Dr. Howard M. Vollmer is a Research Sociologist at Stanford Research Institute. He holds the B. A. and M. S. degrees in psychology and sociology, respectively, from Stanford University, and a Ph. D. degree in sociology from the University of California. Before he joined the Institute in 1958, Dr. Vollmer was on the staff of the Institute of Industrial Relations at the University of California, Berkeley. He formerly planned the Ordnance Corps Civilian Supervisor Selection Project for the Army Ordnance Corps and directed it for 1 year. He has also taught courses on organizational analysis and sociology for the University of California, Pennsylvania State University, and the University of Alberta, Canada. In his present position, Dr. Vollmer specializes in the social and behavioral aspects of research and development, with particular emphasis upon analyses of manpower, personnel management, and human relations factors in R. & D. management. He has conducted several projects on topics related to this specialization, the most recent being a continuing study of the adaptations of scientists and research personnel in various industrial, government, and university research organizations, sponsored by the Air Force Office of Scientific Research. He has written technical publications on a variety of organizational and personnel management topics and is a Fellow of the American Sociological Association. This is his first lecture at the Industrial College.

SOCIAL ORGANIZATION OF SCIENCE AND TECHNOLOGY

18 September 1964

DR. POPPE: Gentlemen, it gives me pleasure to introduce to you, Dr. Howard Vollmer who will lecture this morning on the "Social Organization of Science and Technology." Dr. Vollmer.

DR. VOLLMER: Today we find ourselves in what is becoming more and more a science-dominated society, even as our ancestors once lived in a social milieu dominated by concerns of theology. In his recent book on "Science: the Glorious Entertainment," Jacques Barzun has written:

> The scientific stance is everywhere, even among the overt enemies of science; it is the strongest unifying force, because in the world of thought it is the only one. . . . Just as the language of theology ended by permeating common thought, so with us the language of science. The ultimate appeal (in medieval culture) was to the certitudes or sanctions of theology, as with us to the certitudes or sanctions of science. The highest concern of the culture was to support, perfect, and disseminate theological truth and practice, as with us to support, perfect, and disseminate scientific truth and practice. . . . It is worth noting that "layman" now means "not a scientist" as before "not a cleric. . . "[1]

So we find that faith in science and its methods of thought and expression has come to be almost a worldwide religion (or perhaps I should say a pseudo-religion) that dominates the value structure of all advanced modern nations. The atom bomb and sputnik have boosted the public image of science and technology to an all-time high.[2] Along with these developments has come the penetration of scientists into the "power elite," that is, the key decision-making councils, of modern nation states.[3] We see this power elite in action in the scientific advisory boards related to many agencies of our own Government, where both military and civilian executives are dependent not only upon the advice and assistance of leading representatives of scientific communities in order to achieve the

objectives and programs of their agencies, but are also often dependent upon the good will and approval of these leading scientists, at the same time that the scientists are becoming increasingly dependent upon government for the funds needed to conduct large-scale research projects. We have growing evidence that the influence of scientific thought, and certainly the privileges of scientists are considerable in other advanced countries, such as the Soviet Union.[4]

All this poses a major social problem for our time. How are we to control the activities and products of science in the interest of mankind? Most men agree--including most scientists, incidentally--that science, like any other major area of human activity, is not an end in itself; it must be related eventually, if not more immediately, to the service of the basic interests of mankind. In other words, how do you organize science in such a way that science itself can flourish, and yet at the same time, the products of science can be rapidly utilized in accord with the needs of increasingly complex areas of technology?

This problem is, of course, a familiar one to many administrators in government and industry, and many consider it to be a frustrating one. It is frustrating because we seem to know more about how to go about unlocking the secrets of nature than we know about how to design effective organizations--and especially effective scientific organizations. At the same time, this is a frightening problem to many scientists. They do not like to have people thinking too much and too seriously about "organizing" scientific research activities, because the very idea of "organization" to many scientists means some kind of external control, and they see the concept of "control" as inimical to concepts of "freedom" and "independence" which are keystone values in scientific endeavors. Like one school of political philosophy, most scientists feel that the form of organization that governs best is the form that governs least. This point of view is reflected in much of the literature on research organizations in industry and government, which has focused attention upon the conflict that it presumed to be inherent between the "scientific mind" and the "management mind."[5]

We now have increasing evidence, however, that although it is indeed valuable to point out conflicts between managerial and scientific interests that are likely to occur in various forms of research organization, these conflicts are nevertheless not inevitable. Where they do occur, they can be overcome by appropriate organizational forms, policies, and practices. The reconciliation of the

interests of management and the interests of science does not imply less organization, but rather a more appropriate and hence more effective form of organization. Organization of an area of effort does not necessarily mean that individual initiative and freedom will be done away with. Indeed, there is a good argument to the effect that both order and freedom (or individual initiative) are inherent in any effective form of organization, and that one cannot really have freedom without some form of order, law, or in other words, organization. Freedom without some effective form of order--that is, coordinating authority and rules for operation, becomes anarchy, in which no one's interests and rights are effectively protected. Certainly, even in the most highly structured organizations, the orderly pursuit of organizational goals cannot be maintained without the exercise of a certain degree of individual initiative and judgment in the face of day-to-day exigencies, as we know in the case of soldiers in combat situations, for example. [6]

Thus, the problem of organizing science is not basically a problem of administrative considerations versus scientific research considerations; it is rather a problem of determining the form of administration that is most suited to the particular blend of order and independence required in scientific endeavors.

Social Processes

In order to understand better the blend of order and independence that is required in effective scientific pursuits, it is important to recognize that both science and its cousin, technology, are social products. Like our ancestors who were sometimes overawed by the mystery and charisma that surrounded the activities of priests, we are sometimes prone to think of scientists as being somehow set apart and different from other men--as being "geniuses" with rare gifts of discernment that enables them to give birth to new scientific insights without outside assistance. Scientists and inventors usually view themselves with more modesty, as the statement of one outstanding man to the effect that "he had stood on the shoulders of giants" indicates. Although we recognize that scientists are frequently men of high intellectual endowment and academic attainment, we must also recognize that scientific "breakthroughs" often come at more or less predictable times in history after a number of scientists have been working simultaneously on the same problems and have been in communication with each other. [7] Also we note that scientists, like other men,

have been subjected to two large scale social processes that have affected practically all of the modern world of work. I refer here to (1) the process of professionalization and (2) the process of bureaucratization.

The process of professionalization, which has been most systematically investigated in its historical perspective by the English social scientist Professor A. M. Carr-Saunders, has affected many modern occupations, including the various scientific occupations. [8] Whereas scientists were essentially amateurs at one time (for example, in the days of Benjamin Franklin), they have now become organized into professional groups. These more professionalized occupations are characterized by (1) highly specialized intellectual skills based upon training in a general body of underlying theory, (2) formal occupational associations that control entry and develop codes of ethics to govern the occupationally related behavior of members, and (3) general community recognition of the special occupational status of professional members, often resulting eventually in some form of certification or licensing. Professionals develop rather strict standards of behavior, but their behavior is commonly controlled by mechanisms that are deeply impressed into the personalities of professional people. They learn these standards through the long training period that leads up to their professional degree, and most often through advanced degrees in the case of scientists. Before they are ever allowed to practice their profession, other full-fledged members of the profession have numerous opportunities to judge whether or not each individual has internalized appropriate standards of professional behavior in a satisfactory manner.

Since professionals are trained to respond primarily to internal standards of control in professionally appropriate ways, they tend to resist external controls from sources outside their particular profession in a work situation. Having proven their professional competence and compliance to those whom they have been taught to respect and emulate in their profession, they are likely to seek jobs that allow them a high degree of individual professional expression and opportunity to move from one position to another and from one organizational context to another in ways that will further these deeply ingrained professional interests and aspirations.

Also it is well to remember that persons trained in different professions tend to differ in the kinds of values and standards that they acquire. For example, one study has shown us that physologists

are prone to take particular pride in making contributions to the general body of knowledge in the natural sciences and to feel more identified with a career in a university setting, while mechanical engineers are much more interested in solving technical problems and look forward to a career in industry. [9] Since these interests become deeply ingrained in the value systems and personality structure of scientists in contrast to engineers, and vice versa, it is well to distinguish between the problems attendant upon employing scientifically trained persons in development activities and those attendant upon employing engineers in research activities. Research differs from development not only in its objectives, but also in the kinds of professionals who are likely to be most amenable to these objectives. This is sometimes overlooked by those who talk about R. & D. without distinguishing between "R" in contrast to "D".

Much of the conflict between scientists and managers that has been described in the relevant literature arises from the fact that scientists today are being employed in industry and government in increasing numbers. In these contexts, they are subjected to the process of bureaucratization. In the process of bureaucratization, as it was originally described by the German social scientist, Max Weber, a formalization of the structure of administration takes place. [10] This includes (1) a clearly defined heirarchy of authority which appears as a pyramid in an organization chart, (2) a high degree of specialization of function in various departments and down to the level of individual jobs within the organization, and (3) a proliferation of formal policies, written regulations, job descriptions, and other prescriptions. In this bureaucratic form of organization that is characteristic of government agencies and large industrial corporations, employees are hired to fill the prestated requirements of particular jobs; personnel are presumed to be interchangeable within job categories; and thus men are hired to meet the requirements of the job, rather than tailoring the job to correspond to unique individual capabilities. Furthermore, the normal pattern of career progression in bureaucracies is upward through the heirarchy into managerial roles within the particular organization. Finally, and perhaps most important for our considerations here, the bureaucratic form of organization ordinarily assumes that quality control over work will be exercised by higher level management officials within the organization, rather than in terms of internalized professional standards.

Thus we can see the contrasts between the conditions of work that scientists have learned to expect by virtue of their professionalized training and the conditions of work that they find in the

more highly bureaucratized environments of government and industry. Conflict in such situations may be fairly predictable, but it is not irreconcilable. Research of the adaptations of scientists in different organizational contexts being conducted at Stanford Research Institute under sponsorship of the Air Force Office of Scientific Research,[11] along with other studies, has pointed to a number of what we have called "adaptation mechanisms" that enable scientists to live in these strange organizational environments and, in turn, have enabled different kinds of organizations to utilize scientific products in ways that support organizational objectives and scientific interests at the same time.

Adaptation Mechanisms

Let me now describe what some of these adaptation mechanisms are and how they operate.[12] Some will be more familiar than others. I believe that an understanding of these mechanisms can provide a more sound basic background for tackling some of the more persistent and more frustrating problems that occur in the management of research organizations. We will briefly consider some of these problems later, after we have reviewed the mechanisms that enable scientists to adapt to different organizational contexts, and vice versa.

The first class of adaptation mechanism we shall call "professional adaptation mechanisms," because they permit scientists to pursue professional objectives in accord with professional values and modes of behavior in organizational contexts like government and industry. We have already pointed out that scientists tend to be different from many other kinds of employees in that they typically enter an organization with a deeply ingrained sense of what they want to do and of the conditions of employment under which they expect to do it. Scientists have been taught to value basic research--that is, research that is oriented toward making fundamental contributions to basic knowledge in their scientific fields--more highly than applied research, oriented toward the solution of practical problems of their employer. They expect to do their work with a bare minimun of direction from non-scientists; they tend to be especially jealous of what they consider to be scientific prerogatives, including a high degree of freedom in choosing the research problems they engage in, as well as a high degree of freedom in the actual day-to-day conduct of research.

How do they go about trying to achieve this kind of freedom in organizations whose basic mission is not the advancement of science per se, but rather the defense of the Nation, the provision of public services, or the manufacturing of consumer products?

Perhaps the most commonly recognized way that scientists achieve freedom of action in such organizational contexts is by what we have called status-advancement, that is, to move into supervisory positions where scientists themselves can control the direction and administration of research programs that have institutionalized recognition within the larger agency or corporation. There is a dilemma involved in scientists assuming managerial responsiblities, however, in that, although they can thereby obtain more control over the work they are doing and thereby better shape their own future within a larger organizational context, they also find more of their time taken up by administrative responsibilities and less time actually to conduct research--that is, to do the kind of work for which they were trained. First line supervisors, or research group heads and lower-level laboratory managers, may still think of themselves primarily as scientists rather than managers, and may still try to allow themselves opportunity to engage directly in their own research, but sometimes at the neglect of administrative responsibilities. Moreover, they find themselves in the typical marginal position of the first level supervisor or foreman as a "man in the middle," who Janus-like must represent managerial requirements to non-managerial scientist employees and at the same time must represent employee interests to management.[13] Many individuals who still see themselves as active scientists do not like such responsibilities, and we have reason to suspect that the number of scientists who have tried managerial responsibilities and voluntarily rejected them to return to the laboratory work bench is probably larger than for most other categories of employees.

A much less commonly recognized way for scientists to achieve freedom to pursue professional interests in various organizational contexts is by means of what we have called research entrepreneurship.[14] Research entrepreneurship involves the "selling" of research ideas to sponsors--individuals or groups who will provide funding for research projects--either inside or outside the employing organization. Under such conditions the relation between a scientist and the sponsor of his research becomes very much like a relation between a professional person and a client. This relationship is institutionalized and recognized in terms of formal obligations and responsibilities wherever a research contract exists between a principal investigator and a sponsoring

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individual or agency. The significance of the research contract for the professional scientist is profound because it establishes a relationship that cuts across the normal lines of hierarchical authority in a bureaucratic type of organization. The scientist, as principal investigator, becomes responsible for the quality of his work and for meeting various research requirements to the project monitor, or official representative of the sponsoring group or agency, rather than to his immediate administrative supervisor in the employing organization. Under such conditions, the employer comes to provide service functions to the principal investigator, rather than exercising direct supervisory responsibilities over the research.

Our studies of research entrepreneurship have found that almost all scientists in large universities and in independent research organizations reportedly have opportunity to sell research ideas to various sponsors outside their immediate administrative hierarchies, while from 50 to 60 percent of those in several industrial and government laboratories studied also report the opportunity to be research entrepreneurs. We find that many scientists do not personally enjoy "having to sell" research ideas, but that those who do and who are therefore successful research entrepreneurs are more likely to be more highly qualified scientists in terms of their education and experience, are more likely to be more professionalized in their interests and activities, are more likely to be able to freely select the research projects they work on, and are also more likely to produce more research publications in professional media. In other words, those whom we have identified as research entrepreneurs are more likely to obtain the kinds of opportunities that they desire in government, industry, and other organizations. At the same time, they are also more likely to be productive in a professional-scientific sense.

Not all research scientists are successful at selling their own research ideas, however, and fewer still are successful entrepreneurs all the time. Another mechanism is available to these people to obtain means for professional self-expression in more bureaucratized employment contexts. I refer here to what researchers commonly call "bootlegging." By this I mean engaging in non-formally-specified research activities within the context of larger formally-structured research projects. Bootlegging is sometimes done with some sense of guilt among research scientists, but ordinarily it does not represent any direct contradiction of requirements set forth in work statements for research projects. Usually the work statements for research projects are general enough to allow considerable flexibility in the way that scientists can handle the details of a project. Where they have this flexibility, scientists are

likely to choose alternatives that permit them to pursue their own interests in a manner that does not detract from the objectives of the project, but that may also result in contributions in general scientific knowledge in areas of professional interest to the scientists involved.

Our studies of this matter indicate that about one-fourth of the scientists admit frequent participation in non-formally-specified research activities, whereas another 40 to 50 percent admit occasional participation. Again, we find that those who are involved in these kinds of informally specified research activities are more likely to produce publications in professional media.

So much for this brief review of some of the ways that scientists try to express their professional interests in more bureaucratized organizational contexts. Our studies and those of others have also found a variety of ways in which organizations that are essentially bureaucratic in character attempt to utilize scientists and to integrate their activities with organizational goals or missions. These we can call "bureaucratic adaptation mechanisms."

The first of these is a familiar mechanism, recruitment and displacement. All organizations try to obtain the kinds of employees that are presumably best suited to what the organization wants them to do, and to get rid of those who are unable or unwilling to fulfill job requirements. Interestingly enough, however, we find that many organizations are really unsure about what to look for when it comes to hiring scientists. For example, where they have a choice, they are likely to hire a scientist who has a doctor's degree, rather than one who does not. They assume that the man with the doctor's degree is better qualified as a scientist, and , in a professional sense, this may be true. In terms of more bureaucratic requirements for applied research related to special organizational interests and problems, however, scientists with master's degrees may be more appropriate candidates for employment. Furthermore, we find that scientists with less than a doctor's degree are more likely to develop a strong sense of personal obligation toward the goals of an employer and to plan for a career with their employing organization. In any case, our research suggests that a more careful look at recruitment practices might well lead to the hiring of more suitable scientists as employees in certain kinds of research contexts. Recruitment takes on even more significance because of the fact that, in contrast to universities, few government or industrial organizations have any systematic means for displacing, or getting rid of, unproductive scientific personnel.

After serving for a period of time in a particular organizational context, most employees begin to develop a better appreciation for the objectives and requirements of their employer. In more bureaucratized contexts, especially, employees tend to internalize the values of the employing organization so that these become the personal values of the employees themselves. This is known among social scientists as the process of organizational socialization. However, it is a mechanism of adaptation that is not often recognized, and even less often skillfully used, by management.

Our studies indicate that in the process of socialization, an interesting thing happens. Initially, many scientists, like other employees, are inclined to be strongly attached to what they see as the objectives and values of the organization they are joining. After they get inside, however, comes a period of disillusionment. The initial "honeymoon" is over, so to speak, and they find out that problems exist "backstage."[15] Few organizations look as good to insiders as they do to those on the outside. So for a period of perhaps from 1 to about 5 years after initial employment, the level of general job satisfaction and identification with the organization is likely to be lowest among scientists, as among other kinds of employees. (We know, for example, that this is the time when turnover is likely to be especially high among personnel in the military service, as also among industrial workers.) Then after a few years, an employee learns "the ropes;" he learns how to live in the organization and how to find expression for his own interests within it. His first significant decision was to join the organization; if he now makes the second significant decision and decides to stay in the organization, his job satisfaction and personal identification with the organization is likely to increase again.

Therefore, we can generalize and say that for scientists as for other employees, the pattern of socialization into an organization tends to be curvilinear rather than rectilinear; organizational identification tends to be high at first, goes through a low period for a while, and then gradually increases again after the second decision to stay with the organization. After this second decision, the employee really becomes an "organizational man" in the sense that William H. Whyte has used the term.[16]

Insofar as this analysis of the pattern of socialization is accurate, it raises a number of questions that have not been faced very squarely by management in organizations employing scientists to date. Does management really want scientists to remain long

enough in an organization that they become committed to the organization? Are individuals who retain strong professional scientific interests more likely to leave in the interim period before making final commitments to the organization, leaving a gradually increasing proportion of older scientists who may have lost their scientific interests and capabilities? How might incentives be used more effectively by management to retain the more capable scientists and to sustain the professional interests and scientific capabilities of those who do remain for longer periods of time?

This last question brings us to a third mechanism that can be used by organizations to attract, retain, and maximally utilize scientific talents in ways that contribute to organizational objectives. The first I described as recruitment of those who are presumably most amenable to working toward the accomplishment of organizational objectives; the second as socialization, the provision of experiences over time that help relate and stimulate employee activities and attitudes in directions that support organizational goals; the third is the use of incentives--that is, inducements and rewards that are meaningful to scientists and that are also tied to organizational requirements.

In this regard, we also find that scientists are like other men in that salaries are important to them. Where they feel that they are not making as much money as professional colleagues who are doing similar work elsewhere, then they are likely to become quite dissatisfied with their conditions of employment and to be waiting for the first chance to get a good job elsewhere that pays better. Most enlightened management in larger bureaucratized organizations knows this, and accordingly tries to provide high salaries for their scientific and technical personnel. The mistake that some managers make, however, is to assume that higher-than-average salaries will necessarily attract better-than-average scientists. We are familiar with the escalation of salaries that has occurred in some industrial areas--especially in the aerospace industry.[17] However, the available evidence does not support any contention that higher than average salaries will attract unusually competent scientists. On the contrary, the data from several studies indicate that once scientists have what they consider to be an adequate salary and fringe benefit package, they can only be stimulated to higher levels of research productivity and can only be attracted to other research environments by non-monetary incentives. The most potent of these non-monetary incentives--that is, the one that is most frequently related to higher levels of research productivity and general job

satisfaction among scientists who feel that they have basically satisfactory salaries--is freedom to select the research projects they work on, or in other words, opportunity to work on those things they want to work on.[18]

Now, you see, we have come full circle in our analysis of adaptation mechanisms for scientists. We have said that the professional orientation of scientists--that is, the values that are ingrained into them in their long period of professional preparation and education--emphasizes the importance of autonomy and freedom in the conduct of their work. They carry this orientation with them when they become employed in industry, government, universities, or elsewhere. The professional values and disciplines that they have acquired are almost as strongly ingrained as the codes and disciplines of the professional soldier.[19] If organizations are to utilize these professional scientists in an effective way, they must provide for the fullest expression of professional attitudes and activities, rather than resisting them. Bureaucracy must learn to accommodate to professionalism.

This is all well and good, you may say to yourself; but you may feel that what I am really saying is that organizations that employ scientists ought to just "give in"--"give the scientists what they want and keep them happy." My answer here would be that I do not say that organizations ought simply to reorient themselves to the needs and interests of scientists. Instead, I have been speaking about accommodation. In this regard, I would emphasize that accommodation is not capitulation. Let me try to explain further what I mean by brief reference to three common problem areas in the management of research organizations--the evaluation of research productivity, the organization of research in contrast to development, and the coupling of research with nonresearch activities.[20]

Management Applications

We know that research organizations, unlike many other kinds of organizations, do not produce tangible things; they produce ideas. We also know that ideas are not as easy to count and measure as are items of hardware. At the same time, top level management must have some way to evaluate the comparative effectiveness of different research laboratories under its direction with regard to the production of ideas, just like top management must have ways to evaluate the comparative productivity of hardware divisions.

Furthermore, we know that research ideas are of no value until they are used by someone--in other words until they are communicated somewhere. The question is, where? In this regard, we must carefully examine the objectives or mission of the research organization in question. Is its objective to produce ideas that get into the basic fund of knowledge in a scientific discipline, or is its objective to produce ideas that can be quickly translated into applications in development or production segments of a larger organization? Or both? This question about organizational objectives seems obvious, and yet I would point out that it is often confused and not clearly specified by management in many organizational contexts.

Having determined the objectives that a research organization is supposed to accomplish, we can then begin to measure the extent to which it actually is accomplishing these objectives. Of course, one cannot measure ideas directly like one can measure tangible objects, but, insofar as we assume that ideas must be communicated somewhere before they are of any value, then we can begin to measure communications. The most obvious and common way to measure basic research productivity is to count publications contributed to the professional literature from an individual or laboratory group, assuming of course, that each paper accepted for publication by an editorial review board of a respectable scientific journal must contain at least one scientifically good idea, and maybe even two or three. But many will be quick to point out that this system has its faults, the main one being that it provides a crude indication of quantity of research productivity, but not necessarily of quality. There are additional ways to measure quality of research output, however. These include determining the number of times that research papers, once published, get cited by other scientists in papers that are published later, and also calculating the numbers of papers published in journals that are uniformly recognized by scientists as being top quality journals in their scientific fields, in comparison with numbers of publications in journals considered to be of lesser quality. In contrast, an indicator of applied research output may be obtained by examining the quantity and quality of in-house research reports. We are now investigating the usefulness of several of these measures of research productivity for laboratories in one part of the defense establishment.

The point I wish to make here is not that any of these admittedly crude measures is adequate by itself to measure either quantity or quality of research productivity, but rather that, in combination,

they provide multiple indicators of output in ways that can contribute to the concerns of bureaucratized organizations for accounting for the effectiveness of specialized segments within the organization and, at the same time, are in accord with the value system and objectives of professionalized scientists. They represent one way to accommodate bureaucratic and professional objectives.

Another problem area is the degree to which research activities can be organized along the same lines as development activities, and can therefore perhaps be combined with development activities in complex organizations of government and industry. Our research findings imply that such combination is undesirable because the organizational requirements for research differ so markedly from the organizational requirements for development. Not only are different kinds of people required in research activities, and people who have different attitudes toward their work and toward their employing organization, but there is evidence that significant decisions regarding the initiation, conduct, and technical evaluation of research must be made at a different level than for development. Whereas development projects can be planned and programed from a relatively high level of management and can be controlled by PERT systems and similar indicators of progress and accomplishment, only those scientists at the working level can decide on the appropriateness of alternative lines of investigation open to them at any given point in a research project, and frequently such decisions about the conduct of a project cannot be made until the scientists have accomplished a previous step in the research. Some of the most important research findings have come from "serendipity," that is, unexpected discoveries made during the course of some research oriented in another direction. This is why such highly professionalized persons with internalized standards of discipline must be employed in successful basic research activities; external control procedures are as insufficient here as they would be in the case of a surgeon in the operating room of a hospital.

This does not mean, of course, that research programs cannot be planned in a general sense. General scientific areas can be selected for sponsorship in terms of mission requirements of the sponsoring agency, but with regard to most of the day-to-day conduct of research within these general scientific areas, there must be a special accommodation between bureaucratic requirements and professional modes of operation, as there is in universities and in hospitals.

This leads us to the third area of very understandable concern to management in practically all larger government and industrial organizations, namely, how do you "couple" research and non-research activities. We may agree that research activities need to be organized differently and separately within the larger organization, but we cannot afford to neglect the translation of research findings into useful applications at every possible opportunity. We need to know much more than we now know about how to facilitate the coupling of research with nonresearch activities, but at this point we might consider the imagery of the concept of "coupling." Coupling implies the relationship of two things that retain their integrity; it contrasts with the concept of "assimilation." Thus the problem becomes, how can research activities be related to other activities without detracting from the integrity of both research and nonresearch functions?

To date, our investigations of this matter indicate several findings to bear in mind. One is that we have found that scientists employed in government laboratories who are quite professional in their attitudes and quite devoted to making basic contributions to their scientific fields also develop strong feelings of obligation for helping to translate their findings into useful applications for their employing agency. In other words, we find that most scientists, like other professionals, are very moral men when it comes to their work. The employer who allows them freedom in the pursuit of basic scientific concerns will also call forth a reciprocal obligation from scientists to spend some time considering the employer's problems also. We suspect that a more authoritarian attitude on the part of management would conversely induce scientists to have more resentment and less interest in their employer's problems.

Furthermore, we have reason to believe that the most effective interchange of ideas occurs in person-to-person contact, rather than through written communication alone, and we also know that many scientists are stimulated to higher levels of research productivity by engaging in a variety of activities, rather than being completely absorbed in one all-consuming research project.[21] Therefore, there is good reason to believe that many scientists might spend a limited amount of time in contact with non-scientists in ways that would facilitiate coupling and perhaps even stimulate new research ideas. Here again, we believe that we are moving toward a better understanding of accommodation between the requirements of bureaucracy and the interests of professional scientists.

In conclusion, I find myself recalling the movies about Frankenstein that I used to see when I was young. They were both thrilling and terrifying to a little boy. You remember that Frankenstein was a mad scientist who created a monster in his laboratory that nobody could control. It is an old theme that goes far back into prior literary traditions. Faust, in the late middle ages, let his scientific curiosity get the best of him, and found that he had sold his soul in the process. We are told by the Bible that at the beginning of human history, Adam and Eve lost their home because they could not resist eating the fruit of the tree of knowledge. Today, however, perhaps we can have more confidence that we are beginning to develop a sophistication in our knowledge of human relations and social organization which may eventually approach the sophistication with which we can probe the secrets of nature. I believe that herein lies some hope that we can avoid treating science as an end in itself, but rather organize it so that it can more effectively contribute to human betterment.

QUESTION: Dr. Vollmer, in looking at the aspects of bootlegging, with the scientists desiring the freedom of their actions and the like, it raises in my mind a certain concern for our security of classified information. In our cultural scientific exchanges with the Soviet Union, what is your opinion of the danger within the scientific environment, the quid pro quo or exchange of ideas, of perhaps violating security or giving away information we should not?

DR. VOLLMER: I think there very definitely is a problem here, that if I were a security officer I would be concerned with. At the same time I think it is a problem that you have to live with as long as you are dealing with scientists; not because scientists are not patriotic people and are not concerned with security themselves. But science is something by it's very nature which you want to communicate to other scientists, whether they happen to be overseas or wherever they happen to be from.

Of course, we know that many scientists participate in international meetings where they come in contact with people from their same fields overseas in other countries. So, there may be some security problem here.

I am no specialist on security, but I think it is probably a greater problem in the development or engineering area than it is

in the basic scientific area. Because, I believe at the present time most of the scientific work that is going on within the Soviet Union is published in open sources, as is ours in almost every field. It is only later on when this gets into certain weapons development areas that it is classified, either on our side or on the other side.

QUESTION: Doctor, the overall impression that I get from your paper is that scientists present a completely unique problem when it comes to the problem of directing their efforts toward predetermined or broader goals imposed from above. Can you explain in what way scientists are unique from any other professional group, and would not the general problems and principles you have laid out apply to any professional group effort toward organizational goals?

DR. VOLLMER: No, I think scientists are different from any other kind of professionals. Engineers, for example, as I tried to indicate in one of the illustrations that I gave, are more likely to be problem-oriented in their training. They will take a problem from almost anywhere, whether it comes from management or elsewhere if it is a technically challenging problem. Whereas scientists--and now I am speaking of physical scientists, these people tend to be oriented, through many years of training to look at things which they believe are of theoretical significance. That is, basic significance within their discipline. This may not be relevant to a management problem; so therefore you do have something here which is somewhat different from other professionals.

I do not know which other professionals you had in mind--take attorneys for example--lawyers. These are people who obviously have to be oriented toward the problems of their clients. Not so with scientists; I think they are somewhat different here. Of course, I am talking in generalities. We all have lots of exceptions to point to.

QUESTION: Dr. Vollmer, we have discussed the rapid expansion or explosion of discoveries and knowledge in practically every field of science. This has brought up the need for post-doctoral education. Has industry or government tried to use to any extent a sabbatical-arrangement or guaranteed a post-optimum of work as an incentive to get and retain good scientists.

DR. VOLLMER: This has been used on a small scale. That is, where scientists can get some funds sometimes from an outside

foundation, for example. The Ford Foundation, I believe, provides some funds for a post-doctoral studies in certain fields, and there are other foundations that do this. I think the most common practice for industry--and this may apply in some governmental agencies--is to allow a man time off; a year's leave without pay if he can get some foundation funds to sponsor this kind of thing. But I do not know that many organizations have had many internal funds to sponsor this kind of thing.

There might be some argument for this, as I think some of the points that I made imply. But it would have to be done on a rather limited scale.

QUESTION: Would you compare the contributions made by organizational-type scientists as compared to free-lance operators? I have the impression that most contributions are made by atomic energy and comparable fields, and the rest are made by organizational-type sciences.

DR. VOLLMER: No, I could not contrast them if you meant by that a kind of invidious contrast, to say that the organizationally-oriented scientist is more important to us than one who is not. Because, I believe that in the field of atomic energy, for example, or in the field of the space sciences, a lot of the practical accomplishments like the kinds of space shots that we have had recently, that have gone up and been able to hit the moon, et cetera, are directly attributable to the efforts of the engineers, often, and of organizationally-oriented scientists, if you want to call them this.

I think it is unfortunate that in the public press they do not get enough credit for their contribution to these kinds of things. We talk about scientists and we think of some guy in a laboratory doing this. Whereas, these were other kinds of people who did this directly. But their ability to do this, to have these engineering accomplishments, would certainly go back to the research that other individuals did in this country and in Europe. Certainly, we know this is true in the field of nuclear energy; going back to the work of Einstein and various European scientists.

QUESTION: Sir, you indicated that the numbers of scientists who gave up administrative supervisory deals and went back into the lab were greater than for other professions.

DR. VOLLMER: I said I suspected it was greater. We do not have data on this, but I think if we went out and collected it, I am pretty sure that that is what we would find.

QUESTION: Military management has demonstrably met the criteria of occupational freedom for the scientific community. However, I find the location of a sharp social conflict develops from the inter-action of the parents of the two groups, through, generally, a strict discipline on the one hand and the rejection of social conformity on the other. Can you comment on the necessity for the scientist to extend occupational freedom to a very social non-involvement?

DR. VOLLMER: I am not completely sure that I understand your question but I think I got the gist of it. You mean the fact that many scientists wear beards and vote strange ways, sometimes get involved with strange political movements and organizations, et cetera. Is this the point of your question?

QUESTION: I mean mingling the military group with the scientific group in common community relationships. (Remainder was inaudible.)

DR. VOLLMER: You mean they may have different patterns in the way they control their children and this kind of thing? Well, I have no doubt that this may be true. I do not know what to say about it, frankly. I am sure that it contributes to trouble, and all I can say is that I think this would be a place where one would probably want to have councils of both groups of parents getting together and seeing if they could not come to some kind of norms of standards of behavior that might be acceptable to all the people who have to live in this community where their children have to get along with each other.

QUESTION: Do you think that scientists ought to comment on strictly political matters outside their areas of special scientific competence?

DR. VOLLMER: I think that what we have to remember is, that when these scientists comment on these kinds of things that their point of view, if they are speaking outside of their particular field, is not more valuable than the point of view of any other citizen--you, I, or anybody else. At the same time I think we also have to recognize this, that insofar as these people are also

citizens, they certainly do have concern with education and matters of national policy that every citizen would have a concern for as a voter, and they can speak on those terms.

So, what we ought to do and they ought to do, I think, is to separate their professional field of competence from what they have to say as a concerned citizen in some particular areas.

QUESTION: Previous speakers have mentioned the collection of public speeches that are coming out and that probably a great number of scientific publications are produced. Would you care to comment on that?

DR. VOLLMER: I think that this is a very serious problem. Certainly, it is a serious problem for scientists--the so-called "information explosion" which is occurring today. Because, most people who work in scientific fields find it very difficult to keep up with what is new in their field because there are so many things in their field nowadays that they have to read in order to keep up with it. Often there is an over-emphasis on quantity of publication, sometimes, many of us think, to the detriment of quality in publications.

The last comment that I want to make on this is that some of us have proposed some research. In fact, when I leave here this morning I want to go to talk to somebody in one of the government agencies about a study that is being proposed, to determine the degree to which the present information explosion actually represents an explosion in concepts--that is, worthwhile new ideas--or whether it represents a kind of increase in noise in the system; in other words, people talking more and more about the same old things to each other. We suspect in some fields it is the latter.

So, what we would like to do is have a technique or methodology for measuring the growth of concepts rather than just the growth of written material, to find out whether we are really getting something worthwhile out of this so-called information explosion. I think that this needs to be done by somebody.

QUESTION: (Inaudible--too much coughing to make question coherent).

DR. VOLLMER: Again, I am not sure whether I exactly understand the point of your question here.

QUESTION: Behind all our Federal grants is competition for the dollar; contracts going to universities for scientists. There are family pressures for this man to get more grants rather than worry about the scientific detail.

DR. VOLLMER: I think there are two things that you are talking about here; that is, money that is to be used for salaries and money that can be used for research purposes.

Now, scientists, I think, are no different from anybody else in that they are effected by this status advancement pattern that we have in our society nowadays. They like to have a nice car and a nice place to live, for their families. Where they do get married and have families, they have wives who like to wear nice clothes and like to have a nice standard of living, naturally, for the children. So, there are all kinds of pressures to increase their salaries. Yet, at the same time these are men who did not go into business, for example. Many of them might have been successful businessmen; perhaps where they could have attained even higher salaries.

The point I tried to make in my lecture was that I think once these people have a basic minimal salary level here that reflects what they believe ought to be their status in society, then over and beyond this, if you pay them more in terms of personal reward or emoluments, it does not do much from the standpoint of any kind of scientific productivity. We have some data on this which show this.

Management would be better advised in this case, I think, to use extra dollars over and beyond this, rather than paying these people unusually high salaries; use extra dollars to enable them to do more basic research, perhaps, in areas that have a good likelihood of payoff for the company.

QUESTION: Dr. Vollmer, could you tell us something about how a specific project like how Standford's Linear Accelerator gets started; who had the initial idea; who has the idea; how it is financed; and what do they expect to get out of it?

DR. VOLLMER: This would be another lecture, to talk all about the linear accelerator. Let me ask if you could focus a little bit more on the point of your question. Perhaps I could talk on it. Obviously there are many individuals involved in this--various interest groups; a university is involved in this; a government agency

is involved; there are political considerations--Congressmen involved in it.

QUESTION: Who started the idea, "Well, now, we need a two-mile linear accelerator. Let's get the funds." They say, "Okay, we'll give you the funds and it's going to be confined to the people who work with the results." Are they going to be people from Stanford, from all over, or what?

DR. VOLLMER: The point of this project, as well as I understand it--and I am no specialist on this particular project--is that it is something that is going to be of general interest to the whole scientific community in understanding more about the nature of nuclear energy. I am still not quite sure that I understood your question, but I think perhaps I should say that there are many individuals--scientists and non-scientists--who get involved in these kinds of things. You have to look at both sides of the picture.

Also, another point that I tried to make in my lecture today is that science is a big interest group in the country today, obviously. What scientists want comes to the ears of politicians and government officials, and they cannot be ignored. I think many of you have had experiences with this and know that this is a very important group in our society today. These are people who are politically powerful.

QUESTION: (INAUDIBLE)

DR. VOLLMER: Please repeat the question.

QUESTION: (Repeated)

DR. VOLLMER: Oh, I see. I think obviously here you have to bring the practical consideration to his attention. How you do this, I do not know that I can answer that question. It is a problem; I recognize it.

DR. POPPE: On behalf of the College I would like to thank you very much for your excellent presentation. Thank you.

(22 Jan 1965--5, 900)H/pd:ca/bn

FOOTNOTES

[1]Jacques Barzun, Science: the Glorious Entertainment (New York: Harper and Row, 1964), pp. 16-17.

[2]A review of public attitudes toward science and scientists is provided by Wilbur Schramm in "Science and the Public Mind" in Studies of Innovation and of Communication to the Public (Stanford, Calif.: Institute for Communication Research, Stanford University, 1962), pp. 263-86.

[3]The concept of interlocking power elites was discussed in detail by C. Wright Mills in The Power Elite (New York: Oxford University Press, 1956). See also Harry S. Hall, "Scientists and Politicians," Bulletin of the Atomic Scientists XII (1956), pp. 46-62, and C. P. Snow, Science and Government (New York: Mentor Books, 1962).

[4]Nicholas DeWitt, Soviet Professional Manpower (Washington: U.S. Government Printing Office, 1955).

[5]See for example, Opinion Research Corporation, The Conflict Between the Scientific Mind and the Management Mind (Princeton, N.J.: Opinion Research Corporation, 1959); Simon Marcson, The Scientist in American Industry (Princeton, N.J.: Princeton University, Industrial Relations Section, 1960); William Kornhauser, Scientists in Industry: Conflict and Accommodation (Berkeley and Los Angeles: University of California Press, 1962).

[6]S. L. A. Marshall, Men Against Fire (New York: William Morrow, 1947).

[7]Bernard Barber, Science and the Social Order (New York: Collier Books, 1962); W. F. Ogburn, Social Change (New York: Viking Press, 1950).

[8]A. M. Carr-Saunders and P. A. Wilson, The Professions (Oxford: Clarendon Press, 1933); for a collection of writings on this subject see H. M. Vollmer and D. L. Mills, Professionalization: Readings in Occupational Change (Englewood Cliffs, N. J.: Prentice Hall, Inc., forthcoming).

[9]Howard S. Becker and James Carper, "The Elements of Identification with an Occupation," American Sociological Review, Vol. XXI (1956), pp. 341-48.

[10]Max Weber, The Theory of Social and Economic Organization, translated by A. M. Henderson and Talcott Parsons (Glencoe, Ill.: Free Press, 1947); see also Peter M. Blau, Bureaucracy in Modern Society (New York: Random House, 1956) and R. K. Merton, et al., Reader in Bureaucracy (New York: Free Press, 1952).

[11]This research under Contract No. AF 49(638)-1028 has produced the following AFOSR reports to date: H. M. Vollmer, T. R. LaPorte, W. C. Pedersen, and P. A. Langton, Adaptations of Scientists in Five Organizations: a Comparative Analysis, 1964; Adaptations of Scientists in an Independent Research Organization: a Case Study, 1963; and A Preliminary Investigation and Analysis of the Role of Scientists in Research Organizations, 1962.

[12]A more detailed report of findings with regard to these adaptation mechanisms in a university research organization, an aerospace industrial research organization, a nuclear industrial research organization, a government defense research organization, and an independent research organization is presented in Adaptations of Scientists in Five Organizations, op. cit.

[13]F. J. Roethlisberger, "The Foreman: Master and Victim of Double Talk," Howard Business Review, Vol. XXIII (1945), pp. 285-94; also D. E. Wray, "Marginal Men of Industry: the Foremen," American Journal of Sociology, Vol. XLIV (1949), pp. 298-301.

[14]W. G. Bennis, "The Social Scientist as Research Entrepreneur" Social Problems, Vol. III (1955), pp. 44-49; H. M. Vollmer, "Entrepreneurship and Professional Productivity Among Research Scientists" in Professionalization: Readings in Occupational Change, op. cit.

[15]For a further discussion of differences between "out front" and "backstage" views of an organization, see Erving Goffman, The Presentation of Self in Everyday Life (Garden City, N. Y.: Doubleday Anchor Books, 1959), especially pp. 106-40.

[16]William H. Whyte, Jr., The Organization Man (Garden City, N. Y.: Doubleday Anchor Books, 1956).

[17]This salary escalation is largely the result of what has been called the "project piston effect"; see A. Shapero, R. P. Howell, and J. R. Tombaurgh, An Exploratory Study of the Structure and Dynamics of the R&D Industry (Menlo Park, Calif.: Stanford Research Institute, 1964).

[18]Adaptations of Scientists in Five Organizations, op. cit., and L. Meltzer "Scientific Productivity in Organizational Settings," Journal of Social Issues, Vol. XII (1956), pp. 33-40.

[19]M. Janowitz, The Professional Soldier (Chicago: Free Press, 1960).

[20]These matters are discussed in more detail in a forthcoming report to the Air Force Office of Scientific Research, H. M. Vollmer, Explorations of Applications of the Behavioral Sciences in Research Management: an Initial Study in the Office of Aerospace Research, 1965.

[21]D. C. Pelz and F. M. Andrews, "Diversity in Research," International Science and Technology (July 1964), pp. 28-36.

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