Social Influences and Scientists

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The author, Warren O. Hagstrom, has undertaken the research for this book, **The Scientific Community** (Basic Books, New York, 1965. 320 pp., $5.50), as a contribution to the study of the behavior of scientists. His stated purpose is "to study the operation of social control within the scientific community, with the problem of discovering the social influences that produce conformity to scientific norms or values." In so doing he embarks on a delicate theme in the eyes of the scientific community to whom the words "control" and "conformity" are provocative.

For this study he has selected primarily three science disciplines: mathematics, physics, and molecular biology, the first two as being well established and the last as an active emerging subdivision of the life sciences.

Although attention throughout is focused on the main theme, the book contains interesting discussions of auxiliary topics—namely, teamwork, communication, segmentation, and functional differentiation; the origin, conduct, and resolution of disputes; and the future of science.

The study is chiefly based on 90 interviews with a sample of scientists and science students in the fields selected, mostly from five prominent universities. From them very many quotations are given, most of which are anonymous. Their responses are used partly as expert testimony and partly in canvass of opinion.

As the author notes, a study of social controls involves concurrent study of the factors that give rise to deviation. Consequently much of the study is devoted to discussion of variations in behavior patterns and to the factors that may be responsible; in fact this is pursued to such an extent that the reader may lose sight of the point, that the presence of deviation means the existence of behavior standards, and controls.

In view of the growing importance and prestige of natural scientists and the technological applications of their research, the scientific community has been a topic of increasing interest and indeed curiosity to the general public, and especially to social scientists. Curiously, perhaps, the author does not seem disposed to include social scientists in the scientific community; however, he concentrates his attention on the other end of the spectrum, mathematicians and physicists, as having the purest objectives for basic research, and probably the greatest independence. It is clear that his purpose is to do a scholarly job as a social science study. At times considerable curiosity and some bafflement are apparent in his approach. As one of his findings, it appears that this scientific community contains individuals who show practically all the variations in activity, belief, and prejudice found among professionals in any career. Evidence that even mathematicians and physicists have aims, traditions, and behavior similar to those of scholars in other professions may appear significant to many readers.

The many quotations included in the text leave one with a feeling of uncertainty as to the mode of their selection. They are mostly used to illustrate particular points significant to the author, and there is little indication of the degree to which the views quoted represent a concensus, except in so far as a quotation is attributed to some outstanding leader (usually unnamed) in the field. This kind of attribution is typical of many of the quotations given.

Faddism in Perspective

In commenting on this undertaking and its documentation there may be morals in two historical quotations. The first comes from a statement attributed to Plato: "All mankind are divided into three classes—those who think, those who act and those who imitate one or the other." The application of this observation here raises an interesting question. There are ideals and traditions underlying the motivational behavior of any profession. No doubt these have been set as primary goals by the work of outstanding leaders in the past—"those who think and those who act." However, the pattern of research during a particular generation is also influenced by its current leaders and by current progress in the area. These statements are certainly true of research in science. Many scientists are quite independent in their research planning, but there are probably many more, especially students, who follow the current lead or the style of the principals in their discipline. This is inevitable in any career and may result in what has often been called "fads," together with periodic fluctuations of interest and activity among disciplines and within a single discipline. Thus, at a given period, large numbers of scientists may flock to research in a subdiscipline to which they are attracted by the contributions of some gifted individual or group or by its apparent current promise, without special regard for their own preferences or qualifications. There is justification for this behavior in that an important new research finding requires confirmation. Moreover, such a finding justifies and encourages expansion by attracting numbers of disciples to ensure a capable selection of research workers in the new field. But in a very human manner this may go too far, until some of the ablest and most venturesome break away; in time this may start other mass movements. All this is quite understandable, and in the opinion of most scientists should not be replaced by organized efforts to control the areas of research activity. Accordingly, an interesting question is the degree to which members of the scientific community, in undertaking research, "imitate" contemporary outstanding scientists ("those who think") and follow their lead, as compared to the exercise of genuine individual judgment and insight. In other words, how many think and act accordingly, and how many merely follow? This has obvious bearing on social control in science. For, if the emulation of leaders in the profession, apart from recognition by them, furnishes motivation for a large number, then one should inquire as to the motivation of these leaders. Here history and observation of contemporary leaders would indicate the motivation to be that present in all

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creative work—the simple urge to pioneer.

The second observation comes from the writings of Oliver Wendell Holmes and may be briefly paraphrased as follows:

> When John and Thomas talk together there is bound to be misunderstanding among the six of them. There is Thomas's ideal John, John's ideal John, and the real John; they are all different, and the same is true of Thomas. Therefore there is bound to be misunderstanding among the six.

As Thomas, the sociologist, the author is writing about John, the natural scientist. Accordingly, the book should be supplemented by a companion volume written by John. Whether anyone can know the real John is, of course, a question, but it may best be approached by a combination of appraisals by both Thomas and John.

The Sociological Image: Role of Communication

It may occur to readers that the objectivity of the author's thesis is marred by his attempts to fit scientists as a class into one of the conventional categories according to social science, which have evidently been developed on a socioeconomic basis. (This is John talking about Thomas.) Thus he considers and (correctly) dismisses the hypothesis that research scientists progress within their closed group by using money or barter as a means of exchange. He concludes that the system must operate by the "gift" method—that is, the publication or communication of research results, since these bring no financial remuneration, must be classified as gifts for which the reward is social recognition. He presses this hypothesis in many of his interviews and finds almost universal rejection of it. His conclusion is nevertheless that the system moves along these lines even though the members of the scientific community do not admit it.

This thesis implies a degree of obligation. Now it is undeniable that in our society, within one's own circle at least, a gift is commonly assumed to carry with it some obligation to reciprocate. However, in the case of research, the gift is information. It is commonly broadcast to listeners at a scientific meeting or published in a research journal. In the latter instance, there is no obligation toward exchange to the author (a return gift must be purchased by subscription to the journal!) and none for recognition. Of course there is frequent direct communication among active researchers in their special field, but one may question whether the chief motive is to build mutual feelings of obligation per se. In my view it has a quite different basis: in addition to passing muster for its soundness and accuracy, in order to have value a piece of research must be original, not only to the researcher himself but also to the scientists in his field. That is to say, the research must be novel to the profession in its conception, findings, workmanship, or detail, thus representing an advance in knowledge or understanding of the subject. A single individual cannot judge the originality of a research contribution he may plan or make unless he is in constant, close communication with his colleagues in the same field. Because every one of them is in the same situation they must and do communicate. It is true that this constitutes an obligation, but it is not primarily a feeling of obligation toward recognition of any of the parties involved. Rather, for both parties, this exchange of information is an essential factor in planning and presenting research. It is a behavioral characteristic called forth by the special type of objective in scientific research.

Prestige and Its Concomitants

Reflection on the foregoing characteristic of scientific research leads to the identification of a dual aim of scientific research and, indeed, scholarly research in general, which seems almost a paradox. Thus, among research workers, one finds an interesting and effective combination of competition and cooperation. There is certainly competition on the part of an individual in one of the strong personal objectives of his research—namely, to be the first to announce a new research finding to the scientific community. At the same time, as has just been described, each individual is impelled to share his plans and activities with his immediate colleagues. This sounds like a dilemma, but in practice it is not actually regarded as such. The members of the scientific community have lived with it for a long time and, although there are occasional difficulties and abuses, it has worked extraordinarily well. From a philosophical standpoint this may have broader implication, in that individuals living in our kind of democratic society are faced with the same dilemma and must learn its solution in somewhat similar manner.

Social recognition implies degree of prestige. To begin with, the prestige of a discipline is probably an important factor in the selection of a scientific career, and also in the selection of major courses by students. But in principle few scientists would be inclined to rate the various scientific disciplines in a priority list of importance. The pursuit of knowledge should have no such ranking lists. However, in the light of history, there have been many occasions when the advances in a particular discipline were admittedly outstanding and affected scientific thought and the philosophy of society at large. Likewise, contemporary research activity has a pronounced effect on the current prestige of a scientific field, made up partly of the recognition of this progress by the scientific community itself and partly by public opinion based on the research news that reaches the nonscientific community. Thus, begining with the discovery of nuclear fission and especially with the demonstrations of nuclear energy, the prestige of nuclear physics has been high. Similarly, the remarkable progress recently made in molecular biology, with its approach to some understanding of the genetic code and the process of cell reproduction, makes this at present a high-prestige field.

Generally speaking, among physicists the greatest prestige has always been accorded research with important bearing on the major objective of physics, to "understand" the physical world and, more specifically, the study of the fundamental nature and organization of matter, energy, and their relationships. Likewise, the highest prestige among biologists is accorded research bearing on the origin and nature of life.

With the public, prestige may contain several ingredients. Important among them are obviously brilliant and imaginative thinking, opinions expressed by scientific leaders, degree of understanding of the research, occasionally its unusual or bizarre findings, and especially its possible and recognizable bearing on practical accomplishments.

Actually, the recognized disciplines of science have themselves emerged as prestige fields of knowledge: for example, physics, for knowledge and understanding of the physical world; chemis-

try, for the organization of material substances; geology, for the origin and nature of the earth; biology, for the origin and functioning of life; and astronomy, for knowledge and understanding concerning the heavenly bodies and the universe.

Behavior: Socioeconomic and Other Aspects

As in all professions, the socioeconomic aspect of the behavior of scientists is interesting and important, and *The Scientific Community* may be commended for bringing this out. In our society the individual is under compulsion to "make a living." He has varying degrees of motivation toward the quality of this living and its attendant assets for himself and his family, such as possessions, prestige, and hobbies. In these respects there are wide variations among individuals. However, the point is not so much whether scientists have these motivations—of course they do—but rather the relative degree to which they exist in the science community as compared to other professions, and also the presence and importance of motivations which are especially characteristic or possibly unique to their own calling.

In this respect it will probably seem curious to most natural scientists that the author does not seriously consider two elements in the explanation of the behavior of research scientists: (i) the simple urge to acquire new knowledge and understanding and especially the intrinsic attraction of unsolved problems; (ii) the realization among scientists that the development of science itself must be treated with the utmost care and attention lest its structural foundation be undermined, and the consequent strong traditions and guiding principles which are necessary to protect this structure and guarantee its sound development.

This failure fully to appreciate the fascination of the research game is important. This trait (for example, puzzle solving) is obvious in many young children and is perhaps partly responsible for the occasional characterization of scientists as naive. It is an individual characteristic that has nothing to do with recognition and is to many a most powerful incentive. It may be given a more mature interpretation by remarking that a given individual achieves greatest satisfaction in the cultivation and full exercise of a particular ability. Nature, to a scientist, is territory to be explored and, to the extent possible, "explained." He sees his work as requiring curiosity, imagination, accurate observation and deduction, and especially the ability to perceive and solve perplexing problems. Where originality is at a premium, as in research, it is the realization of making a significant original contribution—solving an unsolved problem—that provides the greatest thrill, not recognition of the fact by others nor the rewards or prestige which may result, gratifying though these may be. By the same token, there is satisfaction in independently solving a problem that has been solved before, but none if one knows and follows the previous solution. The analogy with mountain climbing, given by the author, does not hold, because skill and stamina are involved in repeating a first ascent, even if the climber knows the exact route. There is nothing in research which corresponds, except an occasional highly delicate and complex experiment or unexplained analysis. However, these mere facts are a challenge—that a problem has not been solved and that, *ipso facto*, the problem is tough and the achievement great. This characteristic is explicit or implicit in many of the quotations listed, but it is not cited as a valid motive. Instead, the motive "search for truth" is dismissed as a popular illusion among the scientific community.

Some lack of understanding is displayed with regard to communication among scientists and their consultations with others regarding the results of their work. One may classify research projects completed for publication roughly as follows: (i) those whose validity may reasonably be determined from the report itself, by other specialists in the field; (ii) research results that require confirmation, preferably by other methods than the one used by the author; (iii) research reports whose soundness and validity may not be judged with certainty by careful study of the report itself, but where considerable thought and discussion may be necessary before even a temporary verdict may be made. The appraisal of a research report depends very largely on the category to which it belongs. Many observers outside science assume that, from the logic and rigor associated with science, category (i) completely covers the situation. By the same token, category (iii) comes as a surprise to many others. Yet some of the outstanding controversies in science originate in this last category.

Aspects of Social Control

In his conclusion the author appears to retreat somewhat from some of the tenets expressed at the outset. Thus in stating his thesis that social control in science "is exercised primarily through the reward of social recognition for contributions of information," he avoids the implication of obligation. This is more acceptable. Thus social recognition among scientists is not given as an incurred obligation in return for the original gift of information; there is no obligation here except possibly for frank comment on the value of the gift—which is indeed a rare accompaniment in gift exchange. A scientific journal is under no obligation to print a paper submitted for publication; it merely publishes papers that have been screened with respect to quality. To be sure, this constitutes recognition but only of a qualified nature. Recognition is determined by the kind and degree of attention the research receives from the scientific community, who merely feel responsibility to expose unsound work, to call attention to findings of special importance, or to cite appropriate papers in their own publications.

Actually, as a form of behavior control, social recognition among scientists does appear, but mainly in an implicit standard of qualification required to be considered a "good scientist." This is recognition by the scientist's peers. But it depends on many factors: on the critical reception of his publications, on judgments as to the quality and manner of presentation of his papers at scientific meetings, on his performance on professional panels and committees, and above all on the quality of his discussions with his colleagues in the same field.

There is strong motivation to achieve this status, which consists primarily of a tacit acceptance, quite informal in nature. Some scientific societies attempt formal recognition by the designation "fellow," and there are honor societies and academies with more exclusive qualifications for membership. But it is probably fair to say that, as incentives for an individual, this formal recognition, like awards and prizes, is less important than the feeling of acceptance by his peers.

In general, once this status is attained, the recognition form of social control is greatly relaxed and becomes more complex. Other motivations become stronger, chief among them being the

urge to solve difficult problems, competition in so doing, and the ambition to contribute to the progress of science. Without doubt an outstanding motive is simply the deep satisfaction in doing a job which one can do well.

Perhaps it may be constructive to point out that in any analysis of scientists' behavior, two outstanding characteristics of science should be emphasized. They appear to be unique in scholarly work. First of all, the establishment of the validity of a scientific finding is an objective process, as far as that is humanly possible; in general it does not depend on opinion. Einstein's special relativity theory, for example, rests on the accuracy of its factual scientific bases and the logic of its analysis, and not on any scientist's opinions regarding the theory. The rules of this game are, of course, man-made but they have been formulated so as to be as free as possible from individual opinion or bias. As a result the reputation of a scientist, as judged by his research, partakes in this objectivity.

Second, the scientific profession does not depend on the existence of scientific critics as such; the only valid critics are the experienced research workers in the field concerned. On the other hand, in most academic fields, social recognition within a discipline is accorded primarily by its scholarly critics, an identifiable class who may or may not be active in research.

Important consequences follow from this circumstance. The body of science and the career of scientists in basic research may tend to become more insulated. The structural foundation and the sound progress of science demand the utmost in objectivity, and the exclusion of desires and opinions, especially wishful thinking. The laws of nature are not subject to popular vote or to dictatorial decree. This tends further to isolate science and the scientific community from dependence on social affairs. However, at the same time, the increasing importance of science-based technological achievements and the growing dependence of scientific research on public support have introduced pressures on and strains within the scientific community which challenge this isolation and may thereby jeopardize its own standards of objectivity.

In his brief closing remarks concerning the future of science, the author mentions prevalent concern on this point—namely, the danger lest pressures and incentives from society may in some degree warp the ideals and standards of scientists. He adds as another possible concern that some eclectic disciplinary groups may in self-protection isolate themselves more or less completely from society. But he is reasonably optimistic that a satisfactory working relationship may be found between science and society. Thus far, for instance, it has been found possible for science to serve government without exercise of undue government control. Indeed, as he says, under our democratic system the identification and expression of tensions that may exist is the best guarantee of their relief.

In this concluding thought Hagstrom has touched on what is clearly a major problem for the future of science and society, one which will be watched with grave interest by the scientific community.

Astronomy

For more than 30 years, discussions of current research and review articles in astronomy and related sciences have been published in *Sky and Telescope* and its predecessors. The editors of this book, **Neighbors of the Earth—Planets, Comets, and the Debris of Space** (Macmillan, New York, 1965. 341 pp., $7.95), Thornton Page and Lou Williams Page, have selected some 113 articles, by 38 contributors and the staff of *Sky and Telescope*, and have arranged them with "historical development" as the general motif. The inhomogeneous nature of the material required the insertion of considerable commentary to provide explanation and to preserve continuity. The general level and scope can be seen from the chapter titles: "The warmer planets, Mercury and Venus"; "Mars, abode of life?"; "The major planets and Pluto"; "Asteroids: Bits or pieces?"; "Comets, so different from the rest"; "Meteors, meteorites, and meteoroids"; "Atmospheres, aurorae, and exospheres"; and "The debris of interplanetary space." The text contains numerous illustrations, but some of the photographic reproductions are very poor.

The approach attempted by the Pages seems fraught with hazards. The treatment is restricted to areas specifically covered by articles published in *Sky and Telescope* (a problem that is apparent in chapter 8), and the presentation often suffers from the journalistic style of magazine reporting (more noticeably in the unsigned articles). However, many of the articles make very interesting reading and are valuable, particularly those by the late Otto Struve.

The editors have done a commendable job in overcoming many of the difficulties inherent in such a treatment and have produced a volume that contains much of value. The historical material will be welcomed by teachers and others interested in the evolution of concepts and ideas. This is particularly true because astronomical departments tend to turn out students whose grasp of the historical roots of astronomy is virtually nil.

On the other hand, the dust jacket states that the book is "designed to inform the public of developments in astronomy that have led to space exploration and space technology." Here, success is limited, and any recommendation of the book must be rather qualified. For the more straightforward subjects, such as surface markings on Mars, the treatment hangs together and a lay reader can absorb a substantial amount of material and gain a general understanding of the subject. But it will take a rather knowledgeable, well-informed, and persistent member of the public to understand much that is treated—for example, the physics of the aurora or the higher energy solar particles in interplanetary space.

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Ibero-Americana Studies

Aboriginal Watercraft on the Pacific Coast of South America (University of California Press, Berkeley, 1965. 148 pp., $3.50), by Clinton R. Edwards, bears directly on one of the most important problems facing archeologists today—that is, was ancient man capable of making transoceanic voyages? Unlike landlubber anthropologists who tend to regard early man as essentially landbound, and view water masses as cultural barriers, Clinton R. Edwards is refreshingly sea-oriented. He justifiably points out that for many people "water has not separated places; it has joined them." Moreover, he postulates that "man had learned much about the building of adequate craft well before