor qualified as an incumbent. This would be clearly useless for discriminating among individuals. More narrowly encompassing relationships, the other alternative, would reflect more behavioral variation but, by proliferating roles, would lead to relatively more complicated structures.

Thus, apparently the model requires that empirical behavioral variability must be taken up either in broad relationships or in complicated structure. This characteristic of the model is incorporated explicitly in Sim's two-part Axiom of Incumbency by means of which actors are mapped into roles.

Axiom of Incumbency:

> Assume that A_i represents an actor and P_j and P_k represent two positions.
>
> 1) Either A_i occupies P_j (or P_k or both) or he does not. (There is no such thing as a slight case of incumbency.)
>
> 2) If A_i occupies P_j, then for any role-sector of P_j, A_i has all of the relations which constitute the role-sector with some incumbent of P_k.[21]

On the surface at least this appears to be a very stringent axiom, a point which Sim seems to recognize as he goes to some lengths to defend its consequences, i.e., the apparent necessity of translating empirical variation into structural complexity. He argues that this strict axiom has the virtue of permitting unequivocal measurement decisions about whether or not actors are involved in a given role sector.[22] Yet it seems that a more fundamental measurement issue is whether or not an actor exhibits any designated relation,

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[21]Sim, An Explication. . ., p. 97.

[22]Ibid., p. 97 and passim.

for this involves decisions that must be made <u>prior</u> to those relating to role incumbency. Once the investigator determines that an actor does or does not exhibit an appropriate relation it is a relatively simple matter to decide his role incumbency since this is determined by his having the specified relation. It is this, I think, that permits an investigator to circumvent the dilemma of relational scope and structural complexity. For the fact is the dilemma is only apparent, we can have the best of these two theoretical worlds.

Behavioral variation can be absorbed in a third way that need not affect relations or structural complexity. By permitting the criteria for deciding whether an actor exhibits a crucial relation or not to cover a <u>range</u> of behavior, we can reflect behavioral variation as a "relational variable." To illustrate: we might define a role as $P_iR_jP_k$ where P_i and P_k are, as before, positions, and R_j is a behavioral relation, say, "contributes valued information and receives prestige," and ranges from none to some defined maximum. Then for this definition, all scientists, for example, would be role incumbents, <u>but they would differ in the intensity or degree of the relationship</u>. By suitably defining additional relationships, incumbency could be restricted to just those actors of interest in an investigation--in the present case, to scientists.

By appropriately choosing defining relationships, an investigator would have great freedom in designing a structural model to approximate almost any empirical social system in which he might be interested.

In the present situation, by defining certain behavioral relations as variables, we can deal with crucial relational differences between scientists <u>within</u> the disciplinary structure without, at the same time, being compelled to assign scientists with slightly different relations to different positions. This is particularly convenient in situations, like the present, where behavior changes over time.[23] By thus accommodating empirical variation to the theoretical framework, we have avoided one weakness of the classical view of science, its inability to deal with behavioral variability.

Up to now I have only discussed the general properties of the role structures that will serve as a framework throughout the dissertation; the structure is formal and still lacks substantive content. Before considering the most important class of relationships that will "flesh out" these structural bones--exchange relationships--it may prove

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[23]This is not to suggest that it would never be appropriate to reflect behavioral variation by structural variability; again this is a matter entirely up to the investigator's discretion.

helpful to have a diagram of what the forgoing yields in the way of work-organizational and disciplinary structures. Figure 1 is a simplified diagram of the role structure of science.[24] For simplicity, a single line represents the set of defining relationships for the disciplinary and organizational role-sectors, and only one organizational counter position is shown although many are possible.

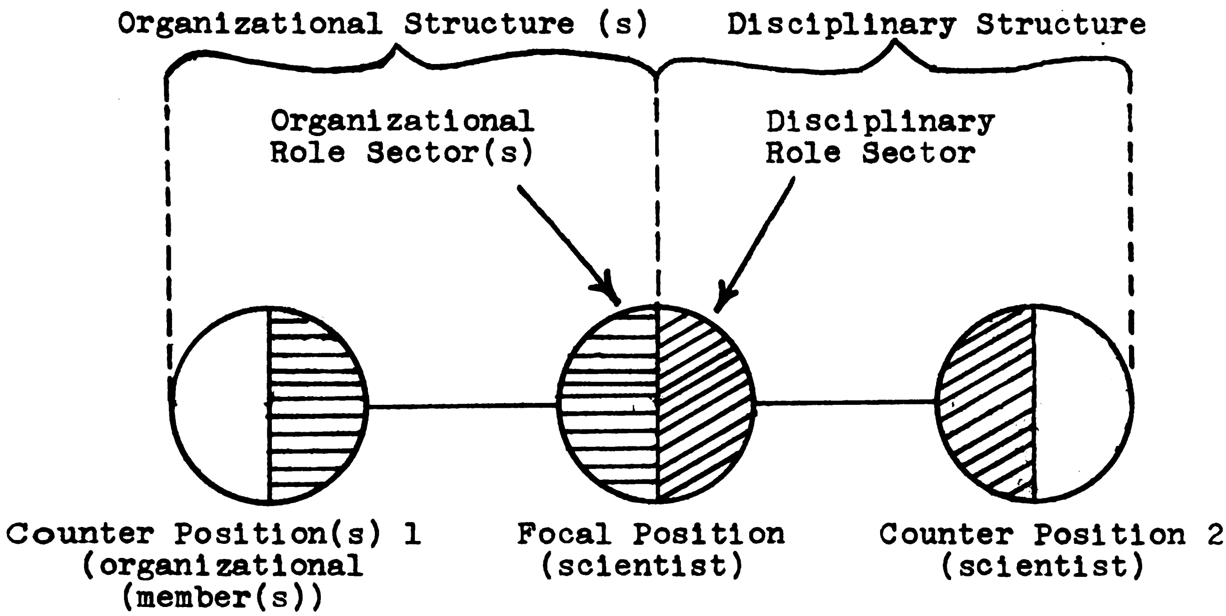

Figure 1. The Two Structures of Science

Many writers commonly distinguish between academic, governmental, and industrial work organizations, but to the extent that work organizations represent <u>unique</u> patterns of role relationships between scientists and other organizational

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[24] I have employed the graphical devices of Gross <u>et al.</u>, <u>Explorations</u>, pp. 51-55.

members these or any other organizational types, could be suitably defined with the present framework simply by choosing appropriate defining relationships. In contrast to the potential variability of work organizational structures, the disciplinary structure is simple and fixed as far as this dissertation is concerned. Behavioral variations within the disciplinary role sector will be taken up by relational variables rather than by different structures.

Figure 1 also should clarify another important characteristic of the present framework implicit in most of the forgoing discussion: many actors may occupy a single position. Of course, this does not mean that their relationships will necessarily be equivalent since actors may differ on relational variables.

With these outlines of the two social systems of science in mind we can consider in the next section an important class of relationships, exchange relationships, and the exchange structures that they generate.

Exchange Relations and Structures

There are, of course, infinitely many ways of defining relationships and hence there are infinitely many structures that might be used for analyzing empirical social phenomena. In this dissertation I will define structures of work organizations and disciplines in terms of exchange relationships, that is, in terms of exchanges of differentially valued social goods between incumbents of two positions. While this may not be the best of all possible alternative relationships that I might have chosen, several considerations persuade me that it is a good one for the kinds of problems with which this dissertation deals. First, exchange structures provide a framework for organizing much existing information on scientific behavior within work organizations and disciplines. Secondly, exchange structures are a natural (almost an inevitable) extension of exchange theory which in later chapters will carry the brunt of hypothesis formation. In short, exchange structures appear both to provide a good fit to empirical reality, and, with additional theoretical tools to be introduced later, to lead to testable propositions.

Though exchange theorists differ on what constitutes a proper theoretical domain and on what primitive processes and terms to assume, there appears to be good consensus on the meaning of an exchange. For the present discussion we will use the following definition.[25]

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[25]For both Homans and Blau, the two writers who have elaborated exchange theory most fully, the concept of exchange functions almost as a primitive term, though this is not to say that it lacks meaning. Gouldner, on the other hand, explicitly defines "reciprocity" (which is identical with exchange) as "a right (x) of Alter against Ego implies a duty (-y) of Alter to Ego. . . . [or] a duty (-x) of Ego to Alter implies a right (y) of Ego against Alter." However, this definition has connotations of fixed obligation associated with "rights" and "duties" that I prefer to avoid. Alvin W. Gouldner, "The Norm of Reciprocity: A Preliminary Statement," American Sociological Review, Vol. 25, No. 2 (April 1960), p. 168. Blau's and Homans'

> An exchange relationship obtains between two actors,[26] A and B, whenever A directs toward B behaviors with positive or negative value for B _because_ B has previously directed toward A behavior with positive or negative value for A.

But exchange theory, a large and rapidly growing body of propositions and assumptions, involves much more than exchange relationships, though these are central to it. Abstracted as it is from the theory, this definition conveys little of the behavioral dynamics that underlie it. There is no need at this point to present a detailed summary of exchange theory; even if it were desirable, it is entirely too complex for a short synopsis. Instead, I will discuss only those aspects of it that are minimally necessary to show how exchange theory ties in with _structures_ to generate _exchange structures_, and how the institution of science can be construed in terms of exchange structures. The following chapter will deal with exchange theory in greater detail in deriving testable hypotheses about the effect of work organizations on disciplinary participation.

Exchange relationships may occur when one actor, desiring benefits from another, attempts to obtain these

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treatment of exchange theory can be found in: George Caspar Homans, _Social Behavior: Its Elementary Forms_ (New York: Harcourt, Brace and World, Inc., 1961); and Peter M. Blau, _Exchange and Power in Social Life_ (New York: John Wiley and Sons, Inc., 1964).

[26] "Actor" may refer to either individuals or groups.

by offering benefits in return or in exchange. The rationale is simple: we reward others with services or activities they value. Exchange need not be quite so calculating as this formulation may seem to imply, however; Hamblin and Smith's recent work, for example, suggests that bestowal of rewards upon another may be grounded in involuntary feelings of approval or disapproval for attributes differentially valued in a group.[27] But, on the other hand, involuntary feelings of approval or disapproval need not necessarily lead to overt displays of corresponding activity. As we all know, deference and condescension, approval and disapproval, praise and condemnation, and other displays of esteem and disesteem may be geared to factors other than one's feelings. But whatever the reason for it, the essential point is that differentially valued activities of some sort are exchanged between two actors.

The range of things that actors find beneficial or rewarding is potentially vast but bounded by the social situation in which exchanges occur, by pre-existing notions of what constitute "fair" exchanges, by the expectations directed toward incumbents of various position, and by cultural definitions of the valuable. Hence for some group

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[27]Robert L. Hamblin and Carole R. Smith, "Values, Status, and Professions," Sociometry, Vol. 29, No. 3, (September 1966), pp. 183-196.

there will always be a finite, even if possibly quite a large, class of behaviors that may be exchanged. Because social approval (or esteem) may be employed in a wide variety of situations, it is one of the most common rewards for valued attributes or activities of others.

Earlier we argued that it was possible to regard scientific work organizations and disciplines as having structures defined by certain relations between positions. And we have specified that these relations consist of exchanges between positions. Hence we are conceiving of scientific work organizations and disciplines as comprised of exchange structures. From an array of possible exchange we can let certain sets define the scientific positions and the associated roles and role-sets for the two structures, and some of these relations can be designated as variables. In later chapters we will be dealing with two principal classes of variables: those having to do with behaviors--how much of an activity an actor directs toward (or receives from) another; and those having to do with value--how much positive or negative reward an actor receives in exchanges, or how valuable an actor regards a potential exchange, that is, his motivation to engage or behave in a potential exchange.

Thus far I have argued that both scientific work organizations and disciplines might best be treated in a common conceptual framework, provided by the general concept of structure. Structures are parts and relations between parts. For scientific disciplines and work organizations the parts are social positions; and the relations are exchange relationships, that is, they consist of exchanges of valued activities between the positions. Hence, the structure of science consists of two systems of exchange structures, work organizations and disciplines. In the next section of this chapter we will see how well science fits this exchange structure model and also what exchange relationships make up the work organizational and disciplinary structures of science.

Exchange Structures and Science

Beginning with the discipline, the activity of scientists that has received the widest attention and the most careful analysis is the process of making discoveries and communicating them to others. Although we discarded the traditional view as an inadequate analytical framework for the problems dealt with in the dissertation, it provides, surprisingly enough, some of the best substantive support for an exchange structure model of science. It is in Merton's elegant and now classic presidential address for The American Sociological Association in 1957[28] that it first becomes apparent that

[28]Merton, "Priorities. . ."

communicative behavior in science fits the present definition of an exchange relationship. Although Merton did not discuss the process of communicating research results in terms of exchange relationships and exchange theory, his analysis readily lends itself to such an interpretation.

Characteristically, Merton begins with an extensive historical cataloguing of the "frequent, harsh, and ugly" priority disputes that enliven the history of science. He points out that these disputes have frequently involved scientists who were ordinarily neither querulous nor assertive, as well as their friends who apparently had nothing to gain from participating. Merton argues that these controversies have generated such passion and have involved others not directly affected because to the participants, their opponents have violated a <u>moral</u> norm.

> . . . as we know from the sociological theory of institutions, the expression of disinterested moral indignation is a sign-post announcing the violation of a social norm. . . . The very fact of . . . [the bystanders] entering the fray goes to show that science is a social institution with a distinctive body of norms exerting moral authority and that these norms are invoked particularly when it is felt that they have been violated. In this sense, fights over priority, with all their typical vehemence and passionate feelings, . . . constitute responses to what are taken to be violation of the institutional norms of intellectual property.[29]

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[29]Merton, "Priorities. . .," p. 454.

We recognize here in Merton's argument the traditionalist's technique of inferring the presence of scientific norms from behavior. But Merton elaborates further and, because his line of reasoning is instructive, I quote from it extensively.

The institution of science demands that scientific knowledge advances, and "on every side, the scientist is reminded that it is his role to advance knowledge and his happiest fulfillment of that role, to advance knowledge greatly." Because an advance is necessarily also an original contribution, "in the institution of science originality is at a premium."

> When the institution of science works efficiently . . . recognition and esteem accrue to those who have best fulfilled their roles, to those who have made genuinely original contributions to the common stock of knowledge. . . . Recognition for originality becomes socially validated testimony that one has successfully lived up to the most exacting requirements of one's role as a scientist, and contributes directly to his self image as a scientist.

And how do scientists come to value recognition?

> It is not necessary that individual scientists begin with a lust for fame; it is enough that science, with its abiding and often functional emphasis on originality and its assigning of large rewards for originality, makes recognition of priority uppermost. Recognition and fame then become symbol and reward for having done one's job well.

Furthermore,

> Once he has made his contribution, the scientist no longer has exclusive rights of access to it. It becomes part of the

> public domain of science. Nor has he the right of regulation its use by others by withholding it unless it is acknowledged as his. In short, property rights in science become whittled down to just this one: the recognition by others of the scientist's distinctive part in having brought the result into being.

Merton goes on to speculate that

> It may be that this concentration of the numerous rights ordinarily bound up in other forms of property into the one right of recognition by others helps produce the great concentration of affect that commonly characterizes disputes over priority. /And/ may also account for the deep moral indignation expressed by scientists when one of their number has had his rights to priority denied or challenged.[30]

There is a subtle but revealing and important shift in the course of Merton's argument: at the beginning scientists were incensed at possible challenges to the priority of their discoveries because a moral norm had been violated; he then observes that they are motivated to achieve priority because of the many institutional rewards for which this qualifies them and because their rights are limited to only these; and he ends by seemingly suggesting that what really bothers scientists in a priority dispute is that they stand to lose rewards, they stand to be deprived of valued responses from others in the form of recognition for their achievements.

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[30]All quotes are from Merton, "Priorities . . .,", pp. 455-56.

Merton has apparently found it necessary to bolster his claim that the disputes result somehow from moral indignation with an appeal to a more earthy theory: people get mad when they don't get what they feel is coming to them, and they get mad at those who appear to be the source of this deprivation. Those familiar with Homans' work will recognize in this a rather loose paraphrase of his proposition 5:

> THE MORE TO A MAN'S DISADVANTAGE THE RULE OF DISTRIBUTIVE JUSTICE FAILS OF REALIZATION, THE MORE LIKELY HE IS TO DISPLAY THE EMOTIONAL BEHAVIOR WE CALL ANGER.[31]

That is, Merton seems to have imperceptibly, but nonetheless significantly, relied on some assumptions about the behavior of scientists that are very close to those in exchange theory.

True, an established form of behavior, an exchange relationship, had been disrupted, and a principle of exchange was violated: the reward for a valued contribution was to be withheld or, worse, go to another. In this sense norms have been violated; but I would suggest that it is more instructive to assume that the scientists were concerned because of the potential costs and rewards involved and because an established way of allocating and distributing these rewards was disrupted--a condition which posed a direct threat to their proprietory rights to their discoveries and the

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[31]Homans, Social Behavior, p. 75.

potential rewards this entailed. Their concern, I suggest, is firmly rooted in calculated costs and potential costs to themselves, and only secondarily in values as moral values; the values support established patterns of reciprocity or exchange and are secondary to these.[32] The norms that have been violated are norms of reciprocity.[33]

For now, however, I am more concerned with using exchange structures to characterize science than with the exact mechanisms that are involved in scientific behavior. In later chapters I will argue that behavior in accordance with specific norms or values is a function of potential costs and rewards in the exchanges that the norms and values represent, and testable hypotheses will be derived from this principle. It is sufficient here to observe that Merton's description of the scientific property rights exactly fits our definition of an exchange relationship: scientists bestow prestige and honor on their colleagues because they

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[32]Storer adopts a position very similar to the one presented here when he observes that, "Scientists subscribe to the norms of science first of all because of their importance for the continued adequate circulation of the commodity in which they are mutually interested," and later, that "the norms [of science] are derivative of scientists' interest in the reward." Storer's position is discussed more fully later in the chapter. Norman W. Storer, The Social System of Science (New York: Holt, Rinehart and Winston, 1966), pp. 84 and 85.

[33]Alvin W. Gouldner, "The Norm of Reciprocity: A Preliminary Statement," American Sociological Review, Vol. 25, No. 2, (April 1960), pp. 161-178.

have previously made original contributions of information to their discipline.

Thus the scientist who makes original contributions receives social approval from his colleagues. The approval may be indirect, however, for his discoveries are likely to be published and used perhaps without his knowledge, possibly even long after the discovery. Establishing clear priority provides reasonable assurance that whenever the contribution is used the contributor will receive social approval in some form of appropriate acknowledgment. If he is lucky, a scientist's discovery may even be named after him, and such eponymy automatically assures him social approval each time his property is used. Eponymy, prizes, lectureships, offices in professional societies not only widen the audience bestowing social approval, but may alter the criteria for bestowing approval from specific accomplishments to admiration and respect for one's ability, thereby assuring generalized approval for past accomplishments.[34]

Numerous career contingencies are also determined by a scientist's recognition and prestige: research grants, opportunities to collaborate on research, desirable jobs, and students all accrue to those who have demonstrated their ability.[35] Having prestige and a reputation for

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[34]Blau, Exchange and Power. . ., p. 63.

[35]F. Reif, "The Competitive World of the Pure Scientist," Science, Vol. 134, 3494, (December 1961); Hagstrom, The Scientific Community (New York: Basic Books, 1965), pp. 111-149.

scientific prowess makes one's services more valuable to others in exchange for which they are willing (or forced through competition) to provide valued inducements in return. In science social approval functions much like a universal standard of value that can be exchanged for a wide range of social commodities.[36]

Because it is obvious and hence easily overlooked, it is well to emphasize the reciprocity of the information-prestige exchange relationship and the other relationships in which prestige is converted to (i.e., exchanged for) other social goods. Scientists are not just earning prestige, good jobs, or grants, they also provide valuable services to others: information, stimulating discussion, constructive council.

The values with which the traditional perspective characterized science can now be seen to describe, among other things, conditions under which this principal exchange relation takes place: rules for directing scientific effort and guaranteeing its genuineness (rationality and emotional neutrality), for determining who may make discoveries and the conditions of their acceptance (universalism and individualism), and for limiting the discoveror's claims upon users and establishing exchange boundaries (communality

[36]Blau, Exchange and Power. . ., pp. 62-63.

and disinterestedness). To the extent that these are values which in fact do contribute to shaping behavior (an assumption which we questioned earlier) the perspective sees these as rooted in the concrete transactions worked out and continually adjusted in countless individual encounters between scientists. As descriptions they roughly reflect the rules according to which contributions are made and rewards allocated.[37] They are, furthermore, always subject to change as the concrete situation affects the costs and rewards of the behaviors entering the relationship.

Other writers have noted the close affinity of exchange theory to Merton's analysis of the importance of priority in science. Drawing upon this analysis, Hagstrom has made the process of exchanging information for recognition central to his important book, The Scientific Community.[38] For Hagstrom, scientific discoveries are "gifts" (a special kind of exchange) to the scientific community which establishes the donor's status and involve the recipients in obligations to repay him with recognition

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[37]Although his interpretation is somewhat different, Storer also regards the norms of science as governing the conduct of exchanges. Storer, The Social System of Science, p. 39 and pp. 76-90. See also f.n. 32 above.

[38]Warren O. Hagstrom, The Scientific Community. . . .

and social approval.[39] Hagstrom regards the exchange of information for recognition as the principal mechanism of social control in science: desire for recognition leads scientists to communicate their findings, and because findings are received only if they conform to acceptable standards and deal with problems deemed important within the community of science, the discipline is able to exert control over problem selection and methods of investigation.[40]

Otherwise unexplainable scientific behavior becomes more readily understandable under the assumption that scientists attempt to exchange information for recognition. The common practice of rushing into print with partial results and incompletely analyzed data, as well as secretiveness about one's work, can be seen as attempts to establish or preserve one's priority, and hence one's property rights, over discoveries in an intensely competitive research market.[41]

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[39]Hagstrom, The Scientific Community. . ., pp. 12-23. As Hagstrom points out, however, scientists aren't supposed to appear to seek recognition, for this would call into question their commitment to the higher goals of science, the selfless search for truth. Ibid., pp. 19-21. For rather different interpretations of the norm against striving for recognition see, Robert K. Merton, "The Ambivalence of Scientists," Bulletin of the Johns Hopkins Hospital, Vol. 112, No. 2 (February 1963), pp. 77-79; and Storer, The Social System of Science, pp. 105-106.

[40]Hagstrom, The Scientific Community. . ., pp. 16, 21, 52.

[41]Ibid., pp. 70-98, and for a similar interpretation see Reif, "The Competitive World. . ., pp. 1957-62.

Norman Storer, in his recent book, The Social System of Science, also adopts an exchange theoretical perspective toward science that is based on Merton's analysis of the scientific reward system. Taking a slightly different perspective from Hagstrom, Storer views science as a social system organized around the exchange of "competent response" from others for one's creative contributions to knowledge. Storer is interested in why scientists value the norms of science and in why they bother to communicate their findings. He concludes that competent response from others concerning a creative effort is a basic component of the creative act itself. It is important because it confirms that one's contribution is valid and meaningful to the common store of knowledge. And as already indicated in previous discussion, Storer believes that the norms of science receive the allegiance of scientists because they facilitate the exchange of a desired commodity--competent response.[42]

But science is not limited only to exchanges of original knowledge for recognition. Because scientific knowledge changes so rapidly and is so highly differentiated and specialized, scientists need to establish contacts with others who can supply them with already existing information

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[42]See f.n. 29 above.

that relates importantly to their work. Menzel has shown that, at least for the university scientists he studied, communication with others is an absolutely essential component of a great deal of scientific research. It may provide information that is the key to solving a research problem, it may stimulate new research ideas, and may provide informal feedback for unfinished work. Information may come at formal gatherings of a specialty or field, at work, or from colleagues in other parts of the country. And much truly vital information appears to be conveyed under wholly accidental or fortuitous circumstances, in chance encounters and conversations in a variety of situations.[43] Given the present state of knowledge, we cannot be certain precisely what is exchanged _for_ information, but it is highly likely that under various circumstances this will consist of other information, social approval and deference.

In addition to supplying information relating to their work, informal communication may help to maintain scientists'

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[43]Herber Menzel, _The Flow of Information Among Scientists: Problems, Opportunities and Research Questions_, Columbia University Bureau of Applied Social Research, (May 1958); "Planned and Unplanned Scientific Communication," _Proceedings of the International Conference on Scientific Information, Washington, D.C., 1958_, (Washington, D.C.: National Academy of Sciences--National Research Council, 1959), pp. 199-243; "The Information Needs of Current Scientific Research," _The Library Quarterly_, Vol. XXXIV, No. 1, (January 1964), pp. 4-19.

motivation in the face of the somewhat impersonal and often delayed rewards that are formally available through the discipline. It appears that social approval is also bestowed on those who display valued attributes short of the highly prized original formal contributions to knowledge.[44]

But exchange relations also serve another function. A scientists' image of himself as a scientist as well as his self-esteem probably depend to a very great extent on the sorts of responses he receives from his colleagues regarding his role performance. For this reason, the various forms of social approval and recognition he receives (or fails to receive) take on an added significance which probably underlies much of the value that such behavior has for him. This is not to imply that all scientists conceive of themselves as making original contributions, but only that whatever self-role conception they hold for themselves will entail certain specific activities and levels of performance and certain responses from others that correspond to and confirm or disconfirm these. Making original contributions is only one possible conception of a scientific role, but if a scientist

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[44]Hagstrom, The Scientific Community. . ., pp. 36, 49.

conceives of himself in such terms then he will require recognition and acknowledgement from his colleagues that he has made such a contribution.[45]

While it is not possible with data presently available to clearly demonstrate that informal communication in science conforms to exchange relationships, there is reason for believing that it would. It seems plausible that scientists would seek out those whom they believe can supply them with the most useful information, and that the latter would be reluctant to take valuable time to help others unless they received something valuable in return, whether it consisted simply of gratitude, or of esteem, information, or assistance. Blau's analysis of advice seeking among government administrators offers empirical and theoretical justification for studying consultation and advice seeking within an exchange theoretic framework.[46] At any rate, until demonstrated

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[45]Belief in the status or self-confirming significance of scientific recognition has been recognized at least in passing by a number of authors, though it has not received the focused attention it deserves. It is inherent in Hagstrom's assertion that a gift (in this case, of information) and its acceptance by others confirms an actor's status; in Storer's contention that competent response from others is required to complete the creative act--to know that one has, in fact, made a contribution; and in Merton's assertion that publication and priority signify adequate performance of the institutional role of scientist.

[46]Peter M. Blau, The Dynamics of Bureaucracy, (Chicago: The University of Chicago Press, Revised edition, 1963), pp. 121-143.

to be otherwise, the presumption that information is *exchanged* in science ought to have the important advantage of directing attention toward the *reciprocity* of those relationships in which information is passed on--on what an individual must *give* as well as receive.

Thus I am suggesting that the *structure* of scientific disciplines consists of at least three principal kinds of exchange relationships: those in which new discoveries or contributions to the fund of scientific knowledge are exchanged for various forms of social approval and recognition; those in which existing scientific information, advice, or assistance relevant to current research, is exchanged for approval or similar assistance; and finally, those in which social support is exchanged between colleagues.

Though exchange relationships are taken as defining the disciplinary structure of science, all incumbents of the role "scientist" are also members of the disciplinary role sector, and hence by definition exhibit the defining relationships. *This does not mean, however, that all incumbents participate in these relations to the same degree*. As we have defined them, these relations are variables ranging from some maximum to a minimum of no participation. Some members of the structure make many important contributions and receive a great deal of social approval in return and probably also exchange advice and information with colleagues. Indeed, much of the preceding discussion was based on evidence derived from groups of scientists who tend to be involved in this way, those in academic work organizations. Most descriptions of scientists are, in fact, of this rather special group. But the disciplinary structure has been defined to include <u>all</u> scientists, including those who may publish only rarely or who have little other information of interest to their colleagues; and it has been defined so as to reflect differences in the participation of these very different members of the disciplines of science along important dimensions of behavior--in this case, along dimensions consisting of exchange relationships.

Scientific work organizations, the second principal social system of science, can be dealt with more easily because we are more accustomed to viewing these in terms of role structures and exchanges than was the case with the scientific discipline. I noted earlier that the present definition of exchange structures would make it possible to define types of work organizations by the different kinds of behavior comprising their exchange structures, or alternatively, by the different level of behavior within an exchange structure. However, any attempts to do so will be left to later chapters; here I will only note that work organizations are often seen in terms of exchanges: compliance with organizational expectations in return for organizational rewards.[47] It should also be noted that the definition of exchange structures permit work organizational structures to be construed in such a way that they could reflect empirically separate organizational role sectors--those involving exchanges between superiors, colleagues, and subordinates.

Summary

This chapter assessed the adequacy of the traditional view of science for dealing with the central concern of this dissertation, the relationship between work setting and disciplinary participation. We concluded that the traditional view, with its emphasis on values and its general neglect of concrete social situations and relationships, failed to provide an adequate basis for accounting for possible effects of work setting on disciplinary behavior. The other perspective commonly adopted toward science, the contextual, with its greater emphasis on the social context of scientific behavior and on actual social relationships, seemed to offer a sounder basis for progress. Adopting this emphasis, we regard science as having two overlapping structures defined in terms of behavioral relations

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[47] For example, see James G. March and Herbert A. Simon, Organizations, (New York: John Wiley and Sons, 1958); Amitai Etzioni, A Comparative Analysis of Complex Organizations, (New York: The Free Press, 1961).

between positions; for present purposes these relations are regarded as exchanges. The principal exchanges comprising the discipline's structure consist of the exchange of new or already existing scientific information for recognition, social approval, or information, and the exchange of social support. The second social system of science, the work organization, similarly had its structure defined in terms of exchanges: compliance for various organizational rewards.

CHAPTER III

EXCHANGE THEORY AND EXCHANGE STRUCTURES

This chapter further elaborates the theoretical relationships between characteristics of work organizations and the disciplinary participation of scientists; and, in this connection, sets forth five specific hypotheses.

It is important to bear in mind that we are treating work organizations and disciplines in terms of their exchange structures; in terms, that is, of sets of exchange relationships. Occasionally I will refer to what is exchanged in such relationships generically as a "commodity" whether it consists of money, assistance, advice, information, or some form of social approval. And when I speak of an actor participating in one of the structures I will mean that he is involved in one of the defined exchange relationships: receiving and giving differentially valued commodities. Consequently, the concern with how different organizational settings affect a scientist's disciplinary participation is, in this view, a concern with how different work organizations affect the extent to which he exchanges commodities in his discipline. I will assume that the commodities that a scientist could exchange for disciplinary recognition--chiefly, but not exclusively, contributions to knowledge--can only be obtained, if at all, from his work organization.[1] In brief, this chapter will argue that different work organizations can affect a scientist's disciplinary participation in two ways: (1) by affecting his <u>opportunities to acquire</u> commodities that are exchangeable in his discipline, and (2) by affecting his <u>motivation</u> to acquire and exchange commodities in his discipline.

THE IMPACT OF WORK ORGANIZATIONS ON OPPORTUNITIES TO ACQUIRE EXCHANGEABLE COMMODITIES

If scientists are to <u>participate</u> in their disciplines, conditions in their work organizations must be such that they can <u>acquire</u> the commodities that will be exchangeable in the discipline. What work organizational characteristics assist or hinder scientists in making such acquisitions? First, I assume that every commodity, scientific information

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[1]Of course it is true that a scientist may acquire information from others in his discipline--for example, who is working on what research, or knowledge of a new technique--that he then may exchange in the discipline for other commodities such as gratitude or other information. But the most valuable commodities from the standpoint of the discipline, new contributions to knowledge, are generated almost exclusively by research performed in some work organization.

included, is produced directly (or indirectly as a bi-product) when some <u>means</u> or <u>facilities</u> are directed toward some <u>ends</u> or <u>goals</u>. Work organizations then will differ in the opportunities they provide researchers for acquiring exchangeable information when they differ in the means and ends of the research conducted in them.

<u>Means</u>

The social and material means necessary to produce any commodity include both the social organization of the productive process--authority relations, division of labor, etc.--and facilities--supplies, equipment, and raw materials. I will assume that there are optimal arrangements of these elements for producing any given commodity such that some combinations will be more adequate than others. Hence work organizations will differ in the opportunities they provide scientists for acquiring exchangeable information according to how adequate their means are for producing exchangeable information.

Those social and material means that maximize the production of material goods such as automobiles and chemicals, have been intensively studied and are fairly well understood. But, because they involve essential but as yet unfathomed questions of creativity, our understanding of those conditions necessary to produce less tangible commodities such as sound decisions or scientific ideas is considerably more primitive. Research on social conditions for producing scientific information has begun to consider such factors as types of on the job contacts between peers and supervisors,[2] the optimal allocation of effort between research and other activities,[3] and types of supervisory climates most conducive to research motivation and satisfaction.[4]

Although the adequacy of the organizational means for producing exchangeable information is important, this dissertation does not deal with it extensively. Different social means are considered only indirectly as these are involved in other factors affecting disciplinary participation. And because the adequacy of the facilities are likely to vary considerably with peculiarities in the type of research in which scientists are involved, these will be dealt with very generally in terms of scientists' perceptions of the adequacy of facilities and resources that are available in their work organizations for whatever research in which they may be involved.

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[2]Donald C. Pelz, "Some Social Factors Related to Performance in a Research Organization," Administrative Science Quarterly, I, December 1956, pp. 310-325.

[3]Frank M. Andrews, "Scientific Performance as Related to Time Spent Technical Work, Teaching, or Administration," Administrative Science Quarterly, Vol. 9, No. 2, (Sept. 1964), pp. 182-193.

[4]Howard Baumgartel, "Leadership, Motivations, and Attitudes in Research Laboratories," The Journal of Social Issues, Vol. 12, No. 2, 1956, pp. 24-31; and "Leadership Style as a Variable in Research Administration," Administrative Science Quarterly, Vol. 2, No. 3, 1957, pp. 344-360.

Thus, with respect to the impact of a work organization's research means or disciplinary participation, we have Hypothesis 1.

> _Hypothesis 1_. The less adequate are the facilities or means for research in an organization, the less will be the opportunities to acquire information that is exchangeable in the discipline, and hence the less will be the disciplinary participation of the scientists in that organization.

Goals

The goals or ends toward which means are directed are a second and, to me, a more sociologically interesting factor affecting a scientist's opportunities to acquire information exchangeable in the discipline. Scientific research can produce a variety of information. What goals guiding the research process should maximize the likelihood that the information produced will be exchangeable in the discipline? The most reasonable answer would seem to be: _research guided by disciplinary goals_; that is, research directed toward solving problems that the discipline regards as significant, since this would affect the likelihood that the information will be valued within the discipline and hence exchangeable there. Thus we can say that scientists will be more likely to acquire exchangeable information in those work organizations where research is directed to disciplinary rather than to organizationally defined ends. And since, as we saw in the last chapter, a scientific discipline exercises social control over research to the extent that scientists select research problems according to what they believe the discipline would regard as important contributions, we can also say that exchangeable information will tend to be acquired most readily in those organizations where the scientific discipline exercises control over research.[5]

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[5]Let us be clear about what is involved here with respect to disciplinary or organizational control of the work process. Many who have written about scientists and other professionals in large organizations have commented on differences between bureaucratic and professional authority structures. Ideally, it is suggested, bureaucratic authority is vested in hierarchically arranged offices while professional (or colleagial) authority rests in each individual's competency or mastery of expertise, and on the collective supervision of equals whose training and experience has qualified them to pass judgment on the competency of the professional's work. But our concern is with whether or not scientists direct their work according to disciplinary norms and values, <u>not</u> with the <u>form</u> of the authority structure by which control is exercised. There is an important distinction here: work that is controlled by profession<u>als</u> is not necessarily the same as work that is controlled by a profess<u>ion</u>; at issue is the relation between two <u>social systems</u> or <u>structures</u>, not the relation between <u>individuals</u> who may happen to be members of these.

Of course it is true that universities, where disciplinary control is greatest, tend to have colleagial structures, and that bureaucratic structures are found in industrial organizations where disciplinary control is often minimal. But this results, I think, from the historic fact that those organizations which applied scientific research to specific <u>organizational</u> rather than disciplinary ends already had bureaucratic authority systems, rather than from any <u>necessary</u> relationship between the extent of disciplinary control and the form of organizational authority. In other words, it should be possible, in principal at least, to design work organizations with colleagial structures but with little or no disciplinary control, or with bureaucratic structures and extensive disciplinary control. This issue is discussed at

The generality or specificity of the organization's goals is the most important characteristic determining the extent to which research is controlled by the discipline, and this is closely tied to the organization's place in the division of scientific labor: to whether it is concerned primarily with creating new scientific information, with disseminating it, or with translating it into useable forms.

Society supplies those organizations that create and disseminate scientific knowledge with the resources necessary to achieve these goals. Consequently, such organizations do not need to insure that scientific research contributes directly to some specific product or process that they can then exchange with other organizations for vital resources.

Since one important goal of such organizations is to advance knowledge, and since scientific disciplines are the

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a number of points in Kornhauser's Scientists in Industry, as well as in, David G. Moore and Richard Reuch "The Professional Employee in Industry," Journal of Business of the University of Chicago, Vol. 28, No. 1, (Jan. 1955) pp. 58-66; Paula Brown, "Bureaucracy in a Government Laboratory," Social Forces, LII, 1954, pp. 259-268; Herbert A. Shepard, "Nine Dillemas in Industrial Research," Administrative Science Quarterly, Vol. 1, No. 3, (Dec. 1956), pp. 295-309; David Solomon, "Professional Persons in Bureaucratic Organizations," Symposium on Preventive and Social Psychiatry, (Washington D.C., Walter Reed Army Institute of Research, April 1957), pp. 253-266; Peter M. Blau and W. Richard Scott, Formal Organizations: A Comparative Approach, (San Francisco: Chandler Publishing Co., 1962), pp. 60-63.

only qualified judges of whether or not valid contributions to knowledge have been made, the discipline tends to have considerable control over the ends toward which the research is directed. Furthermore, since a contribution to knowledge fulfills the discipline's goals as well as those of the work organization, a scientist can exchange such information for disciplinary recognition <u>and</u> for organizational rewards. Thus, not only does the discipline tend to direct the research conducted in such organizations toward its own ends, it utilizes the organization's reward system in doing so!

On the other hand, those organizations that depend on scientific research for products or services which can be exchanged for profit or necessary resources, must exercise greater control over the scientists they employ. To begin with, an organization must coordinate and integrate many more separate and varied functions in order to produce specific products or services than to produce "contributions to knowledge". Furthermore, since the organization maintains its viability by virtue of its productive or service output, each phase of its operation must justify its contribution to the attainment of organizational ends; cost, efficiency, and relevance to organizational goals determine whether proposed research will be undertaken or continued. Channeling all functions toward organizational ends may also indirectly limit opportunities to acquire new information by restricting opportunities to follow up

promising leads that may emerge unexpectedly in the course of normal research. The specific managerial techniques for controlling research need not concern us here.[6] The important point is that since the organization cannot afford the expense of maintaining costly researchers whose information does not contribute to organizational viability, they literally cannot afford to support research that is controlled by the scientist's discipline.[7]

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[6]Industrial organizations, for example, control research toward their own ends by retaining control over project approval and termination, by emphasizing the relation between company goals and research, and by bringing scientists into contact with product users inside and outside the organization. Kornhauser, Scientists in Industry, pp. 56-73; for a detailed description of ways in which management controls research in the central research laboratory of a large electronics company, see Marcson, The Scientist in American Industry: Some Organizational Determinants of Manpower Utilization, (Princeton, New Jersey: Industrial Relations Section, Department of Economics, Princeton, University, 1960), pp. 36-50.

[7]Some industrial work organizations can afford to support a few researchers whose work is less directly relevant to attaining specific organizational goals or which cannot be immediately exploited, but these ordinarily are confined to those organizations whose broad technological base permits them to capitalize on a wider range of possible discoveries.

> A firm with a narrow technological base is likely to find research profitable only at the applied end of the spectrum, where research can be directed toward solution of problems facing the firm, and where the research results can be quickly and easily translated into patentable products and processes. . . . On the other hand, a firm producing a wide range of products resting on a broad technological base may well find it profitable to support research toward the basic end of the spectrum. A broad technological base insures that, whatever direction the path of research may take, the results are likely to be of value to the sponsoring firm.

Unlike universities and other organizations with broad general goals, every contribution to the discipline's store of knowledge will not also be exchangeable for rewards within the organization. And because of extensive organizational control over research, information that is exchangeable within the organization will probably have less value in the discipline than information produced under disciplinary controls. If research is directed toward narrow organizational goals, then new information will very likely be exchangeable for rewards in the organization, but not necessarily in the discipline.

The relationship between the specificity of an organization's goals and the extent of organizational (other than disciplinary) control over the work process carries over to individual roles in the scientific division of labor within an organization. An organization must focus its resources and energy in order to produce any product or process that fulfills specific requirements: it must institute controls and restrictions to insure that the contribution of each role is coordinated, functionally integrated, and directed toward these specific ends. Each role must be responsive to various specific needs of the

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Richard R. Nelson, "The Simple Economics of Basic Scientific Research," Journal of Political Economy, XLVII (1959) p. 302, quoted in Kornhauser, Scientists in Industry, p. 23.

organization and to the total process to which it contributes. And, generally speaking, the closer a role is to the end point of production, the more thoroughly is it so determined and bounded by organizational requirements: discretion is increasingly narrowed by decisions of those with greater authority; and rules are increasingly specific because of the necessity of meshing each activity with a growing number of organizational functions and processes.

Types of research roles also reflect the degree of disciplinary or organizational control over their activities. "Research" is the generic term for an array of activities designed to produce new scientific knowledge. When it is directed toward solving problems valued in the discipline, to increasing the store of knowledge, it is commonly referred to as "basic", "pure", "fundamental", or "exploratory". Basic research need not fulfill any <u>specific</u> organizational requirements, it need only contribute to disciplinary knowledge. Consequently, the discipline, rather than the organization, exercises control over the selection of research problems. If research is directed toward making a discovery which will ultimately have a specific practical application, it is often referred to as "applied" research. Those in applied research must supply information that can be translated into a practical or useable form with resources available to their work organizations. Consequently, organizational rather than disciplinary criteria will predominate in selecting research problems. And finally, research that is directed toward supplying information to implement or adapt scientific knowledge to a particular service or to productive processes is often termed "developmental research," or "methodology". Hence developmental research problems will be largely predetermined by the organization's specific applied objectives and available facilities.

Thus basic, applied, and developmental research roles can be thought to form a continuum from the least to the most organizationally bounded or structured, from most to least subject to disciplinary control in selecting research problems, and, hence, also from most to least likely to produce information of value to a scientific discipline. This interpretation of the relation between organizational control and role function in the division of scientific labor has considerable empirical support. As organizational control over a role increases there is less need or opportunity for discretion and autonomy in that role, and where organizational control is least, demand for individual discretion and freedom should be greatest. Hence scientists who are best equipped to exercise discretion--the most highly trained--should occupy roles where organizational control is least, and scientists least capable of discretion--the less thoroughly educated--should occupy the most organizationally dominated roles. Data from the 1962 National Register of Scientific and Technical manpower Personnel, in Table III confirm this relationship. And a study of

TABLE III.1

RELATION BETWEEN LEVEL OF TRAINING AND SCIENTIFIC ROLE[8]

Role Function	Level of Training: Ph.D. or professional medical	Master's	Bachelor's	Less than Bachelor's	No Report	
Basic Research	58.2	21.8	19.0	.5	.5	100.0
Applied Research	34.9	31.7	31.5	1.0	.9	100.0
Development or Design	8.3	28.3	60.1	2.4	.1	100.0
Production and Inspection	4.4	20.0	71.3	2.3	2.0	100.0
Management or Administration of R and D	37.3	22.7	37.2	1.8	1.1	100.0

[8]Adapted from, Scientific and Technical Manpower Resources: Summary Information on Employment, Characteristics, Supply, and Training, National Science Foundation, NSF 64-28, Table IV-25, p. 97.

552 scientists and engineers, about 10 percent of whom were in academic, 50 percent in industrial, and 40 percent in governmental settings provides further support. Influence over "the person who had most weight in deciding his technical goals, and . . . over the person who had most weight in decisions about funds" was greatest for Ph.D.s in research labs, next greatest for Ph.D.s in development labs, followed by non-Ph.D.s in research labs with few Ph.D.s, non-Ph.D.s in development labs with many Ph.D.s, and least for non-Ph.D.s in labs with many Ph.D.s. These groups also differed in the same order on the importance of inner sources of motivation over the importance of supervisors as a source of motivation.[9]

Thus with respect to the impact of an organization's research ends on disciplinary participation, we have Hypothesis 2:

> Hypothesis 2. The more an organization directs research toward its own ends rather

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[9]Donald Pelz and Frank M. Andrews, "Organizational Atmosphere, Motivation, and Research Contribution," The American Behavioral Scientist, (December 1962), pp. 43-47. And Kornhauser reports that "a study of scientists and engineers in six major industrial firms. . . shows that scientists and engineers with Ph.D.s are much more likely to participate in determining their own work assignments. This is further indicated by the high proportion of doctorates (64 percent) in the company with the highest level of participation in work decisions, and the virtual absence of Ph.D.s (1 percent) in the company with the lowest level of participation." Kornhauser, Scientists in Industry, p. 63. The study referred to is The Conflict Between the Scientific Mind and the Management Mind (Princeton: Opinion Research Corporation, 1959).

> than toward those of the scientific discipline, the less will be the opportunities to acquire information that is exchangeable in the discipline, and hence the less will be the disciplinary participation of the scientists in that organization.

THE IMPACT OF WORK ORGANIZATIONS ON MOTIVATION FOR DISCIPLINARY PARTICIPATION

Besides offering different opportunities to acquire information valued in the discipline, work organizations may affect a scientist's desire or motivation to exchange information in the discipline. In this regard the organization's value or reward system figures importantly, but in order to see why, we must first consider the reasons individuals engage in exchanges.

Motivational Assumptions of Exchange Theory

I will assume that scientists engage in exchanges in order to receive commodities that are valuable to them. Why they are valuable is not important for the moment; it only matters that they want them sufficiently to be willing to supply a donor with some commodity that he values. But exchanges include costly as well as rewarding elements. It is hardly necessary, for example, to point out that it is unpleasant not to receive anticipated rewards. And it is certainly obvious that many experiences are simply unpleasant or painful in themselves for physiological or psychological reasons.

Still others may be unpleasant or costly solely because the culture defines them to be, just as, for the same reasons, many other experiences are pleasant and rewarding. Perhaps it is less commonly recognized that actors incur costs when they are involved in choosing from among alternative exchanges. According to Homans,

> The cost. . . of a unit of a given activity is the value of the reward obtainable through a unit of an alternative activity, foregone in emitting the given one.[10]

And Festinger observes that,

> All those elements, that, considered alone, would lead to action other than the one taken are disonant with the cognitive elements corresponding to the action taken.[11]

Neither author regards the costs of foregone alternatives as permanent though. For Homans they are no longer costly when they cease being viable alternatives and this may happen once one embarks on the chosen behavior;[12] and Festinger proposes that individuals will attempt, by some means, to reduce their dissonance,[13] a point to which we shall shortly turn.

Since exchanges have both rewarding and costly elements, the value, or the anticipated value, of an exchange equals

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[10] Pelz, "Organizational Atmosphere. . .," p. 60.

[11] Leon Festinger, A Theory of Cognitive Dissonance, (Stanford: Stanford University Press, 1957), p. 36 and passim.

[12] Homans, Social Behavior, p. 59.

[13] Festinger, A Theory. . ., pp. 36-47.

the rewards less the costs. Homans refers to this as "profit," and though he explicitly limits costs to foregone activities,[14] I will include <u>all</u> sources of costly or punishing experiences: those from foregone rewards, from rewards withheld, and from any physically or psychologically punishing experiences encountered in carrying out an exchange. I will use profit and value interchangeably to refer to reward less cost while also continuing to use value to mean rewarding experiences only; when it makes a difference, the context will indicate which is the intended sense.

Let us take a closer look at the relation between rewards and behavior. There are no more concise statements on this than Homans' propositions 2 and 3:

> (2) The more often within a given period of time a man's activity rewards the activity of another, the more often the other will emit the activity.
>
> (3) The more valuable to a man a unit of the activity another gives him, the more often he will emit activity rewarded by the activity of the other.[15]

Homans further elaborates these propositions:

> From positions 2 and 3 it follows that the frequency of interaction between Person and Other depends on the frequency with which each rewards the activity of the other and on the value to each of the activity he receives.[16]

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[14] Homans, <u>Social Behavior</u>, pp. 61-64.

[15] <u>Ibid</u>., pp. 54-55.

[16] <u>Ibid</u>., p. 54.

I have referred to those exchange relationships in which exchange behavior can vary as relational variables; Homans' propositions directly links such variation to the frequency of rewarding behavior received from another, or to the value of the rewards obtainable in the exchange; which is another way of stating that individuals attempt to maximize their profits by some strategy of balancing off costs and rewards.

But often, of course, individuals have more than one exchange relationship open to them. On what basis do they decide to participate in one rather than another? The answer is implicit in Homans' two preceding propositions and is clearly stated by Blau.

> The only assumption made is that human beings choose between alternative potential associates or courses of action by evaluating the experiences or expected experiences with each in terms of a preference ranking and then selecting the best alternative.[17]

Thus, individuals allocate their behavior among possible alternative exchanges according to some preferential rank

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[17]Peter M. Blau, Exchange and Power in Social Life, (New York: John Wiley and Sons, Inc., 1964), p. 18. Blau goes on to comment that,

> What is explicitly *not* assumed here is that men have complete information, that they have no social commitments restricting their alternatives, that their preferences are entirely consistent or remain constant, or that they pursue one specific ultimate goal to the exclusion of all others. (p. 18)

ordering; an ordering made, presumably, on their relative value; that is, on the anticipated profit of an exchange relative to the profit obtainable in any other exchange. I assume further that in making a choice between alternatives an actor takes into account the likelihood of successfully achieving any particular level of reward, so that individuals will sometimes choose less rewarding but more certain exchanges over more valuable but riskier alternatives.

How do these assumptions concerning motivation for engaging in an exchange bear on the effect that work organizations have on motivation for participating in a scientific discipline? That is, what work organizational characteristics will alter either the relative value, or the absolute value of disciplinary exchanges? I will consider three organizational characteristics that might alter the relative value of disciplinary exchanges; two are tied to the organization's reward system, and the third to the already mentioned opportunities to acquire exchangeable information.

Motivation and the Organizational Reward Systems

The first organizational characteristic that might alter the relative value of disciplinary exchanges is the extent to which the goals of the work organization are similar to those of the discipline. When they are similar, as we have seen, a scientist contributes to organizational goals to the

extent that he produces information that is exchangeable in the discipline. For example, in most universities publications are exchangeable for organizational rewards, since the fact that it is a publication--and therefore a form of disciplinary recognition--constitutes certification by the discipline that organizational goals have also been met. Since a contribution to scientific knowledge is valuable to the work organization and to the discipline, a scientist can exchange it for organizational as well as disciplinary rewards.[18] In organizations of this type, therefore, the value of a disciplinary exchange for a scientist consists of the rewards obtainable from two sources, from the work organization and from the discipline. On the other hand, in organizations whose goals are not similar to disciplinary goals, information that is exchangeable in the discipline may not in general also be exchangeable for organizational

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[18] The fact that two structures such as scientific work organizations and disciplines share similar goals, in the sense that each will exchange rewards for the same commodity, leads to some interesting possibilities. For when commodities that are valued in one structure are also valued in another, the two may compete for them. Secrecy in science provides just such a situation. Work organizations, for various economic or strategic reasons, may retain control over the information their scientists produce, thus preventing it from being exchanged in the discipline for recognition and social approval. It is precisely when work organizations have produced information that is highly valued in the discipline that secrecy becomes, from the standpoint of the discipline, most insidious. Those organizations, such as universities and some government agencies that share the general disciplinary goals of advancing knowledge, however, can advance their ends only by having the information accepted by the discipline.

rewards; the fact that it contributes to disciplinary goals does not mean that it automatically contributes to more specific organizational goals. In such organizations, therefore, the _value_ that a disciplinary exchange has for a scientist will tend in general to be _relatively less_ than in a goal-compatible organization, since it will consist of rewards from only _one_ source, the discipline.

Organizations are likely to differ in how similar their goals are to a scientific disciplines' and, consequently, in the extent to which they _reward_ disciplinary participation. In fact, a good indication of the extent to which an organization's goals are similar to a scientific discipline's should be the extent to which it rewards disciplinary participation. Work organizations then, since they differ in rewarding disciplinary participation, will differ in their contributions to the _value_ that a _disciplinary_ exchange will have for a scientist, and hence they will differ in their effect on his motivation for disciplinary participation. The relationship between this aspect of the organization's reward system and disciplinary participation is stated in Hypothesis 3:

> _Hypothesis 3_. The less an organization rewards disciplinary participation, the less will be the motivation of its incumbents to participate in the discipline, and, hence, the less will be their disciplinary participation.

The second characteristic of the organizational reward system that can affect the relative value of participating in the discipline is whether the organization provides

<ins>alternative</ins> roles that are relatively more rewarding than those that permit them to acquire information exchangeable in the discipline. Such a system might have two consequences. First, to the extent that these roles <ins>are</ins> perceived as distinct alternatives to disciplinary exchanges, they make the alternatives of disciplinary exchanges <ins>relatively</ins> less rewarding by definition, and, therefore, can be expected to reduce disciplinary participation. A second less direct effect might be to encourage scientists to acquire commodities that could be exchanged in the organization and thereby reduce their effort to acquire commodities that would be exchangeable in the discipline. For example, scientists might begin to increase exchanges in organizational role sectors, such as teaching or administration and decrease their participation in role sectors in which they were more likely to acquire information of value to the discipline. Or these more rewarding alternatives might induce scientists to leave a role sector in which information could be acquired for one where, either because of inadequate facilities or because research problems were not being selected according to disciplinary criteria, there were fewer opportunities for acquiring informations valued in the discipline.

Hypothesis 4 expresses the relationship between alternative organizational role sectors and disciplinary participation:

> <ins>Hypothesis 4</ins>. The more an organization offers alternative organizational role-sectors that

> are more rewarding than those in which they might acquire exchangeable information, the less will be the motivation of its incumbents to participate in the discipline, and, hence, the less will be their disciplinary participation.

Motivation and Opportunities to Acquire Exchangeable Information

Another factor which may affect a scientist's decision to participate in an exchange relationship has already been touched upon, his estimate of the likelihood of successfully achieving a given level of reward. Level of aspiration theory assumes that individuals set goals for their behavior. The goals need not correspond to what they *ideally* would like to achieve, rather they are based upon their estimates of the probability of success colored by present circumstances and past experience.[19] These "levels of aspiration" are what the actor tries to achieve. A great deal of research experience with level of aspiration phenomena lead to the conclusion that:

> . . . generally the level of aspiration will be raised and lowered respectively as the performance (attainment) reaches or does not reach the level of aspiration.[20]

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[19]Kurt Lewin, Tamara Dembo, Leon Festinger, and Pauline S. Sears, "Level of Aspiration" in J. McV. Hunt (ed.), *Personality and the Behavior Disorders* (New York: Ronald Press, 1944), Vol. I, 333-378.

[20]*Ibid*., p. 337.

It is reasonable to suppose that decisions to participate in exchange relationships are based on similar mechanisms: actors aspire to achieve a certain return from the exchange, and their aspirations are raised and lowered with success or failure. However, we can also assume that the level of aspiration will change not only with objective evidence of past performance, but also with other kinds of information on the probability of success. Thus faced with objective evidence that the particular role he occupies may not provide adequate opportunities to acquire information of value to the discipline, a scientist may decide not to try for disciplinary rewards. This would be particularly true if his work organization offered rewards that were well within his ability to achieve. Hence, here too, organizational characteristics which we have already considered come into play. Where work organizations permit little disciplinary control over the ends of scientists' research, or where research facilities are inadequate, there would be little hope of achieving disciplinary rewards; under these circumstances aspirations should tend to decline. And if the organization did offer accessible organizational rewards for producing organizationally valued information or for engaging in other organizational role sectors, then his aspirations could shift to these. Hence Hypothesis 5:

> Hypothesis 5. The less adequate are the facilities for research in an organization, or the more it directs research toward its

> own ends rather than to those of the discipline, the less will be the motivation of its incumbents to participate in the discipline, and, hence, the less will be their disciplinary participation.

<u>Other Factors Affecting Motivation</u>

The foregoing discussion of organizational characteristics that affect a scientist's motivation to participate in his discipline has considered the <u>value</u> of disciplinary exchanges <u>relative to possible alternative exchanges</u>. It has assumed that the relative value of disciplinary exchanges is a function of the potential rewards available in alternative exchanges and the perceived possibilities for achieving them; that is, that differences in the relative value of a disciplinary exchange are a function of objective work organizational conditions: how much they reward disciplinary participation, the presence of rewarding role alternatives, and the objective possibilities for acquiring exchangeable information. The discussion has implicitly assumed that the <u>absolute</u> value of a disciplinary exchange, i.e., its value irrespective of the value of other exchanges, remained constant. But a scientist's motivation to participate in the discipline would also change if the absolute value he places on disciplinary exchanges should change. For example, it is at least conceivable that a scientist might simply come to want disciplinary recognition more or less over time independently of changes in the work organization that affect the value of alternatives. How likely is this possibility, and what process might affect the absolute value of an exchange? There are at least two theoretical possibilities.

Socialization

In most recent thinking, socialization refers to the process whereby individuals learn social roles and the necessary prerequisites for performing them, including, values, attitudes, interests, skills, knowledge, habits, and beliefs.[21] And most authors would probably subscribe to the view that socialization refers, if not to the necessity, at least to the possibility of some motivational changes; that is, in the present terminology, to changes in the value of potential exchanges. For as Brim has pointed out, to perform a role an individual must not only possess certain necessary skills and the knowledge of the behavior and values

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[21]William Sewall, "Some Recent Developments in Socialization Theory and Research," The Annals of the American Academy of Political and Social Science, Vol. 349, (September 1963), 163; Orville G. Brim, Jr., "Socialization Through the Life Cycle," in Orville G. Brim, Jr. and Stanton Wheeler, Socialization After Childhood: Two Essays (New York: John Wiley and Sons, Inc., 1966); and Robert K. Merton, George G. Reader, and Patricia L. Kendall (eds.), The Student Physician: Introductory Studies in the Sociology of Medical Education (Cambridge, Massachusetts: Harvard University Press, 1957), p. 287.

expected of him, he "must desire to practice the behavior and pursue the appropriate ends."[22]

Under what conditions are individuals socialized? Probably because it is such a general concept, referring to a wide range of phenomena and processes, there appears to be no single, generally accepted, theory of socialization. According to Merton et. al., "socialization takes place primarily through social interaction with people who are significant for the individual. . ."[23] But significant in what sense? Brim's analysis of socialization provides an answer that ties directly into the present theoretical framework.

> The individual, because of his previously acquired desire to conform to others' expectations, is motivated to live up to these standards, and his sense of well being or satisfaction depends on such conformity. The self-other relationship leads to an individual's appraisal of himself as being good or bad, according to the degree to which he lives up to another's expectations. The importance of self-appraisal to the individual varies according to the significance of the other person's evaluation of him, which, in turn, is based, in the last analysis, on the degree to which the other controls (or once controlled) rewards and punishments.[24]

Thus, significant individuals are those who are in a position to reward and punish and, thereby, to influence self-evaluations

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[22]Brim, "Socialization. . .," p. 25, my emphasis.

[23]Merton, et al., The Student Physician, p. 287.

[24]Brim, "Socialization. . .," pp. 15-16.

and esteem. In this view then, actors tend to acquire values that are similar to those who are in positions to reward and punish them, partly because these rewards and punishments become linked through their role to their self esteem. Presumably, responses from others may be initially rewarding or costly for reasons that have nothing to do with self esteem; but because rewards are bestowed when one meets another's expectations and are withheld when he does not, they also come to signify how well one is performing a role and reflect upon the kind of person one is.[25]

Thus individuals can be socialized simply by participating in organizations that are capable of rewarding or punishing them. But we are interested in socialization that increases or decreases a scientists' <u>motivation</u> to participate in a scientific discipline. The organizational characteristics that affect the <u>absolute value</u> of disciplinary exchanges through socialization by rewarding or punishing therefore are the same ones that affect their relative value through

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[25]Zetterberg advances a very similar argument. Based upon the assumption that individuals "desire to maintain and to maximize favorable self-evaluations" and from other definitions he derives his "Postulate of Evaluative Compliance:" which predicts changes in individual valuations:

> In an act or system any actor has a tendency to develop attitudes that are synonymous with uniform evaluations (attitudes and/or social values) in the system.

Hans L. Zetterberg, "Compliant Actions," <u>Acta Sociologica</u>, Vol. 2, 1957, 184-186.

the organizations' reward system: the extent to which the work organization rewards a scientist for disciplinary exchanges, the extent to which it provides alternative rewarding roles or role sectors.

Cognitive Dissonance

Cognitive dissonance or balance theory also predicts that under certain circumstances scientists would change the absolute value they placed on disciplinary participation. I have already pointed out that choosing from among alternatives involves costs to the actor in the form of rewarding alternatives foregone. Festinger suggests that "the most direct and probably most usual manner of reducing [such] post decision dissonance" would be to change cognitions about the alternatives in such a way that one changes "the attractiveness of the alternatives involved in the choice."[26] We should expect greater dissonance and hence greater devaluation of disciplinary exchanges the more a scientist's organizational role departed from one that would permit disciplinary participation--that is, the further they were from actually achieving disciplinary rewards. Hence, where their research facilities were inadequate or where their research was directed toward organizational ends, scientists might resolve the dissonance of foregone disciplinary rewards by devaluing them. And where the organization valued information that was not

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[26]Festinger, A Theory. . ., pp. 44, 37.

valued in the discipline, or offered rewarding alternatives in roles with slight opportunity to acquire information that would be exchangeable in the discipline, scientists might be tempted to commit themselves to achieving organizational rewards and again reduce possible dissonance by devaluing disciplinary participation.[27]

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[27]One final process, marginal utility, also bears upon changes in the absolute value of an exchange, though it does not directly tie in with organizational characteristics. The principle of marginal utility suggests that, as Homans puts it in his proposition 4:

> The more often a man has in the recent past received a rewarding activity from another, the less valuable any further unit of that activity becomes to him.
> Homans, Social Behavior. . ., pp. 55.

For example, we would expect the relative value of participating in the discipline to decline when an actor has received "enough" recognition, and to increase when he becomes satiated with organizational incentives. Blau has pointed out that marginal utility applies to a constant level of expectation but that achieving some rewards, social approval for example, increases the level of aspiration and:

> this rise in expectations. . . makes the attainment of sufficient rewards to meet the new expectation level more significant than attaining that amount of reward was before. This has an effect opposite to that of the principle of diminishing marginal utility and mitigates its influence.
> Blau, Exchange. . ., p. 149.

It may be that marginal utility applies chiefly to the short-run, as Homans qualifying phrase "in the recent past" suggests, and that level of aspiration theory applies more to long-run phenomena. This is only a guess, however. At present I am not aware of any theory which attempts to disentangle the possibly confounding effects of these two processes.

CONCLUSION

We have observed thus far that work organizations may affect disciplinary participation by affecting a scientist's <u>opportunities to acquire</u> information that he could exchange in his discipline. Since any commodity, scientific information included, is produced by directing certain means or facilities toward particular ends, work organizations might reduce a scientist's opportunities to acquire information that would be exchangeable in the discipline in two ways: (1) by providing inadequate means or facilities; or (2) by directing his efforts toward nondisciplinary (usually organizational) goals, that is, by not permitting the discipline to exercise control over the selection of research problems. With respect to the second point, basic research, because it is directed toward disciplinary goals, should be more likely to produce information of genuine value to other scientists in the discipline than either applied or developmental research, which function to supply information for rather specific organizational interests.

We have also observed that organizations can affect a scientist's participation in his discipline by affecting his <u>motivation</u> to do so. I assumed that individuals participate in exchanges in order to receive some valuable response from others, and that they choose between alternative exchanges according to the relative value that each alternative has for them and according to their estimation of the chances of achieving each alternative.

Under these motivational assumptions, I argued that two general sets of organizational factors would be likely to affect a scientist's motivation to participate in disciplinary exchanges. The first of these were those elements of the organizational reward system that affect the value of disciplinary exchanges either relative to other alternative exchanges, or absolutely. In this connection I discussed the similarity between disciplinary and organizational goals: the extent to which a scientist could exchange information for organizational as well as disciplinary rewards. I argued that where organizational and disciplinary goals were similar, scientists should be motivated to participate in the discipline since by participating they could obtain both disciplinary and organizational rewards. But where the values were dissimilar--where the organization's rewards could not be acquired for the same information that would be exchangeable for disciplinary rewards--then the disciplinary rewards would be relatively less valuable. Another element of an organizational reward system that might affect motivation is the presence or absence of alternatives to the research role that offered relatively greater organizational rewards than those available through the research role. Where such alternatives were available in an organization this should <u>decrease</u> the relative value of engaging in disciplinary exchanges.

The second set of organizational factors affecting motivation were those factors affecting a scientist's estimate of the probability that he could in fact <u>acquire</u> information that would be exchangeable in his discipline. These, as we saw, would be determined by such organizational characteristics as the adequacy of the <u>means</u> and the appropriateness of the <u>ends</u> in the research situation.

There is an obvious connection between the adequacy of organizational facilities, the extent of disciplinary control over research, and organizational exchange or value systems. One of the chief means for organizations to control the work of their incumbents is to reward only those activities that accord with their directives and expectations. We should expect, then, that ordinarily those organizations that direct research toward organizational ends would also exchange organizational rewards only for information that contributed toward those ends. Conversely, where an organization permits disciplinary control over scientific work, scientists should be able to exchange information they produce for organizational as well as for disciplinary rewards. And we would also expect that the social and material facilities for producing scientific information would be roughly adequate for the ends toward which the research was directed. Finally, we would expect that organizations in which research problems were chosen by disciplinary rather than organizational criteria would provide fewer alternative exchanges (e.g., non-research or non-basic research organizational role sectors) than those which directed research toward organizational ends, the reason being that producing specific products or services for exchange with some social unit for profit or support requires individuals to fill roles in management, development, control, marketing and sales.

In the last chapter I suggested that responses from colleagues--approval, recognition, or lack of either--would figure importantly in a scientists' appraisal of himself as a scientist, and that this should give heightened significance to disciplinary exchanges. This fact also suggests why a scientist's disciplinary participation might decline more than we might reasonably expect from any lack of exchangeable information, or from an unfavorable reward system. For if a scientist could not fulfill his role as scientist with respect to the important obligation of supplying information (in any of its many forms), then confrontations with colleagues would also be occasions in which he received cues that would tend to disconfirm his view of himself as a scientist and lower his estimates of the adequacy of his role performance and of this particular self-stance. Lowering self-esteem in these encounters should lead a scientist to avoid disciplinary participation in which such cues are received.

There is still one more matter that this discussion of the relationship between work setting and disciplinary participation has not considered: the possible effect of selective recruitment and of mobility within and between particular kinds of work organizations. For the sake of simplicity we have been speaking as though work organizations increased or decreased disciplinary participation irrespective of other possibilities. Actually, of course, there are other ways in which a given relationship between work organizational characteristics and disciplinary participation may come about besides direct changes in the behavior or motivation of organizational incumbents; scientists with one type of disciplinary participation may simply move to particular types of organizations either initially or later in their careers. In fact, just those organizational characteristics which may change a scientists' disciplinary participation--the adequacy of facilities, the extent of disciplinary or organizational control over research, and the organizational reward system--could be expected also to influence a scientists' choice of jobs and the recruitment policies of work organizations. Chapter V will attempt to distinguish between the effects of recruitment and selective mobility to and from particular types of organizational settings on the one hand, and organizationally induced modifications in disciplinary behavior on the other.

Hypotheses 1-5 will be considered in the next chapter, and a sixth hypothesis dealing with changes in motivation will be introduced and tested in Chapter V.

CHAPTER IV

ORGANIZATIONAL CHARACTERISTICS AND DISCIPLINARY PARTICIPATION

Research Design and Sample Description

Data for this study were drawn from questionnaires mailed to every fourth individual on the official membership list of the American Society for Microbiology (ASM), the principal scientific society for microbiologists. The ASM was chosen because its members were known to engage in every form of scientifically related work in settings that represent the full range of work organizations characteristic of American science.

The sample was restricted to the members of one scientific society in order to have a large number of individuals representing a cross section of what is probably a fairly representative scientific discipline, and in order to control for possible variations by discipline.

The original mailing was followed by a postcard reminder to nonrespondents and later with another questionnaire. Of the 1,971 questionnaires originally mailed, 1,511, or 76.6 percent, were returned. For a variety of reasons, however, the percentage of questionnaires used in the analysis was less than this figure. Eighty-three of the returned questionnaires were either blank or inadequately completed, 4 had been sent to deceased members, 10 were returned because of inadequate addresses or because an individual was temporarily out of the country, 4 were not permitted to fill out the questionnaire for security reasons, and 10 were received after the coding deadline. In addition to these, 14 unemployed, 12 retired, and 168 students were excluded because they did not meet the requirement of being employed full time in a work organization. Consequently, 1,206 or 61.2 percent of the questionnaires originally mailed out were coded. Of this number an additional 221 respondents were excluded because they were not presently doing research. The final effective sample, then, consisted of 985 microbiologists, members of the ASM, who were employed full time, and whose jobs involved performing some research.

Limitations of time and money prevented sampling non-respondents to determine if they differed in important ways from the respondents. There appear to be no norms to which the sample could be compared to determine if the respondents are typical of the ASM membership.

It is true, of course, that nothing at all is known of those microbiologists who might <u>not</u> belong to the ASM. Since they exhibit a singular lack of involvement in the discipline, they would be of considerable interest in a study such as this. The number of these individuals is probably not large, however, if for no other reason than the fact that receiving the Society's journals and announcements of meetings is contingent upon membership.

The questionnaire was developed from earlier versions administered to microbiologists in three typical work organizations: to selected members of a state public health laboratory in a situation which permitted them to comment on the questionnaire; and anonymously, to microbiologists in a large pharmaceutical company engaged in basic and applied research, and to the members of a department of microbiology in a large state university. In addition, detailed written suggestions were received from three highly knowledgeable members of the ASM. The necessary modifications that these pretests made apparent, were incorporated into the final mailed version which appears in Appendix A.

Indexes of Disciplinary Participation

Work organizations and scientific disciplines have been defined in terms of certain characteristic exchanges of valued commodities between positions. The commodities that comprise exchange relationships in the discipline consist largely of information--an original contribution to knowledge, advice, assistance, ideas--in return for social approval, recognition, support, information, and, as symbolic overlay to these responses, some confirmation or disconfirmation of adequate self-role performance.

Many situations might provide occasions in which exchanges take place, and we could only hope to examine some of the more important of these. Consequently, five types of participation, both formal and informal, were used to study the relationship between organizational characteristics and the extent of disciplinary participation.

Attendance at National Meetings

National meetings of a scientific discipline provide an arena within which many kinds of exchanges may transpire, from formal presentation of papers and special sessions and discussions, to informal corredor chats that may convey anything from significant information regarding existing or planned research to gossip. Limitations on the length of the questionnaire prevented determining what commodities were exchanged or how much valued response was received. We reasoned that attendance itself would provide sufficient opportunities to engage in exchanges of some kind if a scientist had information that others in his discipline would find valuable and if he was motivated to do so. Consequently the scientists were asked to indicate the number of times they attended the national meetings of their disciplines for the preceding three years. Those who had attended two or three meetings were regarded as having "high" participation and those who had attended only once or not at all were regarded as having "low" participation.

Attendance at Conferences or Symposia.

Scientific conferences and symposia provide another important setting for disciplinary exchanges, and may be because of the specialized purposes for which they are held, possibly even more likely than national meetings to lead to exchanges of valued information. The scientists were asked to indicate how many scientific conferences or symposia they had attended in the last twelve months. On the basis of the distribution of scores, those attending two or more were regarded as having "high" participation while those attending none or one were classified as having "low" participation.

Contacts with Other Scientists.

The next two indexes focus more directly than the preceding upon a specific exchangeable commodity--information about one's work. And because exchange theory suggests that scientists who seek out others for information might be different from those who are sought out, both of these possibilities were taken into account in the following two questions.

About how often do you contact other scientists
from OUTSIDE your organization or institution

specifically for help, advice, or information on matters bearing on your work?

About how often do scientists from OUTSIDE your organization or institution contact you specifically for help, advice, or information on matters bearing on your work?

On both questions the scientists could indicate one of five alternatives: once a week or more, two or three times a month, once a month, less than once a month, and never. Those indicating that they either contacted others or were contacted by others once a month or more often were classified as having "high" participation, those who contacted or who were contacted by others less than once a month, or never, were classified as having "low" participation on this form of participation.

Papers.

The fifth and final type of disciplinary participation, the number of professional papers published, has undoubtedly been studied more than all other forms of participation combined. In part this is because publishing is probably the most prestigious and widely recognized way of presenting new information for disciplinary consumption. In addition, such an exchange, unlike many others, leaves its own permanent record and is easily quantified.

Measures of professional publishing more highly refined than a simple frequency are possible, of course. For example, it would be helpful to have some measure of the importance or significance that a published paper has in the eyes of those in the discipline, i.e., its value to them; and for some limited set of papers this could theoretically be accomplished using judges. But with a large sample, and many papers, this was not feasible. Consequently, we settled for this alternative: each scientist indicated the number of papers he had published in professional journals in the last three years. Because, as we shall see in the next chapter, scientific productivity is known to vary with age, and because those who had their degrees less than three years would have had fewer opportunities for publishing, "high" participation was defined as publishing as many as, or more than, the most prolific 50 percent of all scientists who had received their highest academic degrees in the same year.[1] In this way we hoped to introduce some control over any variations in productivity by professional age.[2]

Organizational Characteristics and Participation

Hypothesis 1 proposed that:

> The less adequate are the facilities or means for research in an organization, the less will

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[1]No median productivity level was computed if there were fewer than 20 scientists in any professional age cohort. In those cases where there were fewer than 20, scientists in successive age groups were added until 20 or more cases were reached.

[2]Most studies of the relationship between productivity and age deal with biological age. Probably of much greater sociological significance, however, is what I refer to as "professional age", the length of time since a scientist has earned his highest professional degree.

> be the opportunities to acquire information that is exchangeable in the discipline, and hence the less will be the disciplinary participation of scientists in that organization.

In Chapter III we observed that the facilities or means by which scientific information was produced consist of both social organization and material equipment and resources. This dissertation is not primarily concerned with those combinations of specific social or material means that lead to high output of scientific ideas, though this is manifestly an important problem and one which is receiving considerable attention.[3] These conditions of organizational effectiveness, when finally understood, are quite likely to be extremely complex and to vary with the research problem. In order to circumvent this, this organizational characteristic was measured generally by having the scientists themselves rate the adequacy of their research facilities. By this means it was hoped to have a measure which, though not dealing with specifics, would compensate by cutting across many different work settings and research activities and establish a common base line in terms of felt adequacy or deprivation regardless of the particular nature of the resources. The adequacy, or rather the <u>felt</u> adequacy of organizational research facilities was measured by asking:

> How would you rate the research facilities and equipment where you now work?
>
> ( ) Among the best anywhere
> ( ) Very good

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[3]See for example: Donald C. Pelz and Frank M. Andrews, <u>Scientists in Organizations</u>, (New York: John Wiley and Sons, Inc., 1966).

( ) Good
( ) About average
( ) Somewhat inadequate
( ) Very inadequate

Responses were collapsed into two groups, "adequate" consisting of the first three alternatives and "inadequate" consisting of the remaining three.

Table IV.1 presents data relevant to the first hypothesis for each of the five kinds of disciplinary participation. Although the data are in the hypothesized direction for each type of disciplinary participation, they are statistically significant in only two instances, and approach significance in a third. Scientists in organizations with <u>adequate</u> research facilities, as perceived by the scientists themselves, appear to attend national meetings, to be contacted by others about their work, and to publish more frequently than those whose facilities are inadequate. Inadequate facilities by themselves, apparently have little or no direct affect on the number of symposia a scientist attends or on the frequency with which a scientist contacts others about his work.

Though there is some relation between adequate facilities and some forms of disciplinary participation as the theory predicts, it should be pointed out that because we have no independent measure of how much exchangeable information the scientists actually have, there is no way to directly verify the <u>complete</u> causal chain from inadequate facilities, to little exchangeable information, to low disciplinary participation.

TABLE IV.1

PERCENT OF SCIENTISTS WITH "HIGH" DISCIPLINARY PARTICIPATION BY ADEQUACY OF RESEARCH FACILITIES

Type of Disciplinary Participation	Adequacy of Research Facilities					
	Adequate	(N)	Inadequate	(N)	Chi Square	p
Attended 2 or 3 National Meetings in Last 3 Years	51.7	(722)	45.6	(263)	2.56	n.s.
Attended 2 or More Conferences or Symposia in Last 3 Years	73.8	(722)	71.5	(263)	.41	n.s.
Contacted Others Once a Month or More	40.0	(722)	39.2	(263)	.03	n.s.
Contacted by Others Once a Month or More	42.0	(722)	35.4	(263)	3.18	$<.10$
Published Often	56.8	(718)	46.0	(261)	8.51	$<.01$

d.f.=1

Hypothesis 2 proposed that:

> The more an organization directs research toward its own ends rather than toward those of the scientific discipline, the less will be the opportunities to acquire information that is exchangeable in the discipline, and hence the less will be the disciplinary participation of scientists in that organization.

We argued in Chapter III that the production of any commodity demanded that certain social and material means would be directed toward particular ends. We reasoned that scientists were more likely to produce the commodity with which we are here concerned--information that is exchangeable within the discipline--when research is directed toward solving problems that the discipline defines as important, than when it is directed toward research designed to produce information of interest to some organization that is attempting to market a product or to develop a process. In other words, information that will be exchangeable in the discipline is more likely to be produced <u>when the discipline exerts considerable control over the research process and particularly over the ends of research</u>. The following question was designed to determine whether disciplinary or organizational criteria guided the selection of research problems and the continuation of on-going research:

> For research in which you are involved, what is the rank importance of the following criteria as far as selecting research problems or deciding to continue on-going research is concerned?
>
> a. The chance that it may contribute to services or products provided by my employer or client.

b. The chance that it may contribute to developments or discoveries that have important practical implications.

c. The chance that it may contribute to basic knowledge in science.

We hypothesized that in those organizations where disciplinary control was greatest, scientists would indicate that criteria "c"--contributing to basic knowledge--was most important, and that in those organizations where organizational control was greatest, scientists would rank criteria "a" as most important--contributing to services or products provided by the employer or client--, and that in organizations intermediate between these two extremes of disciplinary and organizational control, scientists would rank criteria "b"--contributing to developments or discoveries with important practical applications--as most important. To simplify analysis, scientists were divided into three groups according to which of the three criteria they ranked as most influential in their research, regardless of how they ranked the other two alternatives.[4] The three groups, ranging from most to least disciplinary control, will be referred to as disciplinary (or basic), applied, and organizational research groups.

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[4]Supporting the view that these criteria formed a continuum with alternative "a" and "c" at the ends and "b" in the middle, is the fact that the responses overwhelmingly fell into that pattern. Thus of the 438 scientists who ranked alternative "c" first, 404 or 92 percent ranked all three in the expected order c-b-a, and of the 272 ranking alternative "a" first, 242 or 89 percent ranked all three in the expected order ab-c. And finally, of the 275 ranking alternative "b" first, 60 percent ranked all three b-c-a, and 40 percent b-a-c.

The relevant data for Hypothesis 2 relating the extent of disciplinary control over research to the five types of disciplinary participation are presented in Table IV.2. The data are in the hypothesized direction for only two types of disciplinary participation, the number of national meetings attended, and frequency of publishing. Scientists who engage in research directed toward disciplinary ends are somewhat more likely to have attended two or three national meetings in the last three years than scientists engaged in either applied or organizational research. But, contrary to expectations, the applied and organizational researchers do not differ in their attendance. The type of control over research is strongly associated with whether a scientist has published often, as the better than 47 percentage point difference between disciplinary and organizational researchers readily attests.

Since both the research facilities and the type of control over selecting research problems bear on opportunities to <u>acquire</u> exchangeable information, combining these characteristics in an organization should cumulativly affect participation. Table IV.3, however, indicates that the combination of these two characteristics has a somewhat more complicated effect on disciplinary participation. Consider first the combined effect of facilities and type of research on attendance at national meetings. Scientists doing disciplinary research are only slightly more likely to attend national

TABLE IV.2

PERCENT OF SCIENTISTS WITH "HIGH" DISCIPLINARY PARTICIPATION

BY TYPE OF RESEARCH

Type of Disciplinary Participation	Type of Research						Chi Square	p
	Disciplinary	(N)	Applied	(N)	Organizational	(N)		
Attended 2 or 3 National Meetings in Last 3 Years	53.9	(438)	46.9	(275)	47.1	(272)	4.63	$<.10$
Attended 2 or More Conferences or Symposia in Last 3 Years	71.9	(438)	74.9	(275)	73.5	(272)	.79	n.s.
Contacted Others Once a Month or More	39.0	(438)	38.5	(275)	42.3	(272)	.98	n.s.
Contacted by Others Once a Month or More	38.6	(438)	43.6	(275)	39.3	(272)	1.91	n.s.
Published Often	68.7	(438)	57.7	(272)	26.0	(269)	123.1	$<.001$

d.f.=2

TABLE IV.3

PERCENT OF SCIENTISTS WITH "HIGH" DISCIPLINARY PARTICIPATION, BY TYPE OF RESEARCH, AND ADEQUACY OF FACILITIES

	Type of Research					
	Disciplinary		Applied		Organizational	
	Facilities		Facilities		Facilities	
Type of Disciplinary Participation	Adequate	Inadequate	Adequate	Inadequate	Adequate	Inadequate
Attended 2 or 3 National Meetings in Last 3 Years	54.2	52.8	49.5	39.1	49.5	41.9
Attended 2 or More Conferences or Symposia in Last 3 Years	72.1	71.3	75.2	73.9	75.3	69.8
Contacted Others Once a Month or More	41.5	31.5	36.9	43.5	40.9	45.3
Contacted by Others Once a Month or More	41.8	28.7	43.2	44.9	40.9	36.0
Published Often	73.3	54.6	58.3	55.9	25.5	27.1
Base*	(330)	(108)	(206) (204)*	(69) (68)*	(186) (184)*	(86) (85)*

*The first row of numbers in parentheses indicates the base from which all percentages except "Published Often" were computed; the base N for "Published Often" is contained in the second row.

meetings if their facilities for research are adequate than if they are inadequate. For those doing applied or organizational research, however, the affect of adequate or inadequate facilities is more marked, though, of course, from the way in which the question was asked it is impossible to know whether the adequacy of research facilities <u>means</u> the same thing for such different types of research. For disciplinary or applied researchers, adequacy of research facilities has little or no effect on whether they attended two or more conferences in the preceding three years, though inadequate facilities appear to be associated somewhat with lower attendance for scientists doing organizational research.

Table IV.3 reveals a curious fact concerning the pattern of contact with scientists outside the work organization when they have inadequate research facilities: disciplinary researchers are less likely than their applied or organizational counterparts to either have much contact with, or to be contacted by, others. One possibility, which cannot be tested with the present data, is that the kinds of contacts, the actual exchange relationships, differ for the three types of research, particularly between the disciplinary researchers on the one hand, and the applied and organizational on the other. We know that their research tends to be directed toward different goals: information contributing to advancing disciplinary knowledge for the disciplinary researchers, and information contributing to a practical result or toward an organization's goals for the applied and organizational researchers. Thus the <u>kinds</u> of information sought or exchanged would be likely to be different as well: disciplinary researchers may exchange less standard information or newer information not yet widely disseminated, while those in applied or organizational research may be seeking or extending advice on more standard information or on techniques of application--on turning out a useable product or process. In short, the kinds of information exchanged, the uses to which it may be put, and the kind of individuals with whom the exchanges are made may differ depending upon the kind of research performed. There may even be complex patterns of exchange relationships in which the <u>type</u> of scientists from whom one receives information differs from those to whom information is given.

Of course this does not explain the specific patterns in Table IV.3, but it does suggest that there may be rather distinct exchange systems within the discipline. For the fact that disciplinary scientists display very different <u>patterns</u> of contact with other scientists than either their applied or organizational colleagues, points to the possibility that very different underlying processes may be involved for the two groups. For example, it appears that <u>organizational</u> researchers with inadequate facilities may be compelled to seek information from others but are relatively less likely to have others seek information from them. On the other hand, when those working under disciplinary control have inadequate facilities they tend neither to contact nor to be contacted by others; but if their facilities are adequate they are involved in giving and receiving information. It is possible that before a disciplinary researcher is in a position either to seek information from others or to be sought out by others he must have acquired a reputation, a generalized acknowledgement of competency, which informs others that exchanges with such a person will be valuable; adequate facilities in this case may simply be correlated with having acquired a reputation. Applied or organizational researchers, on the other hand, may rely more on paid consultants and less on reputation to channel their contacts with scientists outside their organizations.

Turning now to the last measure of disciplinary participation, publishing, we find, contrary to expectation, that the two characteristics affecting opportunities to acquire eschangeable information do not have a cumulative effect. Adequacy of facilities only affects publishing of disciplinary researchers; for applied and organizational researchers the effect of adequate or inadequate facilities is negligible. This may mean that what restricts the ability of the applied and especially of organizational researchers to acquire exchangeable information is the relative absence of disciplinary control over research problems, rather than any lack of necessary means or facilities. A plausible alternative interpretation

cannot altogether be ruled out, however. For if, as the preceding paragraph suggests, adequate facilities are bestowed on disciplinary researchers who have acquired a reputation, and if reputations are earned by high productivity, we would expect to find the association between adequate facilities and publishing that we find for the disciplinary researchers. But the lack of any clear relationship between publishing and adequacy of facilities for applied and organizational researchers could only be explained by assuming that adequate facilities are bestowed for reasons other than having a reputation in the discipline--contributing to organizational goals, or loyalty, perhaps. These possibilities are important enough to warrant further research and analysis, though to attempt to do so here would take us too far from our main task.

We turn now to Hypotheses 3 and 4 which deal with the effect of the organizational reward system on disciplinary participation. I have suggested that the value a scientist places on disciplinary participation is a function of the <u>organization's</u> reward system as well as the discipline's. And to the extent that the organization rewards a scientist for the same commodities they exchange in the discipline, or, put otherwise, to the extent the organization rewards disciplinary participation, the <u>value</u> of disciplinary participation and hence <u>motivation</u> to participate will increase with the result that participation itself will tend to increase.

The converse should hold as well: to the extent that a scientist cannot obtain organizational rewards for disciplinary participation his motivation and consequently his participation should be less. Hypothesis 3 states this relation explicitly:

> The less an organization rewards disciplinary participation, the less will be the motivation of its incumbents to participate in the discipline, and, hence, the less will be their disciplinary participation.

An index of the extent to which work organizations reward disciplinary participation was provided by the following question:

> Where you now work, how much does your advancement or continued employment depend on your publishing professionally?
>
> ( ) Very much
> ( ) Much
> ( ) Somewhat
> ( ) Little
> ( ) Very little or none

Those answering "Very much" or "Much" are treated as working in organizations where such rewards were "present" and those giving other responses as working in organizations where such rewards were "absent". One obvious flaw in such a question is that it deals with an organization's reward system with respect to only one, albeit an important, form of disciplinary participation, publishing professionally. Hence we cannot know for sure that an organization that rewards professional publishing would reward other forms of participation as well, though it seems reasonable to suppose that they might.

Table IV.4 presents the relationship between this measure of the organizational reward system and the five kinds of disciplinary participation. Though all are in the expected direction, only two, attendance at national meetings and publishing frequency, are statistically significant. However, accepting Hypothesis 3 even for only these two forms of participation depends on showing that the organizational reward system affects participation <u>through</u> motivation. That is, that organizational rewards affect motivation which in turn influences partipation. Statistically this means that the relationships between the reward system and disciplinary participation should be <u>less</u> in <u>both</u> partials when motivation is held constant.[5]

Table IV.5 presents the partial relations for motivation[6] and participation for scientists in organizations where organizational rewards are bestowed for publishing and where they are not. The forms of disciplinary participation that were not statistically significant are also included for comparison.

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[5]That is, in Hyman's terms, we expect that motivation will "interpret" the relationship between the organizational reward system and disciplinary participation. Herbert Hyman, <u>Survey Design and Analysis</u>, (New York: The Free Press, 1955), pp. 276-295.

[6]Consistent with our theoretical position which sees participation as a junction of the anticipated value of the participation, motivation was measured separately for each form of participation. For participation in national meetings: "Do (or would) you find participating in national meetings <u>usually</u>: ( ) Very valuable; ( ) Valuable; ( ) Somewhat valuable;

It is evident from the percentage differences that the only form of participation for which both partials are reduced is professional publishing. We can accept Hypothesis 3 then only with respect to this one form of participation. It is worth noting that the impact of organizational rewards on publishing is greatest for those scientists with low motivation to publish. In other words, it appears that the organizational rewards induce disciplinary participation from those scientists who are not self-motivated to participate; while for those who have internalized the value of publishing professionally, the effects of the organizational reward system are much less. There thus appear to be not just one but two fairly distinct ways in which the organizational reward system affects publishing. First, as suggested by Hypothesis 3 and as indicated by the reduction in both partials in Table IV.5, the organizational reward system may influence publishing professionally by affecting motivation to publish. And secondly, as the differences in publishing for similarly

( ) Not valuable; ( ) A waste of time; for attending conferences and symposia: "Do (or would) you find these [the conferences or symposia] usually: ( ) Very valuable. . . etc.; for both measures answers of Very valuable or Valuable were regarded as indicating high motivation. For both measures of contact with others: "Do (or would) you usually find such contacts: ( ) Valuable. .. ." Answers of Very valuable or Valuable were regarded as indicating high motivation. For motivation to publish professionally: "As far as you are personally concerned, how important is it to you to publish professionally? ( ) Extremely important; ( ) Very important; ( ) Somewhat important; ( ) Fairly unimportant; ( ) Not at all important." Answers of Extremely important or Very important were taken as indexes of high motivation.

TABLE IV.4

PERCENT OF SCIENTISTS WITH "HIGH" DISCIPLINARY PARTICIPATION
BY PRESENCE OR ABSENCE OF ORGANIZATIONAL REWARDS FOR PUBLISHING

Type of Disciplinary Participation	Organizational Rewards for Publishing					
	Present	(N)	Absent	(N)	Chi Square	p
Attended 2 or 3 National Meetings in Last 3 Years	53.9	(390)	47.6	(595)	3.55	$<.10$
Attended 2 or More Conferences or Symposia in Last 3 Years	74.1	(390)	72.6	(595)	.19	n.s.
Contacted Others Once a Month or More	40.5	(390)	39.3	(595)	.09	n.s.
Contacted by Others Once a Month or More	42.3	(390)	38.8	(595)	.99	n.s.
Published Often	71.3	(389)	41.7	(590)	80.88	$<.001$

TABLE IV.5

PERCENT OF SCIENTISTS WITH "HIGH" DISCIPLINARY PARTICIPATION BY MOTIVATION TO PARTICIPATE AND PRESENCE OR ABSENCE OF ORGANIZATIONAL REWARDS FOR PUBLISHING

Type of Disciplinary Participation	Motivation		Organizational Rewards for Publishing		
			Present	Absent	% Difference
Attended 2 or 3 National Meetings in Last 3 Years	High		60.3	53.5	6.8
	Low		42.7	37.8	4.9
		TOTAL	53.9	47.6	6.3
Attended 2 or More Conferences or Symposia in Last 3 Years	High		78.2	76.7	1.5
	Low		63.6	63.6	0.0
		TOTAL	74.1	72.6	1.4
Contacted Others Once a Month or More	High		49.5	47.7	1.8
	Low		18.0	22.3	-4.3
		TOTAL	40.5	39.3	1.2
Contacted by Others Once a Month or More	High		50.7	52.8	-2.1
	Low		32.8	24.3	8.5
		TOTAL	42.3	38.8	4.5
Published Often	High		76.6	65.2	11.4
	Low		54.1	26.0	28.1
		TOTAL	71.3	41.7	29.6

motivated scientists shows, organizational rewards for publishing also appear to increase the publishing rate for both highly and less highly motivated scientists, but particularly for the less highly motivated.

This suggests that there may be two rather different motivational bases for participating in the discipline. For example, Gross, Mason, and McEachern have suggested that individuals may adopt different orientations toward the expectations of counter positions ranging from a "moral" orientation in which an actor feels that these expectations are legitimate and _ought_ to be obeyed, to an "expedient" orientation, in which the actors first assess the relative rewards and punishments associated with living up to these expectations. A third mode of orientation, the "moral expedient," falls between these extremes and combines elements of both.[7] Etzioni has also proposed what appears to be a roughly equivalent classification of organizational compliance, and his "normative" and "calculative" orientations correspond to the moral and expedient types of Gross, _et al_.; like them, he also allows for mixed types.[8] It is possible that the highly motivated scientist, one for whom publishing professionally is important, publishes frequently because he regards publishing as part of his scientific role

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[7]Neal Gross, Ward S. Mason and Alexander W. McEachern, _Explorations in Role Analysis_, (New York: John Wiley and Sons, Inc., 1958), pp. 289-293.

[8]Amitai Etzioni, _A Comparative Analysis of Complex Organizations_, (New York: The Free Press, 1961).

and therefore part of his corresponding identity as a scientist; he publishes because that is what he as a scientist ought to do. In the terms of Gross et al., and Etzioni, he has a moral or normative orientation toward the discipline. On the other hand, the scientist who is less highly motivated, for whom publishing professionally is not particularly important, may not regard the role of scientist as demanding this sort of activity, in which case it would not be an essential element of his scientific identity. He would attempt to publish only when he had calculated the relative advantages of doing so, when the relative costs and rewards accruing to him from the environment would make it expedient to do so.

With the present data it is not possible to know if the two types of motivation do in fact correspond to our measures of high and low motivation to publish, or if they did, whether they would grade into one another gradually or if they would be discontinuous. They could be qualitatively different and still permit an individual to move from one orientation to another in response to organizational or disciplinary rewards. Organizational and disciplinary commodities received in an exchange may have more than one meaning and hence more than one value for individuals: money, power, prestige, etc., may be intrinsically rewarding; they may have value because they are exchangeable for still other valued commodities; and they may symbolically reflect upon the adequacy of an individual's role performance. Which of these values a particular commodity may have would depend upon whether an individual was expediently or morally oriented. For example, the role related symbolism of the commodity would be less important for an expediently oriented individual since he would not see his behavior as necessarily reflecting upon some valued dimension of his role performance. Thus, although the nature of the costs and rewards associated with a commodity might differ for the two orientations, similar exchange processes could underlie both. It should be difficult to sustain indefinitely either a moral or an expedient orientation in the face of a consistent lack of rewards, whatever these might be. Insofar as Table IV.5 is concerned, it may be simply that highly motivated (morally oriented) scientists are affected less by the organizational reward system than the less motivated (expediently oriented scientists) because organizational rewards are relatively less significant than organizational rewards for confirming some aspect of their role performance.

A study by Glazer[9] is particularly relevant in light of these remarks. He found that scientists who had had more opportunities to learn the institutional (i.e., disciplinary) definition of the scientific role--one that involves publishing--were less likely to lower their motivation to do basic research

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[9]Barney G. Glazer, "'Differential Association' and the Institutional Motivation of Scientist," The Administrative Science Quarterly, Vol. 10, No. 1, (June 1965), pp. 82-97.

in the absence of organizational recognition for their work than were those whose socialization into the disciplinary role pattern was probably less adequate. Glazer's findings and the data from Table IV.5 suggest that scientists with low motivation, those with perhaps a calculative or expedient orientation toward the discipline, are most responsive to organizational rewards and may also be the most likely to increase or decrease their motivation in response to organizational or disciplinary rewards for publishing.

The fact that Hypothesis 3 was confirmed only for publishing and not for the other forms of disciplinary participation is instructive. It is perhaps no accident that the one case where the partial relationships between motivation and publishing were reduced is also the case where indexes of participation and of the organizational reward system are closest conceptually and operationally. Significant also, is the fact that thus far Hypotheses 1-3 have been supported consistently for only this one of the five measures of disciplinary participation. This raises an important point: unlike publishing, the other four measures of disciplinary participation may simply be too general or inclusive with respect to the kinds of exchanges to which they refer to reveal any actual impact of organizational characteristics on them. Thus, in contrast to the amount of professional publishing, the other forms of disciplinary participation are, as measured, less definite with respect to the actual <u>content</u> of the exchanges

that they may refer to, and may actually cover a variety of different transactions involving commodities other than information. As a result, they might not reflect with any sensitivity or exactness the theoretically specifiable relations between organizational characteristics and disciplinary motivation or participation. For these reasons the remaining analysis will be restricted to only one form of participation, the number of published professional papers.

As far as the present theoretical perspective is concerned, this measure has the greatest face validity of any of the other measures of disciplinary participation. These other measures are somewhat ambiguous with respect to the actual content of the exchanges they purport to measure; that is, they are not specific enough regarding the kinds of behavior that may comprise such an exchange. Lacking this specificity it is possible, as suggested above, that "high" participation may include a number of quite different activities in addition to those of special interest here, the actual exchange of scientific information, recognition, support, ideas, etc.. In this respect the number of papers published has the virtue of being fairly clear; it refers to a specific and relatively well defined communicative act, an exchange of information for recognition. Both elements of the exchange are captured by such a measure since the fact that it was published indicates both that it was offered and that it received recognition through being deemed worthy of publication. There is another reason for restricting disciplinary participation to only publishing frequency; three of the organizational measures, adequacy of research facilities, the extent to which promotion and continued employment depend on publishing professionally, and the type of criteria used in selecting research problems, appear to be more directly related to publishing, or at least to producing exchangeable information, than they are to the other participation measures. Because of this closer conceptual affinity, any real relationships between organizational characteristics and participation are most likely to be found with this measure. Though limiting participation to publishing means a narrower concern with disciplinary participation, this restriction will hopefully be offset by the fact that any findings from this single more clear-cut case will provide a basis for further research on the other forms of disciplinary participation.

Rewarding a specific kind of disciplinary participation such as publishing is not the only way an organization's reward system might affect a scientist's motivation to participate in his discipline. As Chapter III argued, work organizations that contain role-sectors offering relatively greater opportunities to acquire <u>organizational</u> rewards than are available in a research role-sector should tend to lower the value or motivation for disciplinary participation. As Hypothesis 4 proposed:

> The more an organization offers alternative organizational role-sectors that are more

> rewarding than those in which they might acquire exchangeable information, the less will be the motivation of its incumbents to participate in the discipline, and, hence, the less will be their disciplinary participation.

It might have been possible to devise some quite sophisticated measures of competing role-sectors that would take into account the number of alternatives and the degree to which they were more or less rewarding than the research role-sector. However, on the reasoning that more complicated measures might later be employed if a simple one proved to be effective, only a crude index was actually used. The scientists were asked to:

> . . . indicate how good the opportunities are where you are now employed for advancing your career through each of the following:
>
> a. Teaching or working with students.
>
> b. Managing or administering of other than research or development.
>
> c. Managing or administering research or development.
>
> d. Using existing scientific knowledge or techniques in some area of applied science, like quality control, clinical work, etc.
>
> e. Doing research having practical applications in some area of science.
>
> f. Doing research contributing to basic knowledge in science.

They rated the opportunities in each of these role-sectors as either Very good, Good, Poor, Very poor, or Don't know. The index was constructed separately for scientists doing disciplinary and for those doing either applied or organizational research. If the disciplinary researcher judged the opportunities in <u>any</u> of the role sectors as better than alternative "f", "Doing research contributing to basic knowledge in science," then their current work organization was classified as offering alternatives that competed with basic research. If the opportunities in all other role sectors were the same as or worse than alternative "f", competing alternatives were regarded as absent from their work organizations. A similar procedure was followed for scientists doing applied or organizational research, except that the relative advantages of each alternative were compared to alternative "e", "Doing research having practical applications in some area of science."

Table IV.6 presents the relationship between professional publishing, the presence or absence of competing alternatives, and motivation to publish. Consistent with the hypothesis, fewer scientists publish often in work organizations that have competing alternatives than where such alternatives are absent. And, in further support of Hypothesis 4, the fact that <u>both</u> partial relationships between competing role alternatives and publishing are reduced when motivation to publish is held constant indicates that the presence or absence of organizational alternatives affects motivation to publish.

In contrast to the situation in Table IV.5, however, the presence of competing alternatives has its greatest effect not on the <u>least</u> but on the <u>most highly motivated</u> scientists,

TABLE IV.6

PERCENT OF SCIENTISTS WHO PUBLISH OFTEN BY COMPETING ALTERNATIVES, AND MOTIVATION TO PUBLISH

	Competing Role Alternatives				
Motivation	Absent	(N)	Present	(N)	% Difference
High	73.1	(417)	66.4	(131)	6.7
Low	35.7	(280)	33.8	(151)	1.9
TOTAL*	58.1	(697)	43.6	(282)	13.9

*Chi Square=16.30 p .001 d.f.=1

and thus seems to call into question the interpretation of Table IV.5 which suggested that we might be dealing with <u>two</u> types of motivation. Without attempting to minimize this discrepancy, it is perhaps relevant that the two tables differ in at least one important respect: on the theoretical level there seems to be a clearer and more direct relationship between organizational rewards for publishing and motivation to publish than between publishing and competing role alternatives, and this discrepancy is reflected empirically since the percentage difference is about twice as large for organizational rewards than for competing alternatives. Consequently, competing alternatives may have less of an impact on motivation, or may have some other significance for scientists than do specific rewards for publishing. At any rate, it is evident that additional research is needed to clarify the relation between various possible forms of motivation as well as the impact of different aspects of organizational reward systems on them.

Hypotheses 1 and 2 dealt with two organizational characteristics that affect opportunities to acquire exchangeable information, the adequacy of research means or facilities, and the extent of disciplinary control over research. We have also argued that these same factors, because they are associated with perceived opportunities to acquire exchangeable information, should also affect motivation to publish. As Hypothesis 5 proposed:

> The less adequate are the facilities for research in an organization, or the more it directs research

> toward its own ends rather than to those of the discipline, the less will be the motivation of its incumbents to participate in the discipline, and, hence, the less will be their disciplinary participation.

Table IV.7 presents the relationship between type of research (extent of disciplinary control), professional publishing, and motivation to publish. The reduced percentage differences in all three partial relationships when motivation is controlled indicates that the extent of disciplinary control over research affects the level of publishing by affecting motivation to publish.

As Table IV.8 reveals, however, both partials are <u>not</u> reduced when controlling for motivation and we are forced to conclude that inadequate facilities <u>do not</u> affect publishing through motivation, though the impact of facilities is somewhat greater on those more highly motivated. Nor, for that matter, are both partials reduced when we consider disciplinary scientists separately,[9] those for whom, as we saw in Table IV.3, inadequate facilities were strongly associated with less frequent publishing.

It is possible that scientists would regard not being able to direct their research toward disciplinary goals as more consequential than inadequate facilities in determing the likelihood that they would produce an original discovery that would earn recognition from disciplinary colleagues. It

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[9]Table not shown.

TABLE IV.7

PERCENT OF SCIENTISTS WHO PUBLISH OFTEN BY TYPE OF RESEARCH AND MOTIVATION TO PUBLISH

Motivation	Type of Research			% Difference		
	Disciplinary (1)	Applied (2)	Organizational (3)	1-2	2-3	1-3
High	76.6	75.5	45.9	1.1	30.6	30.7
Low	47.5	38.0	16.9	9.5	21.4	30.6
TOTAL	68.7	57.7	26.0	11.0	31.7	42.7

TABLE IV.8

PERCENT OF SCIENTISTS WHO PUBLISH OFTEN BY ADEQUACY OF RESEARCH FACILITIES, AND MOTIVATION TO PUBLISH

	Adequacy of Research Facilities		
Motivation	Adequate	Inadequate	% Difference
High	74.5	62.8	11.7
Low	25.6	27.4	-1.8
TOTAL	56.8	46.0	10.8

is also possible that the _type_ of research involves more than just differential disciplinary control over research; it implicitly may also involve organizational and collegial expectations and rewards for carrying out activities associated with the type of research. Thus disciplinary researchers might be expected to publish professionally as part of their research role and would be rewarded for conforming to this role prescription. The expectations for publishing might be less for applied and organizational researchers who would gain correspondingly fewer rewards for publishing. Some indirect support for this latter interpretation is seen in the fact that 57.3 percent of the disciplinary scientists work in organizations that reward publishing in contrast to 35.7 percent of the applied scientists and only 13.8 percent of the organizational scientists.

Summary

This chapter tested the five hypotheses derived from the theoretical discussion of the two preceding chapters. Hypotheses 1 and 2 specified the relationship between two characteristics of scientific work organizations that would increase or decrease a scientist's participation in his discipline by affecting his _opportunities_ to _acquire_ information that would be exchangeable in his discipline. Hypotheses 3, 4 and 5 concerned organizational characteristics that would increase or decrease a scientist's disciplinary participation by affecting his <u>motivation</u> to participate in his discipline. These hypotheses were tested for five types of disciplinary participation for a sample of full time employed microbiologists engaged in research.

We are forced to conclude that these five hypotheses can be accepted only for <u>one</u> of the five measures of disciplinary participation, professional publishing. For only with this form of participation were the results large and quite consistently in the hypothesized direction. There is reason to believe that, unlike the amount of professional publishing, the other four measures of disciplinary participation may have grouped a number of rather different types of exchange relationships and hence obscured the effect of organizational characteristics upon the exchange of new information. The most important findings and conclusions from this chapter can briefly be summarized as follows.

Regarding Hypotheses 1 and 2, inadequate facilities and decreasing disciplinary control over research are both associated with less publishing. But when both organizational characteristics are considered together, inadequate facilities affect publishing only for scientists doing research directed toward disciplinary ends. This suggests either that working on research directed toward disciplinary goals is more important than adequate facilities for producing exchangeable information, or that disciplinary researchers are rewarded with adequate facilities in proportion to their publishing.

Evidence on how frequently scientists contact others and are contacted by them also suggested that individuals doing disciplinary, applied, or organizational research may be involved in different sorts of exchange relationships; that is, they may exchange different kinds of information among themselves. In short, there is indirect evidence that the discipline of science may best be represented as having several relatively distinct exchange structures. Data for exploring this further, however, are not available.

Regarding Hypothesis 3, the presence or absence of organizational rewards for publishing appeared to have altered the frequency of publishing by affecting motivation to publish. But an important additional effect was also noted. The organizational reward system <u>also</u> appears to have affected publishing without the mediating effects of motivation. We hypothesized that this may indicate two fairly distinct orientations toward the discipline: a moral or normative orientation for those highly motivated to publish, in which scientists regard publishing as part of their role obligations; and a calculative or expedient orientation for those who may be less highly motivated, in which publishing depends on the calculated rewards and costs for doing so rather than upon any notion that publishing is an essential element of one's role and identity.

Because professional publishing seemed to be a more precise measure of the actual content of one form of participation than the other four measures of participation, and because publishing was the only measure that yielded consistent results, the remaining analysis was confined to publishing frequency.

Hypothesis 4 concerning the impact of competing organizational role alternatives on motivation and hence on publishing was supported.

In partial support of Hypothesis 5, the type of research or extent of disciplinary control also appeared to affect professional publishing by changing motivation to publish; this suggests that scientists may recognize the connection between the ends toward which their research is directed and the probability that they will be able to acquire information they can exchange in the discipline. Contrary to Hypothesis 5, however, the perceived adequacy or inadequacy of research <u>facilities</u> or <u>means</u> does not seem to affect participation by altering motivation.

CHAPTER V

THE EFFECT OF WORK ORGANIZATIONS ON CHANGES IN DISCIPLINARY PARTICIPATION AND MOTIVATION

In the preceding chapter the one form of disciplinary participation that was consistently related to the organizational characteristics was publishing. Before the meaning of these relationships can be fully understood, however, certain conceptual and methodological difficulties must be disposed of. To begin with, a correlation between attributes of individuals, such as motivation or disciplinary participation, and attributes of organizations, such as reward systems, may result from any one or any combination of three processes: attributes of individuals and organizations may become matched through the selective recruitment of individuals into organizations of a particular type; through later selective mobility from organization to organization; and through <u>changes</u> in the attributes of individuals or organizations.[1] In the present situation, for example, scientists

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[1]For an excellent general discussion of this issue see Donald I. Warren, "'Structural Effects': Index of Social Structure Statistical Artifact?", Paper presented at the Annual Meeting of the American Sociological Association, Chicago, Illinois, September 1965.

might choose to join organizations that provided opportunities and rewards that appeared to be consistent with their own motivations, and organizations might actively recruit scientists whose aspirations appeared consistent with their own objectives; or scientists might later move from less to more compatible organizations; and finally, in response to the organization's reward system or to perceived opportunities to acquire exchangeable information, scientists might change their participation in the discipline, their motivation or both.

Hypotheses 1-5 implicitly assumed the third possibility: that organizations somehow increase or decrease participation because they affect opportunities for acquiring exchangeable information, because they affect motivation to participate, or because they affect _perceived_ opportunities to acquire exchangeable information. The analysis in Chapter IV, however, provides no means for determining whether one or some combination of recruitment, selective mobility, or changes in individuals may account for the observed correlations between various organizational characteristics and the amount of professional publishing.

This chapter is concerned with determining if work organizations have _changed_ disciplinary participation or motivation. This is not to say that it will be unconcerned with selective recruitment or mobility. As the first chapter pointed out, this dissertation is chiefly concerned with the ways in which the disciplinary relationships among scientists are _modified_

by their participating in different kinds of work organizations. In any event, recruitment and mobility cannot be ignored since their possible effects must be controlled in any imputation that organizations have changed participation or motivation.

The next few pages examine the literature bearing directly on the possible role of work organizations on such changes. This review will focus on those empirical clues, theoretical propositions, or informed speculations pertaining to the organization's role in effecting such changes. For a fair assessment of the extent to which the present theoretical framework adequately accommodates this material, it should be pointed out that the theory was constructed in part in order to systematize and organize much of it.

Studies of Organizationally Induced Changes in Disciplinary Participation and Motivation

Unfortunately no more than a handful of studies bear even indirectly on changes in disciplinary participation or motivation. Even fewer touch upon the possible effects of work organizations on such changes, and fewer still consider changes within the framework of an explicit theory.

In an influential observational study of the central research laboratory of a large electronics corporation Marcson identified four types of scientific career goals, three of which represented changes brought about by organizational participation. According to Marcson one type of scientist remains in research, but becomes less interested in basic and more interested in applied research. A second type "after years of successful achievement comes to realize the financial and prestige ceiling in research and becomes interested in moving into administration." And the third type "after years of achievement comes to feel that he can no longer compete and becomes interested in moving into administration."[2]

There is a temporal sequence to these types. When he enters the organization, the new Ph.D. believes he will have an opportunity to pursue fundamental research, and "in his early years in the laboratory the scientist remains devoted to the scientific career goals which he acquired as a graduate student," but gradually he acquires an interest in the practical research in which he is daily involved. From that point on he may become interested in an administrative career, or in advancing through the laboratory organization, and, "<u>in doing so does not concentrate on acquiring a status in his profession</u>." They may, in the words of one administrator, "begin to see themselves no longer in terms of a professional in their relations with the professional world, but more as. . . [organizational] employees." And as Marcson observes, they have "a conception of themselves as employees on the bottom rungs of

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[2]Simon Marcson, <u>The Scientist in American Industry: Some Organizational Determinants in Manpower Utilization</u>, (Princeton, New Jersey: Industrial Relations Section, Department of Economics, Princeton University, 1960).

the administrative ladder. Their career goals of administration have been influenced by the laboratory's conception of success."[3]

According to Marcson these changes are brought about in part at least through the organization's efforts to "broaden his interests" to include working on applied problems that concern the organization, and to convince him that what he is working on is what he really wants to do. His discussion also suggests that the process of accepting organizational goals may be gradual: in the course of his work, the scientist may become interested in practical problems, or be slowly drawn into accepting more and more administrative chores, until he comes to realize that his career is inextricably tied to the organization.

Although Marcson does not offer a systematic theory of how industrial work organizations change scientists, it is evident from his remarks that several of the processes or mechanisms touched upon in Chapters II-III may be involved. Thus, his description of the second and third types of goal orientation carries with it a fundamental assumption of exchange theory: scientists abandon research and disciplinary participation for administrative positions in order to pursue more readily attainable or more attractive organizational rewards. And Marcson's observations on the overt efforts of

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[3]Marcson, The Scientist. . ., pp. 67-68. My emphasis.

the organization to reshape research interests along organizational lines suggests that the organization succeeds in re-socializing the scientist by subtly encouraging and discouraging certain kinds of activity, and that the scientist, perhaps without realizing that it is happening, comes gradually to accept organizational norms regarding his work. Marcson's observations then suggest that scientists may become involved in administration and applied research activities that are less compatible with disciplinary participation than basic research. Unfortunately we do not also know in this case if these changes in role were also associated with changes in disciplinary participation and motivation. However, Marcson's suggestion, quoted above, that those in administration may be less interested in acquiring status in the profession certainly points in this direction. There is at least a strong suggestion here that the value system of the work organization and the limited opportunities to acquire exchangeable scientific goods have induced scientists to become involved in alternative exchange relations within the work organization at the expense of exchanges within the discipline.

William Kornhauser's Scientists in Industry[4], a comprehensive compendium and analysis of materials on scientists in industrial work organizations, explores in detail many of

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[4]William Kornhauser, Scientists in Industry: Conflict and Accommodation, (Berkeley: University of California Press, 1963).

the strains and mutual accommodations between scientists and the industrial organizations in which they work. Kornhauser explicitly recognizes a link between work organizational and disciplinary participation and motivation. Through its resources and incentives, the organization has the capacity to motivate scientists to comply with organizational and professional goals; but it faces a serious dilemma:

> If the work establishment permits its professional employees to be identified solely with the profession, and to treat the organization merely as a place of work, then it will not be able to motivate sufficiently its professional people to help achieve the goals of the organization. In consequence, professional contributions will be small and turnover high. If on the other hand the organization seeks to stress organizational incentives at the expense of professional incentives, then it will not be able to acquire a satisfactory <u>professional</u> performance from its specialists. In short, the work establishment faces the dilemma of seeking too much integration of its professionals into the organization and thereby losing their professional work, versus granting them too much autonomy and thereby weakening their contribution to the organization.[5]

Industry, as Kornhauser points out, pressures scientists to "de-emphasize professional concerns in favor of organizational concerns." Scientists depend upon management for advancement, they are often offered inducements to leave research for management, and because it offers a greater opportunity to use their abilities, scientists may even find management more challenging than research where they are working below their

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[5]Kornhauser, <u>Scientists in Industry</u>. . ., p. 130.

skill level.[6] Kornhauser believes that when organizations induce scientists to engage in administration or in the practical applications of research "research creativity and the professional commitment on which it depends are weakened."[7] This is because,

> Unless scientists are primarily concerned with their professional allegiance, they will be less likely to uphold scientific standards or to aspire to scientific excellence. Where scientific standards and aspirations are weak, the quality of scientific performance will not be high. Hence, where industry dampens the motivation of scientists to participate in outside professional activities, industrial research suffers.[8]

These conclusions and the presumptions of organizationally induced changes, reasonable and persuasive as they are, however, are not supported by empirical evidence that change rather than initial recruitment or mobility may be the causative factors involved.

Hagstrom's emphasis upon the scientific community's role in maintaining patterns of scientific activity may have sensitized him to factors that alter or disrupt disciplinary control of scientific work (and hence ultimately of disciplinary participation) since numerous references to this are scattered throughout his book on the scientific community. One important agency supporting the discipline's social control is the

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[6] Kornhauser, Scientists in Industry. . ., pp. 136-149.

[7] Ibid., p. 156. My emphasis.

[8] Ibid., p. 155.

primary work group.

> . . . the forms of recognition awarded in primary groups of scientists tend to make the institutional incentives meaningful for day-to-day work. /In universities/ they typically reinforce the effects of institutional incentives, and without them scientists might conform less to the norms and values of science: they might be less disposed to work, to publish, or to select problems and techniques within the scope of their disciplines.[9]

But if work groups may help to sustain disciplinary control, they may also undermine it if competing rewards interfere with the ability of the discipline to exert social control. This is especially true because the rewards and gratifications that small groups of scientists offer are immediate and personal in contrast to the often delayed and impersonal rewards of the discipline.

> . . . recognition and status in the primary group or the small organization may become more important to the individual than recognition and status in the larger community. If the norms and values of such smaller groups differ from those of the larger community, commitment to the latter will be weakened.[10]

Hagstrom goes on to comment that,

> Among scientists who remain in industry there is a strong tendency for the incentives offered by the employing organizations--interpersonal approval as well as formal status and salary--to become more important than recognition by the larger scientific community.[11]

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[9]Warren O. Hagstrom, The Scientific Community, (New York: Basic Books, Inc., 1965), p. 36.

[10]Ibid., p. 36.

[11]Ibid., p. 37, my emphasis.

This statement provides strong support indeed for the theoretical position from which we are viewing the relationship between work organizations and scientific disciplines. Unfortunately, there is little or no direct evidence to justify it. As support for his assertion Hagstrom cites Scientist's in Industry, (in the preparation of which he assisted William Kornhauser), which reviews a number of studies of scientists and engineers in industrial settings. The fact is, however, that none of the studies reviewed there discuss changes in a scientist's orientations from his discipline to his work organization. What these studies do reveal is a correlation between types of work organizations and disciplinary participation or motivation, i.e., in the sorts of organizational or disciplinary rewards that are preferred by those in research, application, or administration. But, as has already been observed, such a correlation, although suggestive, does not constitute proof of change since the possible effects of recruitment or mobility have not been eliminated.

Hagstrom also offers a somewhat different analysis of the effects that organizations may have on disciplinary participation or motivation. Although approached chiefly as one possible consequence of "segmentation" within a scientific discipline, his analysis of the consequences of anomie in science can be applied more generally to situations in which scientists do not receive recognition from their discipline for whatever reason including, we may assume, a lack of opportunity for acquiring exchangeable information, Hagstrom views the consequences of lack of recognition in terms of Merton's paradigm of possible adaptations to discrepancies between cultural goals (in this case recognition) and institutionalized means for achieving them, i.e., contributing information. He observes that "adaptations to a situation in which needs for recognition are continually frustrated may take the form of ritualism, retreatism, or rebellion."[12]

> The ritualist abandons the goal of receiving recognition for his contributions but continues to practice the institutionally prescribed means for achieving the goal, that is to say, he continues to contribute "information" in the usual way.
>
> Failure to be recognized, to have one's judgments reinforced by the judgments of others, may lead to a loss of faith in the value of one's work. When a man is highly specialized, and especially when he is old and dislikes the idea of spending years working into a new specialty, such losss of faith may be followed by a general withdrawal from creative work, a renunciation of both the goals and means of science.
>
> The scientific rebel rejects the goal of achieving recognition from members of the specialty in which he formally finds himself, and he rejects the means for doing so, namely, contributing information of a specific type; but, instead of withdrawing, the rebel substitutes a new community from which he desires recognition, and substitutes as means the contribution of a different type of information.[13]

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[12]Hagstrom, The Scientific Community, p. 232.

[13]Ibid., pp. 232-234.

Rebellion in the above sense involves the collective action of scientists to segmentation within their discipline rather than individual responses to general lack of recognition and has, therefore, only limited relevance for the present discussion.

Another form of rebellion more germane to our present concern occurs when scientists supply information for other than disciplinary goals, i.e., in order to receive tenure, or perhaps to comply with an employer's expectations.[14]

The retreatist who withdraws from research usually "emphasizes other activities in which he can engage--especially teaching, but also administration of university and industrial laboratories, and perhaps sometimes leisure activities." Hagstrom goes on to point out that "these activities have rewards of their own that attract scientists in any case, and most discussions of withdrawal from research activity are in terms of conflicting role sets and career possibilities."[15] This view of the scientist substituting new goals or means of his own choosing from available alternatives is present also in Hagstrom's observation, derived from Durkheim, that in anomic societies (which the scientific discipline resembles when its own information-recognition control mechanism no longer operates), members insist "on the absolute value of personal goals and on the unrestrained freedom of the individual."[16]

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[14] Hagstrom, The Scientific Community, pp. 232-233.

[15] Ibid., p. 235.

[16] Ibid., p. 232.

Running throughout this analysis is a common implied theme that bears directly on what happens to scientists if they do not receive recognition from their discipline. With the possible exception of the ritualist for whom the possibility is not discussed, Hagstrom seems to suggest that scientists will (a) seek other goals and the rewards associated with them, and/or (b) abandon and possibly devaluate the old, unproductive goals or means. Even more specifically, there is the tacit assumption that when scientists substitute new means for research, or new goals for achieving recognition within the discipline, these will be drawn from alternatives, e.g., teaching, administration, appropriate to their long training and experience and connected with that other principal reward structure of science, the work organization. That is, the work organization will assume relatively greater importance for them.

A survey of a large government medical research organization devoted to basic research comes closest to empirically establishing the information-recognition-exchange view of scientific participation and motivation, as well as the theoretically stated relationship between the amount of reward received in an exchange and the likelihood of engaging in that exchange. Although he did not have longitudinal data, by statistical means Glazer was able to demonstrate the following sequence: feeling that they had received much recognition from their organization for their research contributions[17] led

[17]Because this organization emphasized professional recognition for advancement it is reasonable to expect that Glazer's index of felt recognition in the organization also corresponded to felt recognition from colleagues in the discipline.

to (or at least maintained) high motivation for scientists to advance knowledge, which led to greater time devoted to their own research, and ultimately, to higher research performance as judged by organizational colleagues.[18] Motivation to advance knowledge can, in the present theoretical scheme, be equated with the value placed on engaging in exchanges in the discipline; that is, on motivation to exchange information for recognition. And additional analysis by Glazer confirms the interpretation that recognition maintains motivation to engage in exchanges for recognition, and also introduces evidence that prior socialization and alternative exchanges also influence motivation to engage in disciplinary exchanges. He found that commitment to the information reward pattern depends upon the amount of current rewards and the amount of exposure to

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[18]An earlier analysis of the same organization by Pelz, using the same data but different measures, found that a high "science orientation" was associated with high performance as judged by organizational peers. "Science orientation" was measured by "stress on rising present abilities or knowledge, freedom to carry out original ideas, and chance to contribute to basic research," and can be interpreted as tapping motivation to participate in the information-recognition exchange relationship. "Institutional orientation" was also measured and Pelz concluded "that a strong science orientation went with high performance mainly when the institutional orientation was weak; strength of science orientation was not significantly related to performance when the institutional orientation was strong." This suggests that those scientists who do not have their reliance on disciplinary rewards diluted by alternative rewards in the discipline will participate most fully in activities that may lead to disciplinary rewards. See Donald C. Pelz, "Some Social Factors Related to Performance in a Research Organization," Administrative Science Quarterly, Vol. I, (December 1956), pp. 310-325.

situations where this pattern is accepted. Scientists who had been exposed to "university employment, research and teaching, and Ph.D. training" were less dependent upon recognition for high levels of motivation than were those who had only one or two such experiences. He proposes that,

> the firmness of the internalization of the goal for those with fuller . . . experience [in settings supporting this goal] maintains high motivation without the current aid of recognition, while for those with some experience, who presumably do not yet have the goal as firmly internalized, current recognition is strongly needed to support high motivation.[19]

Glazer concludes that scientists who have had no exposure to situations in which the information-reward pattern of science is most dominant--university settings--may later,

> not quite comprehend this pattern or know how to pin their occupational fate on it. When they perform well they receive the rewards of an organizational career which may have little or no meaning for them as rewards of a career in science, as they do for the more fully inducted scientists.[20]

Length of exposure beyond a year did not alter the degree of motivation, leading Glazer to hypothesize that those who selected science were already willing to accept this goal, whereas if they had been selected by the organization they would have had to <u>learn</u> to accept this goal and length of exposure would have been associated with motivation.[21] Past

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[19] Barney G. Glazer, "'Differential Association' and the Institutional Motivation of Scientists," <u>The Administrative Science Quarterly</u>, Vol. 10, No. 1, (June 1965), p. 92.

[20] <u>Ibid</u>., p. 95.

[21] <u>Ibid</u>., pp. 88-90.

experience in applied settings such as "medical practice, hospitals, and private practice or business," did not appear to alter the motivation of scientists working in this basic research organization. But past experience in settings where service may have been stressed, "government agencies and the U.S. Public Health Service" was associated with less motivation to advance knowledge.[22] However, it is difficult to accept Glazer's argument that medical practice and hospitals are less service oriented than government agencies or the U.S. Public Health Service.

And finally Glazer found that in order to maintain their high motivation to advance scientific knowledge, scientists with M.D.s appear to depend more on current rewards than those with Ph.D.s. Glazer suggests that this is because they were not specifically trained to do basic research, and because they have, in private practice, an alternative source of reward available to them for which they were primarily trained. Ph.D.s, in contrast, have had greater exposure to the goal pattern and lack an alternative course of action, and thus may be more reluctant to abandon the goal in the face of deficient recognition.

The tendency for scientists to lower their motivation to advance scientific knowledge is one possible solution to lack of recognition. They may also change their organization,

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[22] Glazer, "'Differential Association'. . .," pp. 89-90.

change both goal and organization, change only the goal, or continue as before.[23] In the organization he studied, however, only a few scientists who had received low recognition planned to leave the organization, none planned to change both organization and goal, and a few planned to change their goal partially to applied knowledge. The few scientists in this organization who plan to leave or change their career goals led Glazer to conclude that "the strain of persisting, at least for the time being, in this highly prestigious research organization appears not to be very great for scientists who lack recognition. . ."[24]

The fact that an organization which by most standards would have to be judged as offering ample opportunities to acquire information and to offer rewards for disciplinary participation had an "unfavorable" impact upon motivation to participate in basic research suggests that these relationships might be even more marked in organizations where such opportunities and rewards were lacking.

One final study indirectly suggests organizationally induced change in disciplinary motivation. In a questionnaire survey of a large Naval research and development laboratory, the percentage of professionals who would choose occupationally related goals in order to achieve "a wide

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[23]Glazer, <u>Organizational Scientists</u>, p. 102.

[24]<u>Ibid</u>., p. 111.

reputation" decreased with increased status in the organization, while the percentage choosing organizationally related goals increased.[25] Since status was also related to age, this suggests that the length of time in the organization increased organizational motivation while reducing disciplinary motivation. But here again since the possibly spurious effects of selective leaving have not been eliminated we cannot be certain that changes in motivation have in fact occurred.

Studies of Changes in Disciplinary Participation or Motivation Unrelated to Organization

A number of authors have shown or suggested that scientific productivity decreases with age, without attempting to tie these changes into organizations in which scientists work. Indeed there is every indication that such a decrease is typical of the scientific career and that the types of organizations in which scientists work may only minimize or accentuate it. Reif and Strauss, for example, have vividly described conditions that contribute to rapid obsolescence of expertise in science. Because of the large amounts of money and great number of workers involved in scientific research, new fields are rapidly developed and "worked out" of new ideas. Consequently a scientist may find that the knowledge and experience so

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[25]Clovis Shepherd and Paula Brown, "Status, Prestige and Esteem in a Research Organization," Administrative Science Quarterly, Vol. I, 1956, pp. 340-360.

laboriously acquired in long graduate training and work experience has become obsolete relatively early in his career.[26]

Lehman's classic studies of age and achievement indicate that the more important a scientist's contribution (by concensus of historians of science) the earlier the age at which they were made, while lesser achievements peak later and decline in frequency much more slowly.[27] The most important discoveries in chemistry, for example, have been made by young men with the peak creative years falling between 30 and 35; for important but less significant works the most productive age is slightly later.[28] However, Lehman does not attempt to tie these findings to work organizational variables. This is remedied somewhat, however, by Pelz's study of 1,311 university, industrial and governmental scientists and engineers which reports similar findings.[29] Scientists were placed in five homogeneous groups: Ph.D.s in development--oriented labs, Ph.D.s in research-oriented labs, non-Ph.D.s in development

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[26]Fred Reif and Anselm Strauss, "The Impact of Rapid Discovery upon the Scientist's Career," Social Problems, Vol. 12, No. 3, (Winter 1965), pp. 297-311.

[27]Harvey Christian Lehman, Age and Achievement, (Princeton: Princeton University Press, 1953).

[28]Harvey Christian Lehman, "The Chemist's Most Creative Years," Science, (May 23, 1958), pp. 1213-1222); and "The Age Decrement in Outstanding Scientific Creativity," American Psychologist 15, 1960, pp. 128-134.

[29]Donald C. Pelz, "The 'Creative Years' and the Research Environment," Survey Research Center, The University of Michigan, Mimeographed, Undated, pp. 1-16.

oriented labs with few Ph.D.s, non-Ph.D.s in research oriented labs, and non-Ph.D.s in labs with many Ph.D.s. Each group showed a distinct bimodal productivity distribution by age, with the peak roughly between the late thirties and early forties, and another peak following this between five and ten years later.[30] Although, unhappily for our purposes, Pelz combined measures of organizational and disciplinary productivity.[31]

Beyond this striking finding the homogeneous groups also revealed several important systematic differences. Comparing the research and development labs, Pelz notes that,

> the peak and trough of the saddle occurred five to ten years later in development oriented labs compared with research oriented. In development, the first creative peak occurred at 45-49, whereas in research it was between 35 and 44.[32]

Furthermore, these broad patterns remained regardless of whether labs were in industry, government or the university,[33] which prompts Pelz to observe that "the distinction between 'research'

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[30]Pelz also reports a similar relationship for the number of citations in an annual review of physiology "for highly motivated physiologists". Donald C. Pelz, "The 'Creative Years'. . .", p. 6.

[31]Performance was measured with a ranking of each individual by his work colleagues according to how much his work contributed to his field and to his organization in carrying out research and development, and by a self-report of published papers, patent and patent applications, and unpublished reports.

[32]Ibid., p. 7; Italics in original.

[33]"Government Ph.D.s in development labs resembled Ph.D.s in industrial development, while government Ph.D.s in research resembled their counterparts in university laboratories." Ibid., p. 7.

<u>and 'development' orientation of the laboratory may be more fundamental than the laboratory's location in government, industry, or academia,</u>"[34] (a point which, it may be noted, this dissertation has implicitly assumed). There were some organizationally specific variations within this broad trend. For development labs "in government younger men did better and then declined; in industry performance continued to improve with age." And among Ph.D.s in <u>research</u> labs, those in the university were more productive overall than those in government, particularly after the productive peak. And finally, for non-Ph.D.s in labs with many Ph.D.s the drop off in contributions with age was severe for those in industry but continued to increase for government until a late peak.[35]

Unfortunately because of the combined organizational and disciplinary productivity measure, it is difficult to know if these changes resulted from changes in disciplinary or organizational participation or both. But the striking bimodal productivity pattern across organizational settings and their overall similarity to Lehman's work on scientists and other occupational groups, suggests that productivity increases and then declines with age for a number of reasons. Differences in organizational compatibility with productivity may contribute to this, but so many many other factors, such as the obsolescence of expertise already considered. Explaining the

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[34]Pelz, "The 'Creative Years'. . .", p. 7; my italics.

[35]<u>Ibid</u>., pp. 7-8.

overall pattern of productivity or any other form of professional participation typical of any scientific discipline remains a fascinating problem for the sociology and perhaps the social psychology of science, but it is not the primary concern in this dissertation. In short, I do not expect to show that _any_ decrease in disciplinary participation results entirely, or even mostly, from organizational factors, only that they account for _some_ portion of it.

It is probably safe to conclude that the present theoretical position is _consistent_ with the empirical studies covered in this chapter. At the very least, of course, this is what we would expect since the theoretical position is in part an attempt to organize these observations into a consistent framework. I do not want to overemphasize the agreement between this material and the present theoretical perspective, however. The data were often collected for other purposes, so they frequently dealt only indirectly with variables central to the theory to say nothing of crucial theoretical _relationships_ between work organizational characteristics and disciplinary participation and motivation. But the test of a theory is not only that it conforms to data from which it was inductively formed, for that is only a minimal condition that any valid generalization must meet. To say that a theory conforms, or at least does not seriously violate, existing facts, after all, is merely to suggest that it meets a _neccessary_ condition for validity; successful prediction with data chosen to conform as closely as possible to the theory's requirements is both a necessary and a <u>sufficient</u> condition of its validity. Nevertheless, the literature reviewed so far, taken in conjunction with the present theoretical orientation, seems at least to justify a sixth hypothesis which our discussion in this chapter has already anticipated.

> <u>Hypothesis 6</u>. Any relationship between organizational characteristics and disciplinary participation and motivation, such as those specified in hypotheses 1-5, will be due in part to <u>changes</u> in a scientist's disciplinary participation or motivation over time.

The remainder of this chapter will attempt to test this hypothesis.

The Effect of Work Organizations on Changes in Publishing and Motivation to Publish

As we have seen, any relationship between characteristics of individuals and of work organizations may derive from selective recruitment, later mobility, or changes in the organizational incumbent. To demonstrate that the correlations between how much a scientist publishes and certain features of the organization in which he works result in part from changes in the incumbent, the relationships must be shown to obtain in the face of controls for recruitment and mobility.

Survey data of the kind available in this study do not readily lend themselves to the application of such controls. This is largely because a mailed questionnaire samples only a static cross-section of what is in reality a constantly changing process. Attempts to sample from earlier time intervals by including retrospective questions on initial motivation or career mobility, as has been done here, are at best weak substitutes for longitudinal data since in dealing with fallible memories, distortions of unknown size and theoretical relevance may be introduced. In the present study the adequacy of such controls for motivational change or for mobility is also reduced because, in many respects, the study was exploratory. For this reason the questionnaire included a greater number of fairly general questions in order to cover a variety of unforeseen contingencies than would have been the case where more precise information was available. This, and some rather severe restrictions on the length of the questionnaire, necessarily limited the length and detail of retrospective questions concerning past disciplinary motivation or participation and characteristics of former work organizations. Had it been possible to know in advance precisely what independent variables we would ultimately be concerned with, it might have been possible with suitable questions to determine to what extent choice of a first job or later mobility were influenced by any of the organizational characteristics under consideration.

The fact is, therefore, that our controls for mobility and initial recruitment are less adequate than they might have ideally been or, more hopefully, than they need to be in any subsequent research. The reader is advised then that the less than perfect tools with which much of the subsequent data have been generated will have a bearing on the confidence with which the conclusions can be accepted.

Initial Recruitment

It is necessary to stratify the sample according to their original career goals in order to determine if the apparent organizational impact on motivation and hence on participation is due to scientists originally seeking out organizations with characteristics that were compatible with their motivation to publish. A retrospective measure of the career goals of the scientists at the time of their last degree is available in the questionnaire. The scientists were asked to rank each of "6 common career objectives of scientists. . . in terms of its importance to you WHEN YOU RECEIVED YOUR LAST DEGREE. . ." The six alternatives were:

a. Do research contributing to basic knowledge in science.

b. Do research having practical applications in some area of science.

c. Use existing scientific knowledge or techniques in some area of applied science, like quality control, clinical work, etc.

d. Manage or administer research or development.

e. Manage or administer other than research or development.

f. Teach or work with students.

Since we are primarily concerned with their original intentions to publish professionally, i.e., to contribute information to their discipline, all those scientists who ranked alternative "a", "Doing research contributing to basic knowledge in science," as first in importance as a career goal were regarded as highly motivated to contribute information to their discipline. Those who ranked alternative "a" less than first were treated as having relatively "low" motivation in this regard.

Tables V.1, V.2, and V.3 present the relationship between publishing professionally, present motivation to publish, and initial disciplinary motivation for the three organizational conditions which, as we saw in the last chapter, appeared to affect participation through motivation. From the percentage differences it is clear that in general the partials between all three organizational characteristics and publishing are reduced only for those scientists whose initial disciplinary motivation was low.

The two cases that do not follow this pattern occur with differences between the applied and organizational researchers in Table V.1. There the partials are reduced for scientists whose initial motivation has high rather than low. Perhaps an adequate explanation for this deviation from the overall pattern will have to wait until such time as we know more about other factors that may distinguish between applied and organizational research or researchers. At any rate, the overall trend seems clear: of the possible fourteen comparisons of percentage differenced in Tables V.1-V.3, only these two do not conform to the tendency for the partials to decrease among those whose initial

TABLE V.1

PERCENT OF SCIENTISTS WHO PUBLISH OFTEN, BY TYPE OF RESEARCH, MOTIVATION TO PUBLISH, AND INITIAL MOTIVATION

	Initial Motivation High Type of Research					
Present Motivation	Disciplinary (1) (N)	Applied (2) (N)	Organizational (3) (N)	% Difference 1-2	2-3	1-3
High	76.6 (209)	78.0 (59)	51.1 (45)	-1.4	26.9	25.5
Low	62.1 (58)	45.7 (46)	16.7 (54)	16.4	29.0	45.4
TOTAL	73.4 (267)	63.8 (105)	32.4 (99)	9.6	31.4	41.0

	Initial Motivation Low Type of Research					
Present Motivation	Disciplinary (1) (N)	Applied (2) (N)	Organizational (3) (N)	% Difference 1-2	2-3	1-3
High	75.3 (93)	73.3 (75)	35.3 (34)	2.0	38.0	40.0
Low	34.6 (52)	33.8 (71)	13.7 (117)	0.8	20.1	20.9
TOTAL	60.7 (145)	54.1 (146)	18.5 (151)	6.6	35.6	42.2

TABLE V.2

PERCENT OF SCIENTISTS WHO PUBLISH OFTEN BY ORGANIZATIONAL REWARDS FOR PUBLISHING, INITIAL MOTIVATION, AND PRESENT MOTIVATION

	Initial Motivation High				
	Organizational Rewards for Publishing				
Present Motivation	Present	(N)	Absent	(N)	% Difference
High	76.2	(185)	69.2	(130)	7.0
Low	65.9	(41)	33.3	(120)	32.6
TOTAL	74.3	(226)	52.0	(250)	22.3

	Initial Motivation Low				
	Organizational Rewards for Publishing				
Present Motivation	Present	(N)	Absent	(N)	% Difference
High	76.3	(97)	60.4	(106)	15.9
Low	43.6	(39)	21.1	(204)	22.5
TOTAL	66.9	(136)	34.5	(310)	32.4

TABLE V.3

PERCENT OF SCIENTISTS WHO PUBLISH OFTEN BY COMPETING ALTERNATIVES, INITIAL MOTIVATION, AND PRESENT MOTIVATION

Initial Motivation High

	Competing Alternatives				
Present Motivation	Absent	(N)	Present	(N)	% Difference
High	72.8	(246)	75.4	(69)	-2.6
Low	47.9	(117)	25.0	(44)	22.9
TOTAL	64.7	(363)	55.8	(113)	8.9

Initial Motivation Low

	Competing Alternatives				
Present Motivation	Absent	(N)	Present	(N)	% Difference
High	71.8	(149)	57.4	(54)	14.4
Low	28.1	(146)	19.6	(97)	8.5
TOTAL	50.2	(295)	33.1	(151)	17.1

motivation to publish professionally was <u>low</u>. And on the strength of this we may tentatively conclude that those scientists whose initial motivation to publish was not particularly strong were more susceptible to having their motivation <u>reduced</u> by organizational conditions which were, from the standpoint of the discipline, unfavorable for publishing: lack of disciplinary control over research, lack of organizational rewards for research, and the presence of role alternatives that compete with the research role.

<u>Mobility</u>.

It is considerably more difficult to adequately control for mobility than for initial motivation. Ideally, we should have a scientist's reasons for every move from one work organization to another or from one role to another within a work organization. Additionally, we should have some indication of the extent to which each work organization displayed each of the characteristics with which we are presently concerned; only then could we determine unequivocally whether a move was initiated to avoid or to seek out an organization with certain characteristics. As already indicated, however, the questionnaire did not go into such detail. The scientists were asked to "describe the two full-time professional scientific positions just prior to your present one, including positions you have held with your present employer if they involved work substantially different from your duties now." Where applicable, they were also asked to describe their first full-time professional position after receiving their last degree. Blanks permitted them to indicate only type of employer, principal duties, and dates of employment. And it is from this information that the characteristics of the work organizations in which a scientist had spent his career have been deduced.

On the assumption that those who had moved into substantially different types or organizations may have been avoiding or seeking specific organizational characteristics, the sample was divided into two groups: those who had remained with the same employer or in substantially the same _type_ of work organization for their entire careers or whose previous job was the same as their present were regarded as _non-movers_; those who had _changed_ the _type_ of employer were regarded as _movers_. We reasoned that the movers, since they had changed their work organizations would be less likely than the _non-movers_ to change their _motivation_ in the face of those organizational characteristics that were not conducive to disciplinary participation. Conversely, those who had not changed their organizations would be more likely to change their motivation to participate when faced with these same conditions.

Employers were regarded as being of the same _type_ if all fell into one of two "homogeneous" groups: academic institutions, medical schools, or a non-profit research agency on the one hand; and on the other, federal, state, or local government, hospitals, clinics, or self employed.[36] Scientists were regarded as "movers" if their present employer was in either one of the homogeneous groups and their previous employer was in another; and as "non-movers" if their present and former employer was in the same group.

Tables V.4-V.6 present the partial relationships between publishing and motivation for movers and non-movers for each of the three organizational characteristics. As expected, the partials are reduced only for those scientists classified as non-movers for competing alternatives, Table V.6, and in five out of six cases for type of research, Table V.4. This seems to indicate that those scientists who were confronted with lack of control over the ends of their research or with competing alternatives and who did <u>not</u> move from one type of

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[36]Defining these "homogeneous" groups is a crude expedient which no doubt glosses over a number of important organizational differences including, of course, the organizational characteristics of interest here. In this respect it violates a tacit assumption of this study: that it is more meaningful to classify organizations according to dimensions or attributes that are relevant for a particular problem or issue, e.g., factors affecting disciplinary participation, than to ritualistically utilize the received taxonomies embedded in common language, e.g., industry, universities, etc.. Nevertheless, classifications such as this are quite common in the literature and do appear to group organizations along some fairly meaningful dimensions, for example, an emphasis on basic research. Our own data revealed that the academic medical school and non-profit research agencies were much more likely to contain scientists performing disciplinary research than were governments, hospitals, or the self-employed.

TABLE V.4

PERCENT OF SCIENTISTS WHO PUBLISH OFTEN, BY TYPE OF RESEARCH MOTIVATION TO PUBLISH, AND CAREER MOBILITY

MOVERS

Motivation	Type of Research: Disciplinary (1)	Applied (2)	Organizational (3)	% Difference: 1-2	2-3	1-3
High	76.9 (52)	80.8 (26)	46.7 (15)	-3.9	34.1	30.2
Low	44.4 (27)	30.4 (23)	20.8 (19)	14.0	9.6	23.6
TOTAL	65.8 (79)	57.1 (49)	35.3 (34)	8.7	21.8	30.5

NON-MOVERS

Motivation	Type of Research: Disciplinary (1)	Applied (2)	Organizational (3)	% Difference: 1-2	2-3	1-3
High	76.0 (250)	74.1 (108)	43.8 (64)	1.9	30.3	32.2
Low	50.6 (83)	40.4 (94)	13.6 (127)	10.2	26.8	37.0
TOTAL	70.9 (333)	58.4 (202)	25.1 (191)	12.5	33.3	45.8

TABLE V.5

PERCENT OF SCIENTISTS WHO PUBLISH OFTEN BY ORGANIZATIONAL REWARDS FOR PUBLISHING, MOTIVATION TO PUBLISH, AND CAREER MOBILITY

Movers

	Organizational Rewards for Publishing				
Motivation	Present	(N)	Absent	(N)	% Difference
High	75.9	(245)	64.0	(201)	11.9
Low	53.9	(65)	25.6	(285)	28.3
TOTAL	71.3	(311)	41.8	(486)	29.5

Non-Movers

	Organizational Rewards for Publishing				
Motivation	Present	(N)	Absent	(N)	% Difference
High	78.9	(57)	66.7	(42)	12.2
Low	52.1	(19)	25.4	(55)	26.7
TOTAL	72.4	(76)	43.3	(97)	29.1

TABLE V.6

PERCENT OF SCIENTISTS WHO PUBLISH OFTEN, BY COMPETING ALTERNATIVES, MOTIVATION TO PUBLISH AND CAREER MOBILITY

Movers

	Competing Alternatives				
Motivation	Absent	(N)	Present	(N)	% Difference
High	72.5	(334)	71.8	(112)	.7
Low	34.4	(230)	28.2	(120)	6.2
TOTAL	56.9	(564)	44.4	(233)	12.5

Non-Movers

	Competing Alternatives				
Motivation	Absent	(N)	Present	(N)	% Difference
High	75.0	(80)	68.4	(19)	6.6
Low	41.7	(48)	15.4	(26)	26.3
TOTAL	62.5	(128)	37.8	(45)	24.7

organization to another lowered their motivation to publish and hence the frequency with which they published. Evidently, however, the difference between disciplinary and organizational control over research (col. 1-3) is sufficiently powerful to affect the motivation of those who moved from one type of work organization to another as well as those who did not move. Similarly, when we contrast scientists in organizations that reward publishing with those that do not, Table V.4, we find that the partials are also reduced for both movers and non-movers. Apparently the effect of an organizational reward policy regarding professional publishing is, like the difference between disciplinary and organizational control, potent enough to affect the motivation of movers and non-movers alike.

The data in Tables V.1-V.6 tend to corroborate the hypothesis that the organizational characteristics considered here change participation by changing motivation. However, the possibility that mobility rather than change in motivation was the underlying mechanism cannot be entirely disregarded in the fact of the rather loose control for this characteristic.

Additional data, however, provide some further evidence that the characteristics of the organizations with which we have been concerned have actually changed motivation to participate in the discipline. The scientists ranked six scientific career objectives[37] in terms of their importance

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[37]The six objectives are the same as those described above in the section on Initial Recruitment.

to them at two separate times: when they received their last degree and at the present time. Among the career objectives that the scientists ranked was, "Doing research contributing to basic knowledge in science"; the rank order given to this objective provides an additional measure of motivation to do basic research and presumably also to publish professionally. With due allowance for possible unconscious or deliberate distortions of memory, any difference between the rank of this objective at the two points in the scientist's career would provide a measure of change in motivation. Although at the time of this writing the most appropriate statistical technique for analyzing data in this form has not been worked out, a rough descriptive index sufficient to at least indicate a trend can be constructed from the mean rank of this objective at the two time intervals.

These mean ranks and the differences between them are displayed for each organizational characteristic in Tables V.7-V.9. In order to detect any change over time the samples have also been stratified by the length of time since receiving the last degree.

Perhaps the most notable trend is that in every case the difference between the mean rank at first degree and at the present time is greater under those organizational conditions that were predicted to lead to change in motivation. Thus the apparent change in basic research as a career objective

TABLE V.7

MEAN RANK OF BASIC RESEARCH AT PRESENT AND AT TIME OF HIGHEST DEGREE, BY TYPE OF RESEARCH AND YEARS SINCE LAST DEGREE

	TYPE OF RESEARCH							
	Basic				Applied or Organizational			
Years Since Last Degree	Last Degree	Present	Difference	(N)	Last Degree	Present	Difference	(N)
0-5	1.91	1.74	-.17	(148)	2.46	2.84	.38	(178)
6-10	1.69	1.55	-.14	(91)	2.56	2.99	.43	(110)
11-15	1.84	1.74	-.10	(92)	2.51	3.16	.65	(102)
16-20	2.19	2.21	.02	(48)	2.38	3.27	.89	(63)
20 plus	2.09	2.36	.27	(59)	3.29	3.85	.56	(94)

TABLE V.8

MEAN RANK OF BASIC RESEARCH AT PRESENT AND AT TIME OF HIGHEST DEGREE BY ORGANIZATIONAL REWARDS FOR PUBLISHING, AND YEARS SINCE HIGHEST DEGREE

	ORGANIZATIONAL REWARDS FOR PUBLISHING							
	Present				Absent			
Years Since Last Degree	Last Degree	Present	Difference	(N)	Last Degree	Present	Difference	(N)
0-5	1.80	1.74	-.06	(136)	2.50	2.77	.27	(190)
6-10	2.05	2.10	.05	(87)	2,24	2.52	.27	(114)
11-15	2.18	1.86	-.32	(79)	2.20	2.91	.71	(115)
16-20	2.19	2.24	.05	(42)	2.36	3.16	.80	(69)
20 plus	2.98	3.04	.06	(46)	2.76	3.37	.61	(107)

TABLE V.9

MEAN RANK OF BASIC RESEARCH AT PRESENT AND AT TIME OF HIGHEST DEGREE, BY COMPETING ALTERNATIVES, AND YEARS SINCE HIGHEST DEGREE

	COMPETING ALTERNATIVES							
	Present				Absent			
Years Since Last Degree	Last Degree	Present	Difference	(N)	Last Degree	Present	Difference	(N)
0-5	2.01	2.07	.06	(237)	2.74	3.07	.33	(89)
6-10	2.15	2.26	.11	(149)	2.21	2.58	.37	(52)
11-15	2.20	2.29	.09	(137)	2.18	2.97	.79	(57)
16-20	2.35	2.64	.29	(74)	2.19	3.16	.97	(37)
20 plus	2.56	2.91	.35	(105)	3.40	4.08	.68	(48)

has occurred in applied or organizational work organizations, i.e., where disciplinary control over research was least; where scientists received no organizational rewards for publishing; and where scientists were exposed to competing role alternatives. Furthermore, under these less "favorable" organizational conditions the differences generally increase with the length of time from the degree until the last period due in large measure to the fact that the mean rank of basic research at the present time decreases steadily over time for the "unfavorable" conditions. This suggests that exposure to such conditions over time may have a cumulative effect on changes in motivation. On the other hand, no clear pattern of differences is apparent for those scientists in work organizations with more "favorable" conditions.

In addition, it appears from the negative difference of the means in Column 3 of Tables V.7 and V.8 that at least initially the effect of those organizational characteristics favorable to disciplinary participation is to increase motivation to do basic research.

The effect of initial recruitment can be seen by comparing columns 1 and 3 in each table. In every case the disciplinary researchers at the time of their first degree ranked basic research higher as a career goal than did their applied or organizational research colleagues (Table V.7). The same is true for organizational rewards for publishing for all but those scientists with the greatest professional age (Table V.8), and for three of the five age groups in Table V.9 where the affect of competing alternatives is considered. These differences for the three measures may indicate that the different organizational characteristics varied in their visibility or initial relevance to alternative career goals. At this point it is impossible to say how much of the differences between the rank of basic research at the two time intervals is attributable to this factor.

The possible effect of later mobility may possibly also be seen in the differences in the mean ranking which, except for the disciplinary researcher where the pattern is not clear, decrease (i.e., rank lower) with professional age under "favorable" conditions and increase (rank higher) up to the oldest age group under less favorable conditions. Such an effect could be produced by movement of the less motivated scientists out of research when exposed to "unfavorable" conditions, and a movement of some more highly motivated scientists out of research in the face of favorable conditions. Why some of the more highly motivated should leave research, if in fact they do, would be an important point for further investigation.

Here too, in evaluating the trend toward changes in motivation, it is necessary to bear in mind that a potential mobility effect has not been removed; it is conceivable that those who were likely to change their motivation may have been left in organizations having unfavorable conditions while those most likely to change either left or were ejected from work organizations with favorable conditions.

Conclusions

This chapter has focused on changes in the motivation of scientists to publish induced by various characteristics of the organizations in which they work. Consequently it has dealt with only one of the possible processes that may lead to a correlation between characteristics of individuals and characteristics of organizations. In general, the weight of evidence seems to permit the conclusion that some discernable portion of the correlation between work organizational characteristics and publishing is caused by changes in motivation brought about by exposure to organizational characteristics.

I have not argued that motivational changes were either more pervasive or more important than factors influencing an initial or later match between scientists and organizations; the present data do not lend themselves to such assertions. In fact, any fair assessment of the size of the percentage differences would suggest that changes in motivation are of relatively less importance than recruitment, accounting for the relationship between organizational characteristics and

publishing frequency. I must leave to others with better data the difficult task of weighing the relative importance of these three processes by which some correlation is established between the professional proclivities of scientists and conditions of their work organizations that aid or hinder disciplinary participation.

Similarly, other methods and measurements, more accurate and sensitive than those employed in this study, will be required before the relative impact of the various organizational factors on disciplinary participation can be determined.

And finally, it is worth noting that this chapter has not provided a test of the assumption that disciplinary participation may have changed independently of motivation since we have no retrospective questions that specifically measured earlier levels of publishing.

CHAPTER VI

SUMMARY AND CONCLUSIONS

The first part of this final chapter gathers together and highlights in summary form the major theoretical and empirical conclusions of the dissertation. This should provide perspective on the dissertation as a whole, making it easier to assess what we have learned, and, disconcertingly, what we have not learned; it will serve also as a backdrop for suggestions for additional analysis and research which will be taken up in the last part of the chapter.

Summary of Theory

The dissertation began by noting some of the structural changes that have reshaped the institution of American science in the first six decades of this century. One of these changes was the shift in the distribution of scientific manpower among different kinds of work organizations; most obviously, a shift from organizations primarily concerned with creating and disseminating new information to those concerned with transforming it into some useable form.

This dissertation has been directed toward understanding how this shift in its organizational base may have affected the larger institution of science. To this end it has focused on a single important relationship between the two principal components of science, scientific work organizations and scientific disciplines. The central question bearing on this concern toward which this dissertation was directed asked: <u>in what ways can participating in different kinds of work organizations affect the extent to which scientists participate in their disciplines</u>?

It was necessary first to construct an appropriate conceptual framework which could be made to yield the theoretical and, eventually, the empirical tools necessary to fashion an adequate answer to this question. Such a framework, I felt, would need to relate the disciplinary behavior of scientists more closely to varying situational or contextual attributes of scientific work organizations than to the overarching moral norms and values which traditionally have been the basis for explaining scientific behavior. Secondly, in the interests of parsimony, I wanted a framework that would embrace relevant facts concerning scientific work organizations and disciplines without it being necessary to treat them as different empirical species, each requiring an array of different concepts and processes. Briefly then, I needed a framework that would meet two overriding criteria: it should relate concrete variables of work organizations to the disciplinary participation of scientists; and secondly, it should permit the mapping of both work organizations and disciplines into a relatively small set of common theoretical ideas.

Combining and adapting elements of role and exchange theories provided a framework that met these requirements. From the standpoint of role theory, the institution of science was conceived as having two distinct but overlapping sets of social structures or role sectors: the organizations where scientists carry out various research, teaching and administrative activities--the <u>work organizations</u>; and that larger arena of activity and association among scientists who share a common theoretical and substantive concern--the scientific <u>disciplines</u>--such as microbiology, mathematics, molecular biology, etc.. Drawing upon Sim's formal analysis of the role concept, both <u>structures</u> were defined as consisting of particular kinds of behavioral <u>relationships</u> between social positions. Sim's formulation was modified, however, to permit any given incumbent's behavior <u>in relation</u> to any other position to vary from zero to some maximum, a convention which, among other things, made it possible to speak of scientists as more or less involved in either structure, thus accommodating the considerable known variability in scientific behavior.

This bare structural framework was then linked to exchange theory by formally defining the behavioral relationships as exchanges of differentially valued commodities or social goods between the incumbents of the positions. Put generally, and hence somewhat inaccurately, in this framework scientists as incumbents of work organizations are regarded as exchanging their compliance with the directives of the organization in exchange for various organizational rewards; and as incumbents of the disciplinary structure they are regarded as exchanging scientific information with their colleagues in return for some form of recognition. The exchange structures of the work organizations and of the disciplines are distinguishable from one another by virtue of the kinds of commodities exchanged within them. And each individual scientist participates in each structure in greater or lesser degree.

In this view, furthermore, the organizational and disciplinary structures are themselves linked by virtue of the fact that every scientist is simultaneously an incumbent of both structures, related through exchanges to positions both in his work organization and in his discipline. And it is the fact of such a connection which makes it possible to specify some of the ways in which a scientist's participation in his work organization influences his participation in his discipline.

Inasmuch as the discipline approximates an exchange structure, a scientist's participation in his discipline depends in large measure on the extent to which his work organization (1) provides opportunities for him to <u>acquire</u> information that would be exchangeable in his discipline, and (2) influences his <u>motivation</u> to engage in disciplinary exchanges. Thus the framework makes it possible to ask some additional fairly specific and, most importantly, some <u>researchable</u> questions bearing on the central concern of the dissertation. It was now possible to ask: what are the characteristics of work organizations that affect a scientist's opportunities to <u>acquire</u> information exchangeable in his discipline, and what are the characteristics that affect his <u>motivation</u> to do so?

Regarding the first of these, the opportunities to <u>acquire</u> exchangeable information, we reasoned that exchangeable information, just like any other commodity, was directly or indirectly produced by applying certain definite <u>means</u> (knowledge, techniques, raw materials, facilities), towards certain definite <u>ends</u>. And hence scientists would be unlikely to acquire exchangeable information to the extent that in their work they lacked either the appropriate means or ends, whatever these might be. We were concerned with only the most general dimensions of the complex issue of precisely which means were most adequate for producing scientific information. Empirically the dissertation deals only with the perceived adequacy of the organization's research facilities from the point of view of the individual scientists, deriving the following hypothesis:

> Hypothesis 1. The less adequate are the facilities or means for research in an organization, the less will be the opportunities to acquire information that is exchangeable in the discipline, and hence the less will be the disciplinary participation of the scientists in that organization.

Because it seemed to be of greater intrinsic sociological interest, the problem of the appropriateness of the ends toward which research efforts might be directed received more attention. I reasoned that scientific researchers would be most likely to produce information that would be exchangeable in a discipline when they pursued problems and discoveries recognized in the discipline as important and valuable; and that they would be less likely to produce exchangeable information when they pursued problems that any alternative social system, such as a work organization, defined as valuable or important. As we saw, another way of saying the same thing is that information that would be exchangeable in a discipline will more likely be produced the more the discipline either directly or indirectly determines or controls the ends of research. Hence the second hypothesis:

> Hypothesis 2. The more an organization directs research toward its own ends rather than toward those of the scientific discipline, the less will be the opportunities to acquire information that is exchangeable in the discipline, and hence the less will be the disciplinary participation of the scientists in that organization.

Operationally, the degree to which the scientific discipline controlled research was determined according to whether the criteria for choosing research problems was that they should contribute to basic knowledge in science (disciplinary research), to practical applications of scientific knowledge (applied research), or to the problems of the work organization (organizational research).

On the basis of exchange theory, we reasoned that _motivation_ to engage in disciplinary exchanges was in part a function of the organizational reward system. Scientists would be likely to regard engaging in disciplinary exchanges as rewarding, that is, they would be _motivated_ to engage in them to the extent that _organizational_ rewards could be obtained by participating in the discipline. Two dimensions of the organizational reward system were considered: the extent to which continued employment and advancement depended on publishing professionally, and the extent to which the work organization provided role sectors that were alternative to research and that were relatively more rewarding than research. Hypotheses 3 and 4 dealt with these dimensions of the organizational reward system:

> _Hypothesis 3_. The less an organization rewards disciplinary participation, the less will be the motivation of its incumbents to participate in the discipline, and, hence, the less will be their disciplinary participation.

> _Hypothesis 4_. The more an organization offers alternative organizational role-sectors that

> are more rewarding than those in which they might acquire exchangeable information, the less will be the motivation of its incumbents to participate in the discipline, and, hence, the less will be their disciplinary participation.

And finally, we reasoned that motivation to participate in the discipline would also be a function of a scientist's estimate of his objective chances for achieving disciplinary recognition. And this estimate would be a function of those organizational factors affecting his opportunities to acquire exchangeable information--the adequacy of his research means, and the extent to which his research was directed toward disciplinary goals. Hence Hypothesis 5.

> Hypothesis 5. The less adequate are the facilities for research in an organization, or the more it directs research toward its own ends rather than to those of the discipline, the less will be the motivation of its incumbents to participate in the discipline, and, hence, the less will be their disciplinary participation.

Summary of Findings

The first three hypotheses were tested for five types of disciplinary participation but were consistently confirmed for only one of these, the extent to which scientists published professionally. We argued that the failure of the hypotheses to be sustained consistently for the other four types of disciplinary participation was probably because they measured a broader, less specific, sample of disciplinary behavior than did professional publishing; and, related to this, that these other forms of disciplinary participation were less directly related to the indexes of the organizational characteristics than was publishing. Consequently, the other Hypotheses were tested only for professional publishing.

Chapter V dealt with the problem of determining whether the observed relationships between the level of professional publishing and certain organizational characteristics had been produced by selective recruitment, selective mobility, or by actual changes in the behavior or motivation of the scientists. Hypothesis 6, based on the available literature and various theoretical arguments proposed:

> Hypothesis 6. Any relationship between organizational characteristics and disciplinary participation and motivation, such as those specified in Hypotheses 1-5, will be due, in part, to changes in a scientist's disciplinary participation or motivation over time.

Hypothesis 6 was tested by controlling for initial motivation and for movement to dissimilar organizations during a scientist's career, and also by examining ostensible changes in career goals over time under the relevant organization conditions. The results of these tests also tended to confirm Hypothesis 6, that work organizations did indeed appear to have changed the motivation of scientists to publish in their disciplines.

Three serendipitous findings emerged during the course of the analysis. The first of these was some evidence of what may be two fairly distinct forms of disciplinary motivations rather than the one that had been expected. One motivational form may correspond to a "moral" or "normative" orientation in which a scientist participates in his discipline, through publishing for example, because he believes it to be a normative component of his position or role as a scientist--it is, in short, what scientists do. The other form of motivation may represent a "calculative" or "expedient" orientation and scientists so motivated may participate in the discipline when the objective costs and rewards in the work organizations and the discipline make it ~~an~~ expedient to do so.

Secondly, no one examining the tables could fail to be impressed with the very sizeable relationship between every form of disciplinary participation and motivation to participate. Indeed, in the absence of any sophisticated multivariate technique which would determine it exactly, motivation appears to be the single best predictor of disciplinary participation, considerably more important in fact than any single organizational characteristic dealt with in the dissertation. I had tacitly assumed that organizational factors would have had a relatively greater impact on participation. We will return to this issue of the importance of motivation later in the chapter when we deal with suggestions for further research.

The third serendipitous result was that the data seemed to point to a view of scientific disciplines as having a number of separate exchange structures within which different sorts of information are exchanged between incumbents of a variety of positions. This is a possibility, however, that we were unable to pursue with the data presently available.

Conclusions and Limitations of the Study

These pages have suggested a partial answer to how different work organizations influence the disciplinary participation of scientists who work in them. In its most general form, stripped of essential qualifying detail, the answer essentially comes to this: work organizations reduce disciplinary participation when they (1) reduce a scientist's opportunities to acquire information that is exchangeable in his discipline, because the *means* they provide for producing it or the *ends* toward which they direct research are inappropriate; or (2) when they reduce a scientist's *motivation* to engage in disciplinary activities because the organization does not reward them. But how are we to judge the adequacy of this answer? Necessarily, of course, it is an answer whose substance and shape is ineradicably marked by the empirical materials and theoretical tools with which it was fashioned and by the necessary limitations in comprehensiveness and detail that were imposed upon it. Hence any assessment must concern itself with the theoretical framework, and with the issue of comprehensiveness, as well as with the empirical results.

I think it is obvious that the adequacy of the answer largely depends on how comprehensive an answer was wanted. No doubt the analysis of how work organizations affect disciplinary participation could have been extended almost indefinitely, simply by specifying further organizational conditions that represent additional instances of inadequate organizational means or ends, or of factors that make disciplinary participation less rewarding. We have not sought to specify many of the <u>means</u> which are more or less conducive to the production of exchangeable scientific information; we have dealt in considerably greater detail with which research <u>ends</u> are most productive of exchange information; and we have considered only three organizational factors that may affect motivation.

A careful search of the available literature would no doubt uncover a number of additional organizational factors that have an important impact on disciplinary participation or motivation. It has not been our purpose here to provide an exhaustive summary of these, but rather to provide a schema that meaningfully organizes and orders most of them.

I am asserting that any organizational factors that affect disciplinary participation must either be examples or further specifications of the means or ends whereby information is produced, or that they affect participation by affecting motivation to exchange information. In short, I am suggesting that the answer to our question is really an answer-outline into which later, more detailed answers can be fitted. It serves, therefore, as a device for meaningfully organizing what is potentially a rather sizeable array of organizational factors that affect disciplinary participation. Regarding the adequacy of the theoretical framework itself, no clear conclusions are possible. In light of the preceding remarks I would have to reassert my conviction that looking at the institution of science in terms of its exchange structures provides a reasonably parsimonious way of organizing a variety of facts concerning different work organizations and scientific disciplines. Perhaps a more important test is that it helped to generate some researchable hypotheses. It is a moot point at this time whether some alternative theoretical position could have generated the same or perhaps even a better set of hypotheses--one would first have to produce the alternative. The results so far seem to justify using the framework for further research.

As long as we are evaluating the theory it is perhaps unnecessary to point out what may already have become obvious to the reader: this research was not undertaken to deductively test some "theory", but rather to answer a question generated from empirical observation. This is an important distinction since the different purposes of research imply different evaluative criteria. In the deductive case the theory is more nearly an end in itself, empirical results reflect on the validity of the theory as it stands and on modifications of it that become necessary in light of the results. In such research the substantive findings are more nearly means to this end. When the research is undertaken to answer a substantive question, however, empirical results bear on the question and the theory is a means to that end. Hence I am more concerned at this point with how well the theory helped to answer the question concerning the relationship between work organization and disciplinary participation than I am with whether the theory is valid. Validity is necessarily implicated, of course, if not now then at some later point where the theory itself generates questions, where additional empirical questions continue to be pursued within its framework, or when the theory is applied to different substantive data.

Exchange theory is not yet a limited body of definite propositions upon which there is complete agreement. It differs in the degree to which the precise conditions under which exchanges take place have been specified--and ranges from specific axioms to sensitizing concepts. At each of these levels the theory is useful depending upon different degrees of precision, specificity, and detail in the data being dealt with. Underlying the various forms exchange theory assumes, however, is a common conviction that some forms of interaction can be regarded as exchanges of differentially valued behaviors.

This study treated interaction in scientific disciplines from this general point of view in terms of exchanges of information for recognition, and in adapting it to the present study appended to it the idea that such exchanges may be contingent upon the conditions under which information is produced. These conditions, necessary means and appropriate ends, are not part of exchange theory per se but are a logical consequence of tying exchanges of information in the discipline to dimensions of work organizations. The hypotheses concerning the organizational conditions under which individuals might increase or decrease their exchanges of information in the discipline as they become more or less rewarding are tied to a more specific form of exchange theory. Here too, assumptions about which particular organizational conditions would be more or less rewarding were not deduced from exchange theory but were suggested by the extension of exchange concepts to organizations.

The results of this study reflect favorably upon the utility of exchange theory as formulated here, but cannot reflect upon it as a quasi-deductive theory with the precise specification of variables that we find in Homans. Exchange theory in this form can best be handled under the controlled conditions of an experimental situation where the necessary precision in measurement can be more adequately dealt with. Ultimately, of course, the results of these experimental studies can give greater precision to exchange theory in its more general form.

Essential elements of the present perspective such as whether or not scientific behavior <u>really</u> corresponds to principles of exchange, of course, are with the present data beyond empirical assessment. Though, and here some of the bonuses of exchange theory may become apparent, it could be used to generate some fairly definite hypotheses regarding the mechanisms underlying shifts in motivation and crucial scientific career choices. To give one brief example; exchange theory would suggest that scientific motivation is in part a function of the differently valued alternative courses of action and the probability of achieving them that are present to an individual with a given location in the social structure. And it would suggest fairly obvious and specific research strategies and questions: scientists--or anyone else for that matter--could be asked to evaluate various alternatives to assess his chances for achieving them and such alternatives could be linked--in somewhat the same way that was attempted in this discussion--to objective structural variables (such as the presence or absence of organizational rewards, or the adequacy of means or ends). This, however, anticipates the later discussion of suggestions for additional research.

What I am suggesting, then, is that the present empirical results reflect less on the theory than on the question, and that the theory's adequacy must be determined more precisely at some later point with data generated specifically for <u>that</u> purpose.

This brings us then to the data: in what ways have the empirical results "answered" our question? This, of course, cannot be divorced from either the issue of comprehensiveness or of theoretical adequacy. As already indicated, we did not seek to deal exhaustively with all possible organizational factors affecting the relationship between the organization and disciplinary participation, so we cannot say that the results provide more than a partial answer. In general, if we disregard the four forms of disciplinary participation, other than publishing the hypotheses appear to have been largely confirmed.

Several important assumptions in the theory and hypotheses still have not been adequately tested. The first of these is that applied and organizational researchers published less than disciplinary researchers <u>because</u> they lacked exchangeable information as a result of being unable to direct their research

activity to appropriate ends, or that scientists with inadequate facilities publish less often <u>because</u> they lacked necessary means. In order to show conclusively that it was inadequate means or ends rather than some additional variable it would probably be necessary to determine whether the scientists themselves saw these restricting their disciplinary involvement below a preferred level.

Similarly, the important assumption that scientists lower their motivation to participate in the discipline because various organizational factors make disciplinary participation relatively disadvantageous, might be bolstered by data which indicated a scientist's awareness of the laternatives in his social milieu that are open to him, their relative importance or value, and the estimated probability of achieving them. These are crucial assumptions for the theoretical position and for the substantive answer but their validity must wait on further research.

Thus, though the answer of how organizations affect disciplinary participation appears plausible, it has yet to be completely tested at several strategic, and from the standpoint of the theory, crucial points. The controls for selective mobility and recruitment are a limitation in the data that contribute to the tentativeness of our conclusions. As the last chapter pointed out these controls were less than ideal and as a result the conclusion that organizational factors changed disciplinary motivation and hence disciplinary participation is, though highly likely in view of the results, not beyond challenge.

Suggestions for Further Research

Correcting these limitations in the present data comprises an important area for further esearch. I have already suggested, in a general way, some of the types of questions that might determine with greater accuracy the effect of organizational factors on research means, ends, and on disciplinary motivation. Additional research should attempt to provide better measures of the extent to which initial recruitment or mobility influences correlations between attributes of individuals and of organizations. More detailed job histories will be needed in order to determine what effect various organizational characteristics may have had on job mobility or on changes in disciplinary motivation or participation. Such histories should attempt to ascertain the extent to which the organizations in which the scientists have worked possessed the characteristics presently under consideration, and whether these were involved in decisions to move from one job to another. This will also be necessary in order to effectively use one important control which, because we lacked this data, we were unable to use in the present study. Thus we should expect that the hypothesized effect of various organizational factors would increase with length of exposure to them. And this requires, for the considerable number of scientists who have moved in their careers, that we have this data on all previous work organizations.

Short of a longitudinal study, additional data will also be required in order to place the imputation of <u>change</u> in motivation or participation on firmer ground. An independent measure of past publishing frequency, and restrospective questions concerning disciplinary motivation at various career points, will need to be combined with detailed descriptions of work organizations at various career points in order to better substantiate the link between motivation, participation, and organizational characteristics.

The questions concerning the other forms of disciplinary participation will have to be cleared up if the proposed model of the institution of science is to apply todisciplinary behavior in general. Since I suspect at this point that the measures of these other forms of disciplinary participation rather than the exchange model itself is at fault, attempts to devise better indexes of participation should receive top priority. As suggested, the present measures are probably too broad, encompassing many different sorts of participation. Consequently, the indexes should be much more specific--they should determine who was contacted, for what purpose, how frequently, and every effort should be made to ascertain if an exchange was made, what social goods were involved, and the relative value of the exchange to each party. Needless to say, obtaining such data will require considerable ingenuity and finesse. With these refinements in data it should also be possible to subject to more refined test the assumption that the scientific discipline can more accurately be represented as having a number of separate exchange structures determined by the different sorts of commodities exchanged in each.

Also of utmost priority in future research, should be attempts to measure the various forms of disciplinary participation in such a way that statistical techniques more powerful than contingency tables can be applied to the analysis. The two preceding chapters have made me acutely aware of the limitations of measuring publishing in terms of categorical "highs" and "lows". Although it would have been tempting to regard frequency of publishing as reported by each individual scientist as a continuous variable rather than as two categories, this would have imparted a spurious authenticity to powerful statistical manipulations. In the first place, it's possible that scientists would not be able to remember the number of their publications with sufficient accuracy to justify this procedure. In the second place, this would assign equal weight to publications that undoubtedly vary widely in importance on the basis of their significance for others and in terms of the prestige of various publication outlets. Instead I adopted the more conservative approach of collapsing the number of papers at the median for different professional-age cohorts. And while this is a less refined measure of participation than might have been obtained, it has the virtue of not claiming to be more than a rough index of one form of disciplinary participation.

Asking for the number of papers published over a three year interval rather than some more accurate technique was necessitated by the size of the sample and by the limitations of a mailed questionnaire. Other studies using interviewing or records could overcome many of these limitations. And the new techniques of measuring the importance of a published work by the frequency with which it is cited in the literature could overcome some of the difficulty of assigning equal weight to all publications and would provide a good measure of the value that any publication had to a given discipline. An index combining frequency and importance might prove to be ideal in this case.

There is another extremely important reason for emphasizing improvements in measuring disciplinary participation: it would permit a multivariate approach to determining the impact of organizational factors on disciplinary participation. For example, in the present study it would have been advantageous to be able to determine the relative, as well as the joint, importance of various organizational factors in determining levels of motivation and disciplinary participation.

One avenue for further research opened up by the present study concerns the very important role of motivation in scientific behavior. One could hardly fail to notice the impressive role of motivation on levels of disciplinary participation in all tables in the preceding two chapters. These impressively large and consistent motivational effects suggest that prior socialization has a particularly significant influence on scientific behavior as some recent studies have begun to show.[1] Fortunately, data are available in the present study that will permit us to follow up this lead.

The other important facet of scientific motivation for further investigation concerns the findings in this study which seem to suggest moral and expedient modes of disciplinary motivation. The two modes, if genuine, are likely to be associated both with length of training and institutional prestige. We would expect, for example, that the

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[1]For example see especially, Barney G. Glazer "'Differential Association' and the Institutional Motivation of Scientists", The Administrative Science Quarterly, Vol. 10, No. 1, (June 1965), pp. 82-97; and Diana Crane, "Scientists at Major and Minor Universities: A Study of Productivity and Recognition", American Sociological Review, Vol. 30, No. 5 (October 1965), pp. 699-714.

moral orientation would be imparted to those who had been socialized for the longest time into the scientific role and to those from the more prestigious schools. This relationship can be tested with present data by exploring fully whether the difference between high and low disciplinary motivation does in fact correspond to these background characteristics. Whether high and low motivation, as measured here, do in fact reflect qualitatively different forms of motivation must wait upon the more accurate measures of a moral or calculative motivation. This might possibly be approached from the standpoint of differing conceptions of the scientific role; whether, for example, highly motivated scientists see adequate performance of the role scientist as necessarily involving disciplinary participation.

There were some definite significant advantages to using a survey in the present study. In the first place, it provided a relatively fast and inexpensive means of gathering data on a large number of scientists in a variety of differing and widely scattered work organizations. Consequently, it provided a good cross section of a single scientific discipline in all its diversity and variety, and hence was admirably suited as a means for getting a glimpse of the distribution of activities and work settings underlying a single discipline. At the same time, however, against these advantages the survey method imposed several rather serious limitations. One of these, already touched upon, was the fact that because so many different kinds of work settings were being sampled, many questions needed to be quite general, questions which would be answerable by scientists in every sort of work setting. This trend toward general questions was heightened by the exploratory nature of much of the study. Lacking information on the variety of work organizational settings that might be encountered in the sample it was impossible to design many specific check list items, and at the same time it was necessary to measure a number of possible organizational factors. The result was that on a number of crucial variables, e.g., the four other forms of disciplinary participation, our measures were not specific enough. In light of what has been learned from the present study, however, many of these limitations could be easily corrected in any subsequent survey.

A far more serious limitation, however, is inherent in all attempts to study organizations by survey methodology. This is the fact that the survey cuts the researcher off from the organizations and imposes the considerable handicap of forcing him to evaluate results and responses out of context, as disembodied answers to questions rather than as organizational features which acquire substance, significance, and meaning in relation to other organizational features and to the organizational members' frames of reference. Surveys of this sort filter the process and significance of social relationships through questions that are not grounded on an intimate knowledge of the organizations. As a result, many questions and indexes lack a clear behavioral referent and it becomes difficult to interpret what various work organizational conditions mean for the scientists involved in them. It is this lack of contact with outgoing social processes which gives such an air of lifeless abstraction to much of the preceding analysis. It is a lifelessness that should be minimized in future studies which seek, as they should, to peer beneath a surface of cold statistics to a deeper reality of behavior and meanings which alone can yield the proper context for interpreting sociological data in all forms. Consequently, I would recommend that further studies, initially at least, be conducted in situ where the work organizations can be observed first hand. If this means a sacrifice in breadth it will be more than made up in depth.

This study was begun in the hope that the information it developed could serve as a basis for some relatively firm policy recommendations concerning the impact of various work organizational settings on the effective utilization of the talents of the scientific labor force. I do not feel that what has been learned here is yet on firm enough ground to warrent specific recommendations. This study has highlighted the importance of maintaining high levels of disciplinary motivation and participation, and the important role the work environment plays in this respect. Kornhauser's work suggests that many industrial work organizations are beginning to recognize the necessity of their research talent remaining integrated in the disciplinary reward and motivational system if they are adequately to perform their organization functions. These organizations see this in terms of maintaining the professional competency of their researchers and some are beginning to restructure their reward systems so that organizational rewards can accrue to scientists without their deserting research for management.

I hope that this study, by focusing on the dynamic interplay of work organizations and scientific disciplines, has contributed to a better understanding of connections such as these between work settings and disciplinary involvement. I hope also that the limitations and shortcomings have helped to point up the need for additional theoretical and empirical work on factors bearing on the effective deployment of scarce scientific talent in an era of accelerating demand and rapid changes in the work settings of science.

APPENDIX

SCIENTIFIC MANPOWER STUDY

CONFIDENTIAL

Although an identification number appears on the questionnaire for control purposes, you do not need to identify yourself by name. The data will be punched on IBM cards and will be reported in aggregate rather than individual terms. Although we do not anticipate that you will have any objection to the kind of questions being asked, all information will be confidential.

Most questions can be answered with a checkmark, number, or letter; for the remainder, one or two words are sufficient. Because of the question format and because a variety of people will be responding, you may find that a few questions do not fit your situation exactly. Nevertheless, please select the answer that comes <u>closest</u> to your own views or situation. Since all questions are interrelated, it is essential that you answer every one that applies to you.

1. Are you: (check as many as apply)

( ) Employed full-time?
( ) Employed part-time?
( ) Unemployed, not seeking employment?
( ) Unemployed, seeking employment?
( ) Student, full-time?
( ) Student, part-time?
( ) Retired?
( ) Other? ________ (specify)

> NOTE: IF YOU ARE A FULL-TIME STUDENT, COMPLETE QUESTIONS 1-9 ONLY.

2. Do you regard yourself as primarily in:

( ) Microbiology ( ) Other? ________ (please specify)

3. Indicate the academic degrees that you have earned. (do not include honorary degrees)

	Year Received	Institution
( ) B.A. or B.S.	____	________
( ) M.A. or M.S.	____	________
( ) Ph.D.	____	________
( ) Sc.D.	____	________
( ) M.D.	____	________
( ) Other ____ (specify)	____	________

4. What is your age? ________

5. Are you ( ) Male? ( ) Female?

6. What is the name of the institution where you have your <u>major</u> employment?

____

7. How would you describe it? (Please be fairly specific, e.g., private university, county hospital, drug firm, state diagnostic lab., etc.

____

8. How many years have you worked for your present employer? ________

9. What is your job title? ________

____

10. How many scientists other than technicians or students do you ordinarily supervise? __________

11. How many of each of the following do you ordinarily work with?

_____ Microbiologists? _____ Scientists other than microbiologists?

_____ Technicians?

_____ Graduate students or fellows?

_____ Other? __________ (specify)

12. What are your primary duties or responsibilities? (Please be fairly specific, e.g., product control, basic research, teach, etc.)

____

13. Is this the sort of work that you prefer to do? ( ) Yes ( ) No
If "No," what sort of work would you prefer?

____

14. When you first began with your present employer, what sort of work did you expect to be doing by now? (e.g., same as above, applied research, etc.)

____

15. Taking everything into account, how satisfied are you with your present job?

( ) Very satisfied ( ) Somewhat satisfied ( ) Not sure ( ) Somewhat dissatisfied ( ) Very dissatisfied

16. What are your plans regarding your current employment?

( ) Definitely stay ( ) Probably stay ( ) Not sure ( ) Probably move ( ) Definitely move

17. Describe the two full-time professional scientific positions just prior to your present one. Include positions you have held with your present employer if they involved work substantially different from your duties now.

Type of Employer:	Principal Duties:	
______________	______________	19__ to 19__
______________	______________	19__ to 19__

18. If not covered in the preceding question, describe your first full-time professional position after receiving your last degree.

______________ ______________ 19__ to 19__
(Type of employer) (Principal duties)

19. Below are 6 common career objectives of scientists. In the first column RANK each objective in terms of its importance to you WHEN YOU RECEIVED YOUR LAST DEGREE; then in the second column RANK these in importance to you NOW.

> Use a "1" for the objective that is/was MOST IMPORTANT to you, a "2" for the NEXT MOST IMPORTANT, and so on for the 3rd, 4th, 5th, and 6th most important objectives. Be sure to use each number only once per column.

(Read over all of the statements before responding.)	RANK IMPORTANCE: At Last Degree	Now
a. Do research contributing to basic knowledge in science.		
b. Do research having practical applications in some area of science.		
c. Use existing scientific knowledge or techniques in some area of applied science, like quality control, clinical work, etc.		
d. Manage or administer research or development.		
e. Manage or administer other than research or development.		
f. Teach or work with students.		

20. Which two of these objectives do you think most of the scientists with whom you come in contact where you work regard as most important?

> Place the corresponding letter from the objectives above in the appropriate spaces.

Most important _____. Next most important _____.

21. Which two objectives do you think your employer regards as most important as far as your continued employment or advancement is concerned?

Most important _____. Next most important _____.

22. The achievement of which two objectives do you most admire in other scientists whether in your organization or not?

Most important _____. Next most important _____.

23. On the whole, how would you rate most scientists <u>with whom you most often come in contact</u> where you work?

( ) Among the very best ( ) Much better than average ( ) Somewhat better than average ( ) About average ( ) Below average

24. On the whole, how would you rate yourself compared to all other scientists with similar training and experience in your line of work or special area?

( ) Among the very best ( ) Much better than average ( ) Somewhat better than average ( ) About average ( ) Below average

25. In the space provided indicate the extent of your agreement or disagreement with <u>each</u> of the statements below.

Use the following rating: 1 = Strongly Agree; 2 = Agree; 3 = Neither Agree, nor Disagree; 4 = Disagree; 5 = Strongly Disagree.

a. ____ If I received an inheritance so large that I did not have to work, I would still work at my present job.

b. ____ The things I do off the job are generally more interesting to me than the things I do while at work.

c. ____ It is more important to me that I do well at my work here than at anything else I do.

d. ____ I care more about what the people I work with think of me than I do about what most others think.

26. How would you rate the research facilities and equipment where you work?

( ) Among the best anywhere ( ) Very good ( ) Good ( ) About average

( ) Somewhat inadequate ( ) Very inadequate

27. Where you now work, how much does your advancement or continued employment depend on your publishing professionally?

( ) Very much ( ) Much ( ) Somewhat ( ) Little ( ) Very little or none

28. To what degree does your employer provide time off, reimbursement for travel, etc., for attending or participating in scientific conferences, or meetings?

( ) Very much ( ) Much ( ) Somewhat ( ) Little ( ) Very little or none

29. Do you think that your work has received the recognition it deserves from:

a. your employer? ( ) Yes ( ) No

b. scientists outside your organization or institution? ( ) Yes

( ) No ( ) Doesn't apply

30. In the space provided, indicate how good the opportunities are where you are now employed for advancing your career through each of the following

Use the following rating: 1 = Very good, 2 = Good, 3 = Poor, 4 = Very poor, 5 = Don't know.

a. ____ Teaching or working with students.

b. ____ Managing or administering of other than research or development.

c. ____ Managing or administering research or development.

d. ____ Using existing scientific knowledge or techniques in some area of applied science, like quality control, clinical work, etc.

e. ____ Doing research having practical applications in some area of science.

f. ____ Doing research contributing to basic knowledge in science.

31. On the average, about how many hours per week do you spend:

Working on research? ____ Administering research? ____

Supervising research? ____

32. If you do not either do, supervise, or administer research, how many years has it been since you have done any of these? _____

IF YOU DO NOT WORK ON, SUPERVISE, OR ADMINISTER RESEARCH, GO ON TO QUESTION 44.

33. How would you characterize most of the research in which you are involved? (e.g., basic, developmental, methodology, etc.)

____

34. How often do others besides yourself have any say in choosing research problems in which you are involved?

( ) Always ( ) Often ( ) Sometimes ( ) Rarely

( ) Never (IF NEVER, GO ON TO QUESTION 38)

35. Consider the one individual or group besides yourself who has the biggest say in choosing research problems in which you are involved: what is his relation to you? (e.g., research director, colleagues in my lab, etc.)

____

36. Just to make sure that we know who this individual is, is he a (check as many as apply)

a. scientist? ____ Yes, ____ No, ____ Don't know

b. research administrator or supervisor? ____ Yes, ____ No, ____ Don't know

c. an administrator of other than research? ____ Yes, ____ No ____ Don't know

37. For the most part, how much weight do your ideas have when it comes to choosing research problems in which you are involved?

( ) Very much ( ) Much ( ) Some ( ) Little ( ) Very little or none

38. For research in which you are involved, what is the rank importance of the following criteria as far as selecting research problems or deciding to continue on-going research is concerned?

RANK the criteria using a "1" for the most important, a "2" for the next most important, and a "3" for the least important.

	RANK
a. The chance that it may contribute to services or products provided by my employer or client.	
b. The chance that it may contribute to developments or discoveries that have important practical applications.	
c. The chance that it may contribute to basic knowledge in science.	

39. How often is the length of time that may be required to complete research a factor in selecting a research problem or in deciding to continue with on-going research?

( ) Always ( ) Often ( ) Sometimes ( ) Rarely ( ) Never

40. What is the usual length of research problems in which you are involved?

( ) ½ year or less ( ) ½ to 1 year ( ) 1 to 2 years ( ) 2 to 3 years ( ) 3 to 5 years ( ) Over 5 years

41. In research in which you are involved, to what degree is it possible to:

a. pursue leads or developments that may lead away from the original research problem?

( ) Very much ( ) Much ( ) Little ( ) Very little

b. make use of the very latest developments in your own or in a closely related field?

( ) Very much ( ) Much ( ) Little ( ) Very little

c. be free from restrictions or secrecy in publishing research findings or results?

( ) Very much ( ) Much ( ) Little ( ) Very little

42. When beginning research in which you are involved, how sure are you usually of what the outcome or results will be?

( ) Very sure ( ) Fairly sure ( ) Neither sure nor unsure

( ) Fairly unsure ( ) Very unsure

43. Are you provided with time off from your normal research or other duties to work on research of special interest to you?

( ) Yes ( ) No ( ) Not applicable

If "Yes" about what percentage of your time per week do you have free? _____

What percent of this free time do you usually use in this way? _____

44. About how often do you contact other scientists from OUTSIDE your organization or institution specifically for help, advice, or information on matters bearing on your work?

( ) Once a week or more ( ) Two or three times a month

( ) Once a month ( ) Less than once a month ( ) Never

45. Do (or would) you usually find such contacts:

( ) Very valuable? ( ) Valuable? ( ) Somewhat valuable?

( ) Not valuable? ( ) A waste of time?

46. About how often do scientists from OUTSIDE your organization or institution contact you specifically for help, advice, or information on matters bearing on your work?

( ) Once a week or more ( ) Two or three times a month

( ) Once a month ( ) Less than once a month ( ) Never

47. Do (or would) you usually find such contacts:

( ) Very valuable? ( ) Valuable? ( ) Somewhat valuable?

( ) Not valuable? ( ) A waste of time?

48. Have you ever served on a committee or held office in the ASM:

a. Nationally? ( ) Yes ( ) No

b. in your Local Branch? ( ) Yes ( ) No

49. How much do (or would) you like to serve on a committee or hold office:

a. Nationally in the ASM? ( ) Very much ( ) Much ( ) A little ( ) Very little

b. in your Local Branch? ( ) Very much ( ) Much ( ) A little ( ) Very little

50. How many scientific conferences or symposia have you attended in the last 12 months? ______ Do (or would) you find these usually:

( ) Very valuable? ( ) Valuable? ( ) Somewhat valuable?

( ) Not valuable? ( ) A waste of time?

51. Are you presently on any national boards or committees to award research grants or review applications?

( ) No ( ) Yes How many_________?

52. Please indicate the number of times you participated in the ASM national meetings for the years 1964, 1965, 1966:

Number attended _____; Gave papers _____; Discussant _____;

Section convenor _____.

53. Do (or would) you find participating in national meeting <u>usually</u>:

( ) Very valuable ( ) Valuable ( ) Somewhat valuable

( ) Not valuable ( ) A waste of time

54. Have you served as an editor or referee for a professional journal in the past three years?

( ) Yes ( ) No

55. Do (or would) you find being an editor or referee:

( ) Very valuable? ( ) Valuable? ( ) Somewhat valuable?

( ) Not valuable? ( ) A waste of time?

56. In the last three years, how many scientific papers or reports have you authored for limited circulation? (e.g., within the institution where you work, for a client, etc. ________

57. In your career, to how many text or reference books have you contributed chapters by invitation of the editors? ________

58. How many professional books have you authored or edited in your career? ______

59. How many papers have you published in professional journals in the last three years? ______

60. As far as you are personally concerned, how important is it to you to publish professionally?

( ) Extremely important ( ) Very important ( ) Somewhat important

( ) Fairly unimportant ( ) Not at all important

61. What was your gross income (before taxes) from all sources directly related to your professional work (salary, fees, bonuses, royalties, etc.) for 1965? $______________

62. In my institution the salary of microbiologists as compared with that of similarly trained scientists in chemistry or physics is:

( ) Higher ( ) Equal ( ) Lower ( ) Don't know

63. In the spaces below, please list the names of up to ten living American microbiologists who have, in your opinion, made the greatest contribution to microbiology? (Note: this question will be used to determine correlates of eminence in microbiology.)

____
____
____
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____

THANK YOU VERY MUCH FOR YOUR COOPERATION

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