File: Book Reponses

AIRMAIL

Copenhagen, Denmark
June 10, 1963

Mr. Zeev Bechler
9 Hasneh Street
Haifa, Israel

Dear Mr. Bechler:

I am sorry to be so long in answering your letter of 26 April, but a glance at the letterhead should explain the reason for the delay. You will guess that there is no sort of letter that I receive with more pleasure and pride than one which opens as yours did. That is the sort of response that makes difficult books worth writing. My thanks for the report.

It would, of course, please me a good deal if you decided to continue your studies at Berkeley and I do hope you will tell me more about your plans. (I shall be at this address until the end of July and back in Berkeley about September 6.) I do think, however, that in considering the next stages of your training, you should seriously consider other centers as well. That recommendation does not grow out of generalized modesty, but from a sense of my lack of competence in the fields that most interest you. Despite my book on Copernicus, I have little of the linguistic and other training necessary to deal with medieval sources. Nor does anyone else doing history of science at Berkeley have those special techniques. If you do mean to make the medieval sciences your specialty, you almost certainly should go to one of the men who specialize in the area. I think, for example, of Marshall Clagett of Wisconsin or, perhaps, of his student John Murdoch, now at Princeton. I am not convinced that one cannot become a medievalist without working directly under a specialist in that field, but I am far from clear that it would be a good idea to take the devious alternate route.

Sincerely,

Thomas S. Kuhn, Professor
of the History of Science
University of California, Berkeley

Zeev Bechler
9 Hasneh Street
Haifa
Israel

26 April 1963

Professor Thomas Kuhn
Department of Philosophy
University of California
Berkeley, California
U. S. A.

Sir:

I have recently read your two books and was fascinated by them. These works are like nothing I have read so far in the History of Science or in the Theory of History of Science. By saying that I was fascinated, I mean that this is the sort of work I would like to do if I were given the chance.

I am studying at the Hebrew University in Jerusalem. My two major subjects are Philosophy and The History and Philosophy of Science. I will receive my Bachelor's degree next year.

Last year I received my B. Sc. degree in chemistry from the Technion (Hebrew Institute of Technology in Haifa).

For my Master's, which I would like to complete at Berkeley, I wish to carry out a research concerning the influence of Medieval Arabic and Hebrew Astronomy on the development of thought in Europe. Since I know some Arabic and am now studying Latin, I feel that such a project will be very fruitful.

I shall await your reply and I am anxious to tell you more about my ideas and plans.

Sincerely,

Zeev Bechler

12 February 1968

Professor John J. Beer
Department of History
University of Delaware
Newark, Delaware 19711

Dear John:

Many thanks for your note of 8 February. It just happens that the job you ask me to do can be done easily, for I gave a seminar using the book as a taking off point two years ago and put together a list of some of the reviews at that time. I am enclosing a copy of the list, and it should be about what you want. Actually, it is heavily weighted towards the unfavorable criticisms, at least as compared with the statistical distribution of reviews. Nevertheless, it should give you an idea of what has gone on.

On the list, you will also notice references to several unpublished papers, one of mine and one or two by other people. If you really need them and can pay for xeroxing, it may be possible to get these to you also, but I should just as soon not circulate them any further. As to a direct response from me to my critics, I have been holding off, at least in print, until I actually have more to say. I am not impressed with the general value of authors' rejoinders. It may not, however, be an awful lot longer before I make my first additional pitch.

I shall be interested to hear how the seminar goes. Thanks for letting me know about it.

Sincerely,

Thomas S. Kuhn
Director

TSK/jt

UNIVERSITY OF DELAWARE
NEWARK, DELAWARE
19711

DEPARTMENT OF HISTORY

February 8, 1968

Professor Thomas S. Kuhn
Program in History and Philosophy of Science
Princeton University
70 Washington Road
Princeton, New Jersey 08540

Dear Tom:

I am requiring my history of science students to read and report on your The Structures of Scientific Revolutions. For class discussion I want us to read some of the critiques that have been made of your thesis in recent years. Can you refer me to some of the best of these; and to your subsequent rebuttals, if any?

Please do this only if it is easy for you to do. After all through this request I'm trying to get out of doing some hours of searching through the literature, and if this needs to be done, I and not you should do the work.

With thanks and very best wishes,

Sincerely

John J. Beer

February 10, 1970

Dr. Jessie Bernard
4200 Cathedral Avenue, N. W.
Washington, D. C. 20016

Dear Dr. Bernard:

Let me begin with the easier questions in your letter of 31 January. Time presses, and I may not get to the more difficult ones, but I do not want to let your good letter go by entirely as will probably occur if I postpone an answer.

First, there are two ways of taking your questions about the emergence of anomalies from general historical forces rather than directly from normal science. General historical factors always, I think, have a considerable bearing upon which problems of normal science get most attention from the most skilled practitioners. Therefore, external factors can have a great deal to do with determining the directions in which normal science moves, and correspondingly, the points at which, in fact, it discovers anomalies. General historical forces can also lead to crises by making practitioners feel the general irrelevance and unsatisfactoriness of what they are doing. That surely has occurred in the early developmental stages of most sciences, and it continues to be a major force in the social sciences or in some of them. I doubt, however, that it is likely to be a source of anomalies for the developed sciences, except in the sense that it could result in their failing to be practiced altogether.

Next, I definitely would speak of a division of a single community into two separate ones when describing the period in which an older generation continues to work in the old way while some of their contemporaries and the entire younger group has set off on a new track.

Third, to say that normal science is not by its nature plodding and pedestrian is not at all to say that the overwhelming bulk of the work which appears in scientific journals is not plodding and pedestrian. But I doubt that, in the hard sciences, there are anything like a majority of good papers about which one can say on first reading that they clearly will contribute nothing to the further development of their field. That sort of wastage is far more typical of the social sciences in which, as I have suggested before, there is rarely anything quite like what I have meant as normal science.

About "scientific backlash," a phrase which I fully accept. I do not mean to suggest that any scientific community is ever immune to external influences. What I am trying to suggest is that those external influences do not often have a significant effect in shaping the ideas and in determining what the problems are. They have everything to do with determining which sciences seem interesting at which point and in making certain problems seem more interesting than others.

Your point about meteorological theory is a tougher one to answer, but I can perhaps do it briefly as follows. What you say about the past population of the galaxy and the solar system could well be right, but it would also affect the utility for past times of astronomical theory. That, in turn, could be wrong. Velikovsky has insisted that it is. My point was only that, to the extent that meteorology is a theoretical science, it needs to be lumped with astronomy rather than with the social sciences.

At this point, I am sad to report that I know of no one likely to take up either of the problems in history of social science that you point to. Both sound fascinating, and I wish I could give you another answer.

Finally, your suggestion for a statement in your preface seems to me an excellent one. I shall be glad to see it, but you need feel no obligation to show it to me.

That leaves me only with your big problem, "ethics." I am sorry, but it is too big for the moment, and I shall have to duck it.

Sincerely,

Thomas S. Kuhn

TSK/ec

4200 Cathedral Ave. NW
Washington,D.C. 20016
January 31, 1970

Dear Dr. Kuhn,

Many thanks for both your letter and the enclosures, both of which are extremely interesting. I have not yet had a chance to study the preprint but even before I do I am going to take the liberty of imposing further on your time to ask a few questions and raise a few, perhaps trivial, points. To justify doing this may I say that I have been wrestling with some of these problems since you were in rompers, or before, and have been looking for new insights ever since. I note from your letterhead that your program includes the philosophy of science which, I take it, must also include its ethics. And that is what has been my own hang-up. I was raised in a tradition which took it for granted that the scientific enterprise was intrinsically humane, oriented in the direction of the welfare of mankind, in the Baconian style. Especially the social sciences. My late husband and I published Origins of American Sociology in 1942 in which I traced the so-called social-science movement which tried to hitch social reform to science in the naive belief that science would answer all the questions. A kind of social salvation via science. I believed that too until the late 1940's when we began to learn the uses to which science (anthropology as well as applied physical science) had been put. I don't think your generation can ever appreciate what a shocking and traumatic experience that was. Well, I wrote half a dozen or more articles dealing with the matter, reprints of two of which I am enclosing. But the forces which you call extrinsic in the development of science have nagged at me ever since. I think perhaps I am driven to emphasize that man should be master rather than let science be. I mean that perhaps I overemphasize the extrinsic factors.

One question I would like to raise is this: do you accept the idea that sometimes history itself throws up the anomalies that force scientific revolutions? Or does your thinking require that they ~~xxx~~ be produced only by normal-science research?

About the existence of incompatible paradigms, would you say that in the transition between an old and a new paradigm, before all the old fogies have died off, there has been a splintering of an old community into two? Or is there still one community, part of whom cling to the old and part of whom turn to the new?

I will modify the plodding and pedestrian characterization of normal-science research but I cannot wholly renege. Have you looked at any book of scientific abstracts lately? Why do we even need computer retrieval? A colleague of mine once estimated that 90 percent of all research issuing from the presses was garbage. I have found that you have to be a Madame Curie to net a centigram of good material out of tons of normal-science stuff. Is it possible that the same is not true in the hard sciences?

You say that the scientific communities you know best are now in the throes of what I would characterize as a scientific backlash. They are leaning over backward to resist influence from outside. Well, that looks to me very much like a powerful extrinsic influence, far from true insulation.

You believe that meteorological theory is just as applicable to the weather a million years ago as it is to the weather today. I respect that belief. But could you prove it? I am at a handicap because I don't know very much physical science but we do know, don't we, that the universe is not static and unchanging and to my untutored mind it does not seem impossible that there may have been astronomical products swooshing around space influencing planets and galaxies and then swooshing off again. Not all that important, to be sure, except I think the difference in time as between sociological and planetary events gives the appearance of more conceptual difference than there really might be. You know, like the charts that compare geological and human history, showing that most of what we know we just learned a few minutes ago.

Do you happen to have any students interested in the history of the social sciences? I think that would be a fertile area for exploration. In Chapter 2 of my book I present the Adam Smith paradigm -- I think it really was one -- which was the foundation for economics and the structure-functional paradigm (both Merton and Parsons call it a paradigm) which undergirds sociology. Mancur Olson, Jr., contrasted them, not very perceptively but interestingly in an issue of The Public Interest. It would be interesting to have both paradigms traced through the 19th and 20th century.

Also, are you interested in recent history of science as well as more remote? For this same book I had occasion to look through a body of research which constitutes a fascinating corpus of work which a good student might have a lot of fun exploring. It is the work done between 1950 and 1970 on modernization. Money was available and ~~the~~ researchers from all the social sciences jumped in with both feet. They were hopeful that paradigms developed in the West would apply also to the Third World. They didn't. They then threw into the hopper as candidates for paradigmatic status a brilliant array of concepts, models, laws, theories, what-have-you, to interpret and explain what was going on. Then there followed the most self-mortifying, self-flagellating mea culpas you ever heard. They fell all over one another blaming themselves for not having done better. Myrdal told them that not only were Western paradigms not useful; they were positively harmful. Sociologists accused themselves of scientific chauvinism, ethnocentrism, etc. It is one of the most interesting phenomena I know of in the history of science. Perhaps it was related to the fact that this tack would not have been followed if communities of scientists had themselves determined the direction of their research; it was followed primarily because that was where the money was. (Cynical thought). I am only suggesting that if you do have any students in the social sciences, one might find this an interesting project. I did a once-over-lightly job for the last chapter of the community book, but there's a lot more left.

on modernization

In discussing the failure to arrive at any paradigms/I had occasion to point out that the only test of a paradigm is its ability to produce consensual agreement among the relevant community. I used the analogy of the legal concept of the prudent man. What the prudent man would do is prudent behavior. What a community of scientists will accept is a "valid" paradigm. That is its only test. The community of scientists are the only ones capable of judging it; if they say, OK, that's it. Right? Or does this bother you as too "relativistic"?

Now for more practical matters. In reply to the question, what should we do? may I say that it would be impossible for me to extirpate you root and branch. But I will work out some general statement in my preface -- which I will submit to you -- to absolve you from responsibility for any use I have made of your material that might be misunderstood or wrongly attribute to you ideas you did not then or no longer hold. OK?

With warmest personal regards and best wishes for the continuing success of your very seminal work,

Sincerely,

Jessie Bernard

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THE ART OF SCIENCE: A REPLY TO REDFIELD

JESSIE BERNARD

ABSTRACT

Redfield's prescription of more humanistic studies for social-science students to counteract too great concentration on technique may not be successful, since the demand for and prestige of technique are so great in our culture. What Redfield says of the art of social science is equally true of the art of any science. Sciences are social inventions; they grow up in a very unscientific way. The choice of problems which determines the course that a science will take is personal and reflects the values of the scientist. The humanistic imagination required of social scientists is of the same kind as that required of all scientists. The social scientist has no advantage, and may have a disadvantage, from the fact that he is a human being studying human beings. Physical science can be as liberalizing and enriching as the social sciences and the humanities, depending on the way it is taught.

With most of the points made by Redfield in his stimulating article on "The Art of Social Science"[1] there will doubtless be widespread agreement. It is salutary to be reminded from time to time of the true nature of science-building. We are likely to be so immersed in the technical details of our science that the larger perspective which he provides is sometimes lost.

The chief points of divergence which I should like to make are these: (1) It is doubtful whether Redfield's proposed remedy for overemphasis on technique, i.e., more humanistic studies, is either feasible or likely to be effective; the emphasis on formal method and technique in graduate curriculums is a result rather than a cause of the great preoccupation with technique. Furthermore, humanistic studies can be as technical as the scientific. (2) Redfield writes as though what he says of the art of science-building is peculiar to the social sciences, as if they are unique in so far as science-building is concerned. The thesis here presented is that the fundamental art of science-building is the same in all the sciences and that all science-building involves art in the sense that Redfield implies. (3) Just as the social and the physical sciences require the same kind of psychological processes, so, too, do these sciences have the same kind of psychological background as the humanistic studies.

The efficacy of Redfield's suggestion of a large exposure of social-science students to

[1] *American Journal of Sociology*, November, 1948. In this article Redfield warns that overemphasis on formal method or technique in the training of social scientists may lead to the neglect of what he calls the "art of social science," that is, the creative phases, which seem to be more or less independent of sheer technique. If we identify the social sciences too closely with the physical sciences, we tend to overemphasize the technical phases, to the detriment of the creative, or artistic, aspects. He suggests, therefore, that, although we cannot teach creativity, we might encourage it by greater exposure of graduate students in the social sciences to the humanistic studies like literature, biography, and history.

the humanities as a corrective to too great concentration on technique would depend on how the humanities were taught. Humanistic studies can be taught as technically as any other. Preoccupation with grammar, syntax, and form rather than with the humane content of great classics has probably killed humane subjects for countless students. But, even granting that the humanities were taught as Redfield would want them taught, the problem is far deeper than such a remedy implies. Social scientists turn to the technical phases of their subject because the technical phases of science—any science—have tremendous prestige in our culture. There is an almost insatiable demand for techniques today. Because mores and folkways are no longer adequate for all contingencies, this is especially true in personal and social life. Scarcely a single issue of any popular journal appears without rules for something or other: how to stay young and beautiful; how to win friends and influence people; how to stop worrying and live; how to secure peace of mind; how to achieve salvation; how to find God. In view of this torrential demand it is small wonder that scientists, as well as others, step in to meet it. The scientific technician has a ready market for almost anything he can produce; he commands the best salary. People will even hire a scientist to show them what kinds of greeting cards will sell most rapidly. Hardly anyone will hire, or even finance, a scientist to study how human frailties are exploited for profit—the kind of questions that men like De Tocqueville, Sumner, and Veblen attacked.

Techniques can be taught. They are independent of the personalities who practice them. Anyone with the necessary intelligence can learn how to compute averages, measures of dispersion, tests of significance. But so far there are no techniques for teaching creativity. Since the time of Tarde we have accumulated a considerable body of sociological data on invention. We know what the conditions of creativity are. But we have not yet reduced it to a teachable skill. If we did, we might develop a technique to produce it. The sociologist may say that certain inventions are inevitable; but no invention is ever automatic.[2]

Another, more subtle, reason for the greater appeal to the young scientist of the technical aspects of science, especially sociology, lies in their relative freedom from emotional problems. Redfield speaks of "an imaginative dissatisfaction." All kinds of dissatisfactions are involved in creativity, including dissatisfactions with ends. Technical problems deal with means. The ends, or problems, are set for the technician by industry, government, editors, and employers of one kind or another. The technician is simply given a directive. He does not have to run the gamut of criticisms from his colleagues that he is dealing with values, or collide with vested interests. His evaluations can all be technical ones; such and such is a competent method for achieving your goal. He may act more or less like an automaton. He may even have a his-not-to-reason-why-his-but-to-do-or-die psychology, so that he becomes completely impersonal, detached. "Find a cheap, efficient way to kill a million human beings a year" is as valid a problem for the technician as "Find a way to reduce the morbidity rate among displaced persons." The technician is a hired hand, who shares no responsibility except that of getting an assigned job well done. He may say that he is not responsible for the use or operation of his method or the goals that are set for it by people who can afford to use it. Interestingly enough, leaders among the physical scientists are abandoning this purely technical, detached, point of view with respect to the uses of their sciences. In a charter for scientists proposed by the Committee on Science and Its Social Relations of the International Council of Scientific Unions they include the duty "to examine searchingly the meaning and purposes of the work that he or she is performing, and, when in

[2] This is the same dilemma the Marxian theorists ran into. The coming of socialism was inevitable, but it nevertheless was not to be an automatic development. Even the inevitable requires human beings to bring it about.

the employ of others, to inquire into these purposes and to evaluate the moral issues that may be involved" and the duty "to promote the development of science in the ways most beneficial to all mankind and to exert his or her influence as far as possible to prevent its misuses."[3]

In contrast to the detached, relatively easy, and heretofore emotionally irresponsible frame of mind of the technician is that of the creative scientist, who, whatever his science, is deeply involved. He insists on both choosing and evaluating his own problems. Here is one place where sociology does differ from other sciences. In the field of medicine no one seems to object to the existence of value-premises in research. If top-flight scientists choose to do research on the causes of cancer or of polio, no one attacks them as expressing value-judgments, for assuming that health is preferable to illness. It is taken for granted that the values of our culture—in this case physical health, absence of pain and suffering—are valid. The fact that other cultures may have different values does not seem to deter or even to bother medical researchers. If a first-rate economist like Keynes studies the causes of depressions or unemployment, no one casts aspersions on him for dragging in value-premises. Here, again, it is taken for granted that the values of our culture are valid. Value-premises are inherent in the selection of research problems.[4] This is judged worth doing because it will throw light on some human problem. It is only among sociologists, apparently, that this seems reprehensible. Only among sociologists does it seem like a virtue to avoid discussing problems.[5] Other sciences may accept the values of our culture without apology. Only among sociologists does the author of a good text have to apologize for using the concept "disorganization" because it implies a value-premise.[6]

In view of these facts, it is difficult to see how the students' leaning toward technique can be avoided. Technical skill is rewarded; the art of social science may, as in the case of Veblen, be punished. The prestige of technique is not the result but the cause of the curricular trends in the training of social scientists.

Science-building, as Redfield rightly contends, requires more than technique. He has made this fact stand out vividly in the case of the social sciences. "In the reaching of a significant generalization," he says, "as to man in society there is an exercise of a gift of apprehension so personal and so subtly creative that it cannot be expected to result merely from application of some formal method of research" (p. 185). An art is clearly involved. But this is equally true of the physical sciences. "To the great man of

[3] Bart J. Bok, "A Charter for Scientists," *American Scientist*, XXXVII (January, 1949), 156. As sociologists, we may shake our heads at the colossal naïveté implicit in this formulation, as though evaluating moral issues were a simple chore that one could do in off-moments or as though ways of using science most beneficial to mankind were quite obvious or as though misuse of science could be determined in every case in a clear-cut way. To the sociologist, whose data include just such problems, the answers do not seem nearly so simple as such a formulation implies. They have complex technical aspects which are cavalierly ignored in this charter. It is, however, an encouraging sign when physical scientists list such duties in their charters. Perhaps in time they will turn to sociologists for technical help in deriving techniques for implementing and even enforcing such duties. It is a question whether certain sociologists would accept such an assignment. Would not some of them—those most insistent on repudiating value-premises—insist on imposing their own values by refusing the job on the grounds that "science" itself could not justify such duties?

[4] Arnold Rose, "The Selection of Problems for Research," *American Journal of Sociology*, LIV (November, 1948), 210–27.

[5] See dust jacket, T. Lynn Smith, *Population Analysis* (1948). The book, I hasten to add, is better than this blurb would indicate.

[6] R. E. L. Faris, *Social Disorganization* (1948), p. 21 n. I venture to predict that ten or fifteen years from now, when sociologists are as secure as their scientific colleagues, historians of the subject will look back with some amusement at the almost phobic behavior many contemporary sociologists exhibit in the presence of a value-premise. Social psychologists, now that they are sure of themselves, in demand in clinic and market, are no longer afraid to come right out and say that they want to use their science to implement certain values (see Newcomb and Hartley, *Readings in Social Psychology* [1947], p. v).

science, science is an art, and he himself is an artist."[7]

Anyone accustomed to viewing science as a social institution[8] must freely grant that the building of any science, physical as well as social, is indeed an art. Creative imagination as Redfield describes it it no less essential in the physical than in the social sciences. It takes a special effort of imagination to create in any field, artistic or scientific, physical or social. Of the work of Mendelyeev, as of Darwin or of Einstein, it can be said that it is art in that these scientists created imaginatively out of their "own human qualities brought into connection with the facts before them" (p. 188). All new concepts in any science are what Redfield calls "leaps of thought" (p. 185). "By some obscure process of reason and intuition that cannot be clearly analyzed, Maxwell developed his equations. He started with mechanical concepts not unlike those of his predecessors, but in developing them he made jumps—flashes of genius—that took him outside the mechanical scheme."[9] The human qualities required to create the concept of the mores are precisely the human qualities required to create the concept of the periodic table. The data on which these human qualities operate may vary, but the human qualities—intuition, imagination, insight—are the same, whether the data they operate on are rocks, men, or molds.

Sciences are made by human beings. This truism is sometimes overlooked. We create an illusion of science, particularly of physical science, as a great impersonal machine, independent of human beings. We have the myth of the "detached" physicist. Observe physicists in action. They are no more detached than other scientists. Many are consecrated. "You are young, I am old," wrote Faraday to Tyndall, "but then our subjects are so glorious, that to work at them rejoices and encourages the feeblest; delights and enchants the strongest."[10] Have any great physicsts, actually, been "detached"? "The emotional condition which renders possible" the achievements of men like Max Planck, so Einstein writes, "is like that of the religious devotee or the lover; the daily striving is dictated by no principle or programme, but arises from an immediate personal need."[11]

Sociologists are presumably adept at seeing social behavior as it actually is rather than as it is supposed to be. Yet in this matter of science-building, or of the art of science, we often allow ourselves to be fooled. We sometimes act as though we believed sciences developed as the books on scientific method prescribe. One gets the illusion from reading scientific reports that these reports actually describe how the project proceeded or how the human mind operates.

Here Redfield's prescription of the humanities would be an excellent corrective. We can learn more about the art of science from a study of the history of science and the autobiographies of scientists than from a study of the monographs themselves. If we study the sociology of science-building, we see, not the impersonal, awesome objectivity of the monograph, but human beings acting like human beings. The stories of Archimedes and Newton are doubtless apocryphal so far as the bathtub and the apple tree are concerned. But they are undoubtedly true so far as the ideas back of them are concerned. If we watch scientists at work, we see them sitting at desks, talking, arguing, reading, driving cars, and just sitting in the much-maligned armchair. Here is one who has an idea. He got it, let us say, while tinkering with his car. He plays around with it in his mind while shaving. He jots down notes. He goes to the library and reads. The idea nags him. If it is going to involve a lot of work, he may try to shake it off. Finally it

[7] J. W. N. Sullivan, *The Limitations of Science* (Mentor Book Co., 1949), p. 166.

[8] Jessie Bernard, "Social Salvation through Science," *South Atlantic Quarterly*, XLVI (January, 1947), 44–55.

[9] Sullivan, *op. cit.*, p. 159. This process has been described analytically as "projective invention" (see L. L. Bernard, *Introduction to Sociology* [1942], chap. xxvii).

[10] Reproduced in Sullivan, *op. cit.*, p. 166.

[11] *Ibid.*

gets the better of him. He succumbs. He lays out an experimental design to test it. He goes into the laboratory to see if it works out as he thinks it should. Or here is another man in a laboratory. He notices that the bacteria he had been culturing are dead. The culture has molded. A dozen times he might curse the loss of his precious bacteria. At home, while eating lunch, it occurs to him that there may be a relationship between the mold and the loss of the bacteria and that, after all, the destruction of the bacteria is as important as the experiment he was working on. "Einstein's theory," we are told, "is so original that it is very difficult, even after the event, to provide it with an ancestry. It was not in the least a natural culmination of the ideas that preceded it. It was a bolt from the blue. . . . It seemed to be the product of an alien mind."[12] An outstanding plant biologist tells us that out of "the dozen or so really good ideas I have had during the past forty years I can remember the time and circumstances of the origin of three. One came while I was helping load sweet corn to be hauled to a cannery in southern Maine, one while I was crossing Copley Square in Boston on a bitter cold morning, and one while I was bouncing around in an upper berth of something called a sleeping car between Pittsburgh and Washington."[13] A colleague of his, apparently, did a good deal of his scientific work while sleeping at his desk.[14] Some important scientific work results from simply playing around with data. Any accumulation of data may challenge a creative mind. Such a mind mulls over records and waits for ideas. It can start without the prescribed hunches or hypotheses, just convinced that there is something valuable in the records. The hunches and hypotheses follow. Sometimes a project veers off into quite a different direction from the one imposed by the researcher at the beginning, just as, so we are told, characters in novels sometimes take matters into their own hands and proceed according to their own inner logic.

In brief, the great creative ideas in any science occur to people in a wholly unscientific way. Having such ideas is doubtless psychologically the same in all creative fields. One can imagine a social scientist, a physicist, and a poet all crossing Copley Square, Boston, at the same time. So far as outward appearance and behavior are concerned, they may seem indistinguishable. Yet one is creating a sociological concept, one a physical concept, and one a poem. The realization of these great creative thoughts will vary widely; the art of creativity is not so different in the three cases.

The artist-scientist is primarily interested in asking the most pertinent and meaningful questions. He creates the problems of his science and thus charts its course. But the selection of problems is a social as well as a personal act. Exaggerating the differences between the physical and the social sciences tends to make us forget that all sciences are social in the sense that they are human, therefore social, inventions and that science-building is a social activity like any other social institution. The scientific problems which men attack do not impose themselves on men. Men choose them. And men reveal their values in the kinds of problems that they choose, as artists do. The personality of the scientist is as clearly revealed in the problems he selects, the questions he asks, the choice of methods he makes, and the interpretation of his results as is the personality of the artist in his design. Any of the sciences could, presumably, have followed any one of a number of courses. "The history of science is not the history of some sort of automatic development. The actual course that science has pursued depends very largely on the types of mind which, as historical accidents, happen to have risen to the level of genius at favorable instants."[15] It was not foreordained that mathematics, for example, should develop as it did. It developed in this manner because the culture that served as its matrix developed in a certain way and

[12] *Ibid.*, p. 168.

[13] Neil E. Stevens, "Fun in Research," *American Scientist*, XXXVII (January, 1949), 121.

[14] *Ibid.*

[15] Sullivan, *op. cit.*, p. 169.

because mathematicians were the kind of people they were. "Poincaré was led to divide mathematicians into psychological types, and to point out that a kind of reasoning which would convince one type would never convince another. These differences, in his opinion, are fundamental, and play a great part in the actual construction of science."[16] All sciences followed the paths they did because men chose one set of questions rather than another to probe.[17]

In discussing the nature of genius with my classes I sometimes raise this question: Would Marie Curie still have been considered a genius if radium had turned out not to have the remarkable properties which it did turn out to have? There is usually some discussion, and then one student makes the remark that the question is misleading, that she showed her genius in selecting for study an element that did have remarkable properties. In other words, scientific genius reveals itself in the choice of problems that it makes as well as in the technical brilliance which it shows in tackling them. And this ability to see and to formulate meaningful scientific problems is an artistic gift.[18]

The technicians who inherit the results of the artist-scientists' creative imagination may never realize the debt which they owe to the men who map out problems. Once the questions have been asked, it seems that they were always there. But if the technicians are dependent on the artist-scientist, so, too, is the latter dependent on the former. If the technicians do not develop methods to test and measure the artist-scientists' concepts, the concepts may die or at least remain inert. No more than Redfield do I wish to decry formal method. Without it, work may be great, as was De Tocqueville's, Sumner's, and Veblen's. But, without it, work may also be very bad. Illustrations are mercifully omitted.

With Redfield's point that social scientists gain more than physical scientists from exposure to the humanities most people will probably agree, although not necessarily for the reasons which he states. Exposure of the social scientist to all kinds of data about people and cultures is valuable because people and cultures are the subjects that he studies. Exposure of a physical scientist to the humanities will not help him because his data are not in the field of the humanities. What Redfield is arguing essentially is that the social scientist must have great familiarity with the data and problems of his field as well as with its techniques. It so happens that he can gain perspective and stimulation from novels, philosophy, history, biography.

If, then, by "humanistic imagination" Redfield means an imagination filled with humanistic facts, there can be little dissent. The more one knows, the more play his imagination has. But, if Redfield implies that a humanistic imagination is psychologically or sociologically different from the imagination required in the physical sciences, some people will disagree with him. The fact that the data of social science are human beings and their relations does not necessarily, as Redfield argues, mean that the scientist must approach them differently, "must become a part of the human relations he studies." It may be necessary to

[16] *Ibid.*, p. 171.

[17] Anyone who has ever supervised student research knows, in a minor way, the intimate relationship between the researcher and the problems that appeal to him. One presents a student with four or five possible projects. Not interested. At mention of the next one, however, his face lights up. He likes it. He wants to do it. The same thing occurs in all the sciences. That, incidentally, is why freedom is so essential to the art of science. The artist-scientist has to choose what is congenial to him; he is involved, and his personality is part of his method. This is not necessarily true of the technician, who is more detached.

[18] Graduate students in one great sociology department have been known to complain that, once they had found a suitable subject for a doctoral dissertation, there were great resources of talent and skill available to them, but there was no one to help them formulate the problems. Perhaps this is as good a way as any to force future technicians to appreciate the art of science. It may even have more merit than Redfield's suggestion that the graduate student's curriculum be reoriented.

undergo an analysis to become a psychoanalyst. But one does not have to be a criminal in order to become a criminologist; a sick man, to study disease; a parent, to study family relationships; a Negro, to study Negroes; a Jew, to study Jews. The social sciences are no different from any other science, no more humanistic in their demands on human nature. One can be mechanical or imaginative in any science, even in the humanities. "One may be taught how to pursue a course of questioning, how to map a neighborhood, or how to tabulate and treat statistically the votes cast in an election; but to know how to do these things is not to be assured of meaningful conclusions" (p. 186). To be sure. Likewise, one may be taught all the laboratory skills of modern physics; but to know how to do these laboratory chores is not to be assured of meaningful conclusions. The artist-scientist in any field contributes the meaningful conclusions, no less in the physical than in the social sciences and humanistic studies. All sciences require such humanistic qualities as insight, intuition, imagination, the ability to create and to generalize. Newton mulling under the apple tree, Archimedes soaking in his bathtub, are performing exactly the same kind of humane or artistic function as Cooley mulling in his study at Ann Arbor, or Sumner at New Haven, or Veblen at Cedro, and all resemble Proust pouring over his pages in his stuffy Paris apartment. The human psychological or humanistic qualities back of any great and illuminating generalization are the same regardless of the data that they operate on. The secret of creativity in any science, physical or social, is to fill the scientist full of tested data and then let his imagination play over them. Creative insight is, as Redfield insists, a humanistic art; but it is as important in the physical as in the social sciences.

"To find out the nature and significance of human nature there is," Redfield tells us, "no substitute for the human nature of the student himself." This is undoubtedly so. Yet it can also be said that there is no substitute for human nature in any science. No science is impersonal, independent of the personalities who create it.

> Einstein's theory is perhaps the clearest instance that can be given of our assertion that even a scientific theory may possess a personal element. . . . It is precisely this quality of originality which is the personal element in science. . . . It is . . . in the selection of original problems that the personal element in the creation of a scientific theory is most clearly revealed. . . . In view of these facts it is obviously misleading to present science as differing fundamentally from the arts by its "impersonal" character. There is no absolute difference here, but only a difference of degree. Science is less personal than the arts, but it is a mistake to suppose that it is wholly impersonal.[19]

The fabric of a science contains the mind-stuff of its creators, part of the essential personalities that shaped it. And this applies to the physical sciences as well as to the social sciences and the humanities.

The social scientist, Redfield contends, must use his own humanity to understand humanity. In reply to this argument—that, inasmuch as the social scientists are dealing with human beings, they have a special advantage over the other sciences—it has been argued with seemingly equal cogency that, rather than being a special advantage, this fact disqualifies the social scientists from truly scientific observation and thinking; for, in addition to all the other disciplines demanded by scientific work, the social scientist must discount himself as a human being with a full complement of biases. He has to observe as nearly as possible as a nonhuman being. Who is better qualified to give a scientific analysis of a primitive language, the trained linguist who creates a grammar for it or the native who has spoken it from birth? The fact that his informant is a human being, that his data consist of the behavior of human beings, renders the scientific problem of the linguist not less difficult than that of the entomologist whose data consist of the behavior of insects. He has no

[19] Sullivan, *op. cit.*, pp. 168–71.

special advantage because both he and his informants are human beings.

The fact that the social scientist is a human being studying human beings has, in fact, so the argument continues, led him into the great morass of internal motivation. The social sciences still carry about with them the incubus of preoccupation with subjective "Why?" which no science has ever been able to survive. It is only when sciences shed this preoccupation with subjective or internal why-questions that they achieve maturity as sciences. If social scientists were not human beings studying human beings, they would not be so obsessed with the necessity for such concepts as instincts, wishes, drives, needs, etc. No respectable social scientist imagines that the answers which he gets when he asks people "Why?" represent the true why. The replies to questions of why are treated like any other kind of observable data. The investigator can tell the why of behavior better than the subject can, and he gets at the why exactly as any other scientist gets at the why of his data, by putting together a lot of objectively observed fragments into a systematic whole that makes sense. The individual human being does not know why he does things any more than a drop of water does. The social scientist, who studies the culture, the socializing process, intergroup relations, etc., knows much better why his subject does things, just as the physicist knows why the drop of water behaves as it does. De Tocqueville, Sumner, and Veblen knew better why Americans behaved as they did than most Americans did before they had read De Tocqueville, Sumner, and Veblen.

It is true that "social science . . . 'liberalizes' or 'enriches' " (p. 190). But so do the other sciences, as Spencer, among others, so eloquently argued in the nineteenth century. Laplace's celestial mechanics, Darwin's *Origin of Species*, Gauss's normal curve (which Quételet said the Greeks would have deified if they had known it), even Einstein's famous equation, have liberalized countless minds. Any creative scientific generalization "does things" for the perceptive student, "liberalizes," "enriches" him. "We may say that the chief part played by science in helping on the developing consciousness of man is to be found in the new thoughts that it has made us think."[20] To arrogate to the social sciences any pre-eminence in this field, as Redfield does, might seem to outsiders a kind of scientific parochialism, if not chauvinism. It is, one might add, as true of the physical as of the social sciences that they (as well as the humanities themselves) have not always been taught in a liberalizing manner. Technique and formal method, rather than creative generalization, have been emphasized, so that all too often the student is denied the enriching contribution which science can make. This is a defect in teaching in no matter which field of learning or of science that it occurs. It turns out journeymen rather than enriched minds or creative artists. Conant, himself a physical scientist, has argued for the teaching of science at Harvard as a liberal rather than as a technical subject. This movement, if successful, will doubtless spread, once its merits are demonstrated, and so the teaching of all sciences will be liberalizing.

It should be pointed out, however, that, if one chooses to aim his science toward liberalizing the mind, he can doubtless succeed; but he will be in the company also of writers like Adolf Hitler, Marx, and Engels. For *Mein Kampf* and the *Communist Manifesto* were liberating to many more millions than *Folkways*, *Democracy in America*, or *Theory of the Leisure Class*. Scientific generalizations and perspectives are liberating; novels are, too; but so, likewise, can be the most palpable nonsense. The ability to "do things" for the human spirit belongs to many departments of creative effort. It is no test of the merit of a work.

What Redfield has done, in effect, is to contrast great creative works in the humanities—*War and Peace*, *Remembrance of Things Past*—with the technical aspects of

[20] Sullivan, *op. cit.*, p. 177.

the physical sciences, and he has come out with the expected results. But he could with equal validity have compared some of the great creative works in the physical sciences —Newton's *Principia* or Laplace's *Celestial Mechanics*, for example—with the technical aspects of some humanistic studies, such as the use of the ablative absolute in certain Latin prose writers. The true contrast, then, it seems to me—and it holds in any field of learning—is between the creative or artistic, on the one hand, and the technical, or even mechanical, on the other. Perhaps the solution to the problem that Redfield raises is enough emphasis on the creative aspect of any subject—physical science, social science, or humanistic studies—to lead to an appreciation of the art of science by students destined to become technicians.

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SCIENTISTS AND THE PARADOX OF POWER*

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INTRODUCTION

WHEN I first conceived this paper it was in terms of another title, "Some Non-rational Factors in the Growth of Science." By non-rational I did not mean irrational, but rather factors not inherent in the logic of science itself. Comte's theory of the filiation of the sciences, for example, his principle of increasing generality, is a rational principle. And it is certainly logical that no science can develop until the prerequisites have been worked out: no matrix algebra until algebra has evolved; no projective geometry before Euclidean geometry, etc. Nor can we expect a science to develop until the problems it deals with have arisen. No urban sociology before the rise of cities, for example; no labor economics before the development of an industrial economy. The dependence of modern science on technology is also a rational matter. From the simplest retort to the cyclotron, science is integrally dependent on the tools and equipment available. There is, then, a certain rationale in the history of science; concept succeeds concept; science generates science, in an orderly way.

But it is not necessarily the only orderly way. For there are certain non-rational factors involved in the growth of science also. And it was about these non-rational factors that I had at first planned to write. Such factors, for example, as (1) those of selection. Why do some concepts "take" and others not? Why in sociology did primary group, mores, ethnocentrism, for instance, "catch"; but tentative method or sympodal development or priestly versus prophetic trends, or bohemian versus philistine, not? Why in chemistry was Rey's work on calcination ignored for 150 years and then accepted? Sarton suggests that some theories are preferred to others because they are more beautiful.[1] Conant says that "a scientific discovery must fit the times,"[2] and C. Leonard Huskins, reviewing this selective phenomenon in genetics, agrees that "timing . . . is almost the sole criterion of significance."[3] But timing itself is a complex phenomenon. What *makes* timing correct or incorrect?

(2) What, to continue with non-rational factors, is the influence of authority, even in science? Huskins illustrates half-truths retained in science textbooks simply on the basis of the prestige of some great scientist. In sociology we find textbooks repeating the statement that divorce is a middle-class phenomenon, in spite of the evidence to the effect that it is more characteristic of the lower-middle and lower socio-economic classes. Margaret Mead's generalizations are repeated everywhere; refutations are ignored. It would be interesting to know why.

(3) The influence of fashion in both subject and method is an intriguing one. Subjects and methods wax and wane in the interest they evoke. Why?

(4) The influence of cliques strikes me as worthy of study. Whom are we trying to impress? A doesn't care at all what B thinks of his work; he would be deeply wounded if C did not approve. It becomes conventional to cite certain names; they reappear in footnotes in articles and books and monographs; equally relevant and pertinent works are ignored. It would be much too simple to say that the cited ones are always better than the ignored ones.

(5) The impact of textbooks seems to me especially important in the growth of science. Many research scientists look with undisguised contempt on the "mere" textbook writer. Yet the successful textbook writer probably has more effect on the development of a science than most individual researchers have. He is the selector and integrator par excellence. He mediates the past to the present; he introduces the past to the student. He stands at the gate and decides, within limits, which men, which ideas, which theories shall be admittted.

(6) The role of the scientific society in the development of the physical sciences has been recognized and documented historically. A similar task has been begun in the social sciences.[4] Meetings

* Presidential address read before the Eastern Sociological Society, April 5, 1952, at Haverford, Pennsylvania.

[1] *The Life of Science* (1948), p. 42.

[2] *Understanding Science* (1951), p. 89.

[3] "Science, Cytology, and Society," *American Scientist* (Autumn 1951), p. 696.

[4] L. L. Bernard and Jessie Bernard, *Origins of American Sociology* (1942).

of such societies give us a chance to re-enforce our conception of our role; they polish the mirror which reflects back our looking-glass self; they encourage and they discipline us. The effect lasts from one meeting to the next. We know who is looking over our shoulders as we write, research, and teach, and we dare not do anything that will cause our fellows to look down their noses at us when we come to meetings.

(7) There are other more or less minor non-rational factors in science that invite interest also, like the creation of the metric system by the French revolutionaries as "a reaction against the 'foot of the king' of the ancien regime,"[5] etc.

All of these are interesting and important. But there is one which seems to me to transcend them in significance and that is the role of the scientist as it has come to be defined in our culture and as it is in process of redefinition at the present time. The implications of this role have wide and paradoxical ramifications. The paradox is this: the role—here conceived as the integration of mores, customs, traditions, conventions, etc. which prescribe the expected behavior of people performing certain functions—the role of scientist selects and then proceeds to exaggerate certain qualities in its professors. It selects a man not interested, or at least not talented, in the exercise of power; yet the men thus selected are the creators of the greatest reservoir of power ever known to man—science.

THE EMERGENCE OF THE ROLE OF SCIENTIST

The definition of the role of scientist emerged gradually in the seventeenth and eighteenth centuries and achieved a kind of stability in the nineteenth and early twentieth. For about two hundred years scientists, or *virtuosi*, were amateurs; science was a gentleman's ornamental hobby, or an avocation of the clergy, or a nobleman's diversion. Not until the nineteenth and early twentieth centuries did they become professionals. By that time the role of scientist had evolved to the form now familiar, even stereotyped. The role included a great many mores, conventions, etc.—I have elaborated some of them at length in another place—but implicit in all of them was the acceptance of the belief (1) that controversial issues—which are as inherent in science as in all other aspects of life—were not to be met by power tactics but by an impersonal recourse to facts, and (2) that science must be used for the welfare of mankind, that its horizons were worldwide, international, not limited to group interests. These were the prescribed attitudes for scientists.

The conception of the scientist's role did not arise overnight. The body of tradition which came to define the scientist's role had a long history. With respect to the development of the role of the scientist in the face of conflict, Conant remarks that:

> ... in the embryonic stages of each of the modern disciplines, violent polemics rather than reasoned opinions often flowed most easily from the pen.... It was only gradually that there evolved the idea that a scientific investigator must impose on himself a rigorous self-discipline the moment he enters his laboratory. As each new generation saw how the prejudice and vanity of their predecessors proved stumbling blocks to progress, standards of exactness and impartiality were raised. But as long as science was largely a field for amateurs, as it was well into the nineteenth century, a man could regard his discoveries like so many fish. If he defended their size against all detractors, and in so doing their length increased, well, his opponent was a well-known liar, too. The formation of the scientific societies, their growing importance, and the gradual building up of a professional feeling about science slowly changed the atmosphere.[6]

The impact of scientific societies was, thus, important in this evolution of the concept of the proper behavior of a scientist. Crowther, summarizing Thomas Sprat's history of 1667 of the British Royal Society's first four years, points out that

> ... the Fellows avoid eloquence in the description of their experiments. This quality is "fatal to peace and good manners." They have resolutely rejected all extravagance in expression, all "amplifications, digressions and swellings of style." They aim at a "return to the primitive purity, and shortness, when men delivered so many things, almost in an equal number of words." The Fellows are expected to use "a close, naked, natural way of speaking; positive expressions; clear senses [sentences?]; a native easiness: bringing all things as near the mathematical plainness, as they can: and preferring the language of artizans, countrymen, and merchants, before that of wits or scholars."[7]

[5] Sarton, *op. cit.*, p. 35.

[6] *Op. cit.*, p. 22.

[7] Crowther, *The Social Relations of Science* (1941), p. 377.

+ "Social Salvation through Science," South Atlantic Quarterly (1946), 47:44-55.

Little by little the behavior proper and becoming for men of science became conventionalized:

> The example of the few giants who, beginning with Galileo, had well recognized the need for self-control became the accepted standard. The man who was inclined to use the same weapons in "philosophical" [read "scientific"] as in political and theological debate gave way to the modern scientist who places little reliance on persuading his opponent with rhetoric or driving him from the field with invective. For his jury today is a large body of well-informed peers and to them he need only present exact and impartial facts with the minimum of emotion.[8]

The role of the scientist, like any other role, has great control impact; it is independent of the specific individual who plays it.

> Would it be too much to say [asks Conant] that in the natural sciences today the given social environment has made it very easy for even an emotionally unstable person to be exact and impartial in his laboratory? The traditions he inherits, his instruments, the high degree of specialization, the crowd of witnesses that surrounds him, so to speak (if he publishes his results)—these all exert pressures that make impartiality on matters of *his* science almost automatic. Let him deviate from the rigorous role of impartial experimenter or observer at his peril; he knows all too well what a fool So-and-so made of himself by blindly sticking to a set of observations or a theory now clearly recognized to be in error. . . . Once science became a self-propagating social phenomenon, those who till these fields have had a relatively easy time keeping up the tradition of their forebears.[9]

We shall see presently how the absence of this support from a role influenced Lysenko and the reaction it aroused throughout the world of science. But first a word about the second assumption inherent in the role of scientist—that of the humanitarian function of science.

A team of my students once canvassed the literature on science for statements dealing with its functions. The results included such findings as these: It is the function of science to alleviate human suffering, to aid in the adjustment of mankind, to add to the comforts of life, to push back the boundaries of human ignorance, etc. When these idealistic statements were submitted to a group of future professional scientists, they endorsed them. One need not, of course, be naive enough to suppose that these replies represented the "real" beliefs of scientists. Whether or not they do is beside the point, which is that these are the attitudes with respect to the functions of science which were considered proper, legitimate, correct. Acceptance of them was felt to be part of the role of the scientist.

Alchemy, in contrast to science, selected a person openly and avowedly interested in the exercise of power. He was, in some ways, less naive than the person selected by the role of scientist. This fact may, indeed, explain in part the failure of the alchemist. Yet he was far more aware of the facts of life so far as power phenomena are concerned than the scientist. He kept his craft secret—perhaps another reason why he failed—because he did not want it to fall into evil hands. Preliterates distinguished between black magic and white magic. But until recently we made no such distinction in science. Science was white; there was no black science. Its "attitude is tolerant, friendly and humane."[10] (As a matter of fact, scientists from Archimedes to Galileo to da Vinci to Fermi have contributed to war as willingly as to other programs; war was even justified by the militarists as tending to stimulate science.) The general consensus has been that science was a good thing whose function it was to help mankind.

The role of scientist is taken so much for granted that it can be observed better in the breach than in the practice. The notorious Lysenko controversy is an excellent illustrative case. As Julian Huxley has so forcefully pointed out, the issue here was not simply a difference in theoretical interpretation of data.[11] Other scientists have, from time to time, offered data which seemed to them to call for some acknowledgment of the inheritance of acquired characters. William McDougall, then of Duke University, for example, once argued that wild rats after several generations of laboratory handling were gentled more easily than their forebears. He followed strict scientific conventions in presenting his data and no commotion ensued in the world of science. A scientist, so long as he behaves according to his prescribed role, may offer novel and unusual theories without any untoward reaction on the part of his fellows. The reason Lysenko provoked so much emotion was that he violated the most cherished canons of scientific behavior. He approached controversial issues in a

[8] James Conant, *op. cit.*, p. 22.

[9] *Ibid.*, p. 24.

[10] Foreword to C. H. Waddington, *The Scientific Attitude* (1948).

[11] *Heredity East and West* (1949), pp. 35–62.

manner which, as Conant's and Crowther's quotations above indicated, violated the role of scientist. He used in "philosophical," that is, scientific, debate the same weapons used in political debate, namely ridicule, name-calling, glittering generalities. He relied on "persuading his opponent with rhetoric or driving him from the field with invective . . .," a mode of behavior long since proscribed by the role of scientist. He rejected the use of certain techniques—statistical ones—on prejudiced rather than on reasoned grounds. He did not publish his results in a form to render them reproducible by other scientists for validation and check. He made utility the sole criterion of the value of scientific work; he exalted the use of science for Soviet rather than for human ends; he broke down the century-old tradition of internationalism in science. . . . This was the bill of particulars against Lysenko; these the charges. It was not his strictly methodological lapses which aroused the scientists of the Western world—his lack of controls, lack of tests of significance, and lack of care for purity in his experimental strains. These were pardonable and might well have been forgiven. They were technical in nature. Even Western scientists might be guilty of such technical errors. They can be corrected. It is, indeed, part of the code of scientists to point out such technical errors to their fellows. If the mores and conventions which constitute the role of scientist are carefully followed, the technical errors will invariably be caught. And if the scientist plays his role he will correct his own technical errors. But if, as in the case of Lysenko, he is repudiating the whole role of scientist as it has come to be defined over the last 200 years, he cannot be reached; he is beyond the control of his fellows. In this particular case, he has submitted himself to some other control.

The role of scientist, then, as defined until recently in the Western world prescribed as ideal-typical a dispassionate, disinterested observer, above the battle. He was not oriented toward power or toward the manipulation of people or the amassing of wealth. Aloofness and detachment were part of the price he paid for tolerance and immunity, "a condition of complete intellectual honesty."[12] He was a man whose integrity could never be impugned. He had no prepossessions. He made no ethical judgments regarding his findings although it was assumed that science itself had an ethical function, namely the welfare of mankind. His loyalty was to mankind rather than to any special interest. He was a citizen of an international community. He was a man of good will, for science was conceived of as an instrumentality of good. He was "the rational man" of the eighteenth century Age of Reason, brought within hailing distance of reality.

[12] A. V. Hill, *Nature* (1933), p. 952.

THE ROLE OF THE "POLITICIAN"

A much older type, whose role has been variously defined from time to time, is that of the man who exerts power or who would like to. For brevity's sake we may call him "the politician" with the understanding that no derogatory connotations are to be attached to the term, that it is not to be considered a smear word. The fact that this whitewashing of the word is necessary, that we have no clean term for men who are interested in power, is itself a profound sociological phenomenon reflecting as it does our attitude toward this function. In our society we act as though the scramble for power were a reprehensible thing. We are, after all, anarchists at heart. If all men were rational—like us—we wouldn't need politicians; we wouldn't have fights over policy; we would submit our differences to "science" and whether we liked the results or not we would abide by them. When, for example, we showed that a segregated system of schools was so expensive that all children suffered, those in power would quietly agree and open all schools to all children. Or when we demonstrated that commercialized gambling demoralized men and communities, Costello would stand up and apologize and close all his establishments. Or when we proved—as Adam Smith actually did—that perfectly free trade throughout the world would increase the sum-total of wealth produced, all nations would automatically get rid of restrictive policies. The role of scientist has selected men who might be capable of such behavior. But we have only to look about to see that the role of scientist is not congenial to everyone. Some people are different from us.

Some people are politicians. They enjoy the maneuvering, manipulating, and jockeying inherent in the great game of power. The role of the politician—in the clean sense—has changed from time to time. The use of naked force and murder is no longer *comme il faut*; it is looked down upon; proper politicians wouldn't touch it with a ten-foot pole. Finesse in both the competitive and the

conflict areas is the fashion today. Machiavelli was one of the first to codify the role of the modern type of politician. (And it is interesting that even Machiavelli has come to stand for evil.) It has acquired enough acceptance so that when liquidations are used today they are kept as secret as possible. A self-respecting politician nowadays doesn't like to have it known that he has to resort to purges. If he is good enough at his trade he shouldn't have to. Having to reflects on his competence.

THE SELECTIVITY OF ROLES

Roles select certain types and then proceed, so to speak, to exaggerate them. The role of scientist as it has evolved tends to select a non-controversial type of person who does not like to engage in the great game of power. It seems so unnecessary when science could determine policy so much better if people would only submit to it. Democritus is quoted as saying that "the trouble with scientists is that they take no interest in politics." The kind of men who are not interested in politics are the kind of men who are selected by the role of scientist; and the role proceeds to exaggerate their lack of interest in the practice of power. Some scientists, to be sure, become businessmen, for, little by little, contract research is migrating out of the universities into private laboratories. Deans and department heads and other scientists who have a flair for administration and a taste for power soon learn that administering a research business is not too different from administering a department, and much more profitable. They go into business for themselves. Others go into administrative jobs in government and in private industry; some even into policy positions. Those who remain, who continue in the role of practicing scientists, are confirmed in their personal behavior preferences. In a sense they become incapacitated for conflict. Schooled to submitting controversial matters to the test of experiment or controlled observation, they do not easily catch on to the mechanisms—note I do not use the smear word machinations—of power.

Those of us who teach in land grant colleges have evidence for judging the success of the role of scientist in its control function, for we live in communities characterized by men who represent this role. Conant tells us that his observations lead him "to conclude that as human beings scientific investigators are statistically distributed over the whole spectrum of human folly and wisdom much as other men."[13] I am not so sure. My own observation is that they are an extraordinarily high type. I live surrounded by them—by chemists and physicists and biologists; I sit on committees with them; I attend PTA meetings with them; I square-dance with them; my children play with their children. I am impressed by their qualities and by their contributions to the community.

But I note also that by and large they do not rebel against the idea of loyalty oaths; they do not protest administrative actions; they do not organize to fight authority. They look upon those who do these things as radicals, if not agitators and hotheads. On administrative committees they are likely to be company men. I have seen scientists exploited without their even knowing what was happening to them; they even smiled and were pleased; they did not know they were being manipulated. All they wanted, figuratively speaking, was to get back to their laboratories.

The role of politician—read again "man interested in the exertion of power"—selects a quite different type, and I disagree with Conant to the extent of believing that the man of science is a different type from the man of power. The distribution of relevant traits would, I believe, be bi-modal.

Well, so what? What if this *is* true? What if the role of scientist *does* eviscerate men, incapacitate them for the struggle for power? Other occupational and professional roles also select types not interested in power. Is there anything wrong with that? There's too much struggle for power in the world anyway. Too many people are in it. It's a good thing that some professions bow out.

THE MAN OF SCIENCE AND THE MAN OF POWER

This is what of it. The scientist is key man in control of the greatest power in the world today, the power of science. Men who want to control that power are not going to permit the scientist to remain aloof. Nor are scientists in a position to withstand them. The man of reason is incapable of using or exploiting the non-rational man, but the man of power can and does and increasingly will exploit the man of science.

Bernal tells us that "it was admittedly not very difficult for the Nazis to win the loyalty or at least the obedience of the scientists—far easier in fact, than to get that of the Churches. For that

[13] *Op. cit.*, p. 23.

most of the blame," he thinks, "must be laid on the character and training of the scientist."[14] They were less alert to the threat of this new power to their own interests than were churchmen. The Nazis were fundamentally opposed to the whole theory of science; not dispassionate objectivity but passionate patriotism was their ideal. They were the archetype of if not a travesty on the man of power. Yet military power rests on science. They were therefore "faced with the paradox of having to maintain their strength by the use of methods which they despise[d]."[15] Unlike Nazism, which despised science while having to recognize its dependence on it, Russian communism exalts science and makes it a cornerstone of its programs. The scientist is coddled; he is encouraged; he is even pampered. In return he must supply the scientific bases for economic and political programs, including "the provision of the historical and social theory for combating the ideas of capitalism and dissolving the prejudices which survive in the minds of the people and have been transmitted from earlier forms of society."[16] As in Germany the scientist has been kicked around in the struggle for power. "The violent contests over political policy have been reflected," we are told, "in the life of the [scientific] institutes and, as in other departments of the state, many scientists have been imprisoned, and some have been shot. Insufficiently educated enthusiasts sometimes denounced scientific theories because they were advocated by persons whose political views were regarded as inimical to the state. The scientific researches of persons disapproved by the political authorities have sometimes been omitted in lists of references, and scientists have sometimes been seen apologizing to the political authorities for having held opinions which appear to the majority of the scientists of the world to be correct."[17] Vis-à-vis the man of power, in other words, the man of science caves in.

We usually associate the power of science with the power which results from control of physical and biological processes. But the social sciences have their atomic power too. A preministerial student said to me last summer that he had been warned against sociology because all it was interested in was control. Sociologists wanted to control everything. They were, in effect, power-ravenous people. Comte established a long tradition. Ultimate control of social phenomena has been the justification used by social scientists for their efforts for a long time. The nineteenth century social psychologists taught Hitler much that he knew about control. There comes to my desk a periodical which classifies research under such headings as these:

Attempted Production of Change in an Individual by an Individual
Attempted Production of Change in an Individual by Group Activity
Attempted Production of Change in Group Situations by an Individual
Attempted Production of Change in Group Situations by Group Activity
Factors Limiting the Potentialities of Change in an Individual
Factors Limiting the Potentialities of Change in a Group
Attempts to Establish Behavior Uniformities in the Individual
Attempts to Establish Behavior Uniformities in Group Situations

Some of this research is no doubt trivial; some of it irrelevant; some of it mere fact-amassing. Yet little by little, brick by brick, we are creating a science of power. I am sure that none of the earnest researchers whose works are listed feels any relationship between what he is doing and the power struggles of the outside world. They are doubtless all disinterested scientists who have human welfare at heart, in the old tradition of science. They want to help people, to see their work used for good ends—to get rid of prejudice, industrial conflict, crime, corruption, what-have-you.

But their work will be used for any ends for which men of power wish it to be used. Short of the alchemist's secrecy, which science definitely repudiates, there is no way to license science, to prevent it from being put to any uses people can afford to put it to. Bacteriology can be used to discover the causes of disease; it can be used in bacteriological warfare. This is how it looks to Bertrand Russell:

> It may be hoped that in time anybody will be able to persuade anybody of anything if he can catch the patient young and is provided by the state with money and equipment. This subject will make great strides when it is taken up by scientists under a scientific

[14] *The Social Function of Science* (1939), p. 214.
[15] *Ibid.*, p. 218.
[16] Crowther, *op. cit.*, p. 553.
[17] *Ibid.*, pp. 556–557.

dictatorship. Anaxagoras maintained that snow is black, but no one believed him. The social psychologists of the future will have a number of classes of school children on whom they will try different methods of producing an unshakable conviction that snow is black. Various results will soon be arrived at: first, that the influence of home is obstructive; second, that not much can be done unless indoctrination begins before the age of ten; third, that verses set to music and repeatedly intoned are very effective; fourth, that the opinion that snow is white must be held to show a morbid taste for eccentricity. But I anticipate. It is for future scientists to make these maxims precise and discover exactly how much it costs per head to make children believe that snow is black and how much less it would cost to make them believe it is dark grey. Although this science of mass psychology will be diligently studied, it will be rigidly confined to the governing class. The populace will not be allowed to know how its convictions are generated. When the technique has been perfected, every government that has been in control of education for a generation will be able to control its subjects securely without the need of armies of policemen.[18]

I do not pretend to know what the answer to this paradox is. Some argue that policy makers should become scientific; others argue that scientists should become action people. Both alternatives are probably impossible. Policy making can never be reduced to a science; and the role of the scientist probably precludes his becoming an action man. Huskins puts it this way:

> Here we encounter a real problem: how determine policies without being a politician? And how can a good scientist be a good politician? For the essence of the politician's art is to make people think as he wants them to think. If he is a low-class politician, he interprets and expounds the facts in ways that serve his own ends. If he is a "statesman," he interprets and expounds them in ways he and his party think best for the public good. The first and last job of the scientist, on the contrary, is to find out what impressions the matter he is studying make upon the senses of persons who are judged to have (1) adequate training imposed upon a "normal" set of sense organs, and (2) a brain capable of interpreting these impressions "normally."[19]

On the solution of this paradox, if there is one, depends to a large extent the future welfare of mankind.

[18] *The Impact of Science on Society* (1951), p. 32.

[19] "Science, Cytology, and Society," *loc. cit.*, p. 699.

26 January 1970

Dr. Jesse Bernard
4200 Cathedral Avenue, N. W.
Washington, D. C. 20016

Dear Dr. Bernard:

I am extremely sorry to have taken so long to respond to your letter of December 10 and the manuscript which accompanied it. In fact, I looked quickly through the manuscript when it first arrived, realized that I was going to need some time to write a thoughtful response, and then got totally engaged with pressing duties, the worst of which grew out of a series of crises involving the History of Science Society for which I am currently responsible. Even now I shall not really be able to write the sort of letter I should like to send you, but I dare not wait any longer.

In two respects, for neither of which you are responsible, your letter confronts me with a dilemma. I think you have understood my views quite well. Where you have failed to do so, I am entirely willing to take the responsibility, for I have discovered that there is much that I failed to make sufficiently clear to my readers. But, even if you had not understood me so well as you have, I would be far from certain that I had any right to try to change your views of my book. Nor am I sure that I ought to exercise such a right if I did have it. The fact that other people may derive views from the book which I think are not there and which I may even occasionally think incompatible with what I meant to express does not at all mean that the discoveries are wrong. Even in sciences like physics, fundamental contributions are sometimes aided by misunderstanding.

Even greater difficulties for me are presented by the fact that my own views have developed somewhat since the book was written. That is particularly the case with respect to the notion of paradigm which you use so heavily. It would be absurd for me to suggest that you ought now revise your book to conform to my own current views and usage. On the other hand, there are a number of places where you understand quite correctly what I said in 1962 but are expressing views that I no longer quite hold or could not, in any case, put in quite the same way. So, what should we do? I am interested in what you have written, and have no desire whatsoever to disassociate myself from it. On the other hand, both because my own views have been developing and also because you are making certain moves that I would not have made even in 1962, I would sonner not be explicitly associated with it either. Would you settle for: interest, encouragement, but something less than full agreement?

Now for a few more concrete remarks. Because I think they may interest you, I am sending you under separate cover a couple of reprints and one preprint. The latter is an extra chapter for a new edition of my book which will appear in the spring from the Chicago Press. It will indicate to you what I would do with the notion of paradigm if I were writing the book again at this time. Equally relevant, it will provide at least some hints about what happens to the distinction between the pre- and post-paradigm periods, a matter of particular importance when the attempt is made to apply my general viewpoint to the social sciences. Late in the chapter, there are also a few excessively brief remarks about the extent to which I suppose my ideas may be applicable to fields other than the developed sciences, and these, though very cryptic, may also be somewhat to the point. The reprint labelled "Comment" is my single attempt, very much underdeveloped, to say something about the differences between various sorts of communities, in this case scientific communities versus artistic communities, and it may by implication suggest some of the ways in which comparative community studies could be undertaken. Finally, the piece from the Encyclopedia includes a section in which I tried to say something about the sense in which developed scientific communities remained insulated from the larger society and to distinguish these from the equally important senses in which they do not. I probably should point out that the preprint should be in the public domain during the spring, so I see no harm in your quoting it in your book if you should wish to do so. Probably it would be a good idea to check the exact wording with the published version when it appears, for I have made a few alterations.

I suspect that the materials I am sending you will suggest the main areas in which we probably differ, though these differences are often matters of emphasis rather than substance. Let me close, therefore, with a few comments on some details in your manuscript which do bother me. On p. 5 you suggest that I allow "different and even incompatible paradigms" to exist side by side. I would insist, even as of 1962, that that occurs only when they are the paradigms of distinct communities. Perhaps what leads you to this misunderstanding is the example. Contemporary physicists say that light has some properties of waves and some of corpuscles, but they do not hold either the wave theory or the corpuscular theory of light. Instead they hold a single quantum-mechanical theory. That some philosophers think that theory internally contradictory is not relevant to the point presently at issue.

A passage that bothers me a good deal more, and which it would please me a great deal to have you modify, is the description of normal science in the central paragraph on p. 9. The words "plodding" and "pedestrian" are your additions. Furthermore, in the context in which you place it, the term "conforming" is also very likely to mislead. I think of normal science as an activity that can be extraordinarily challenging and exciting for the right sorts of people, calling forth the very best of scientific intelligence and even genius. Unless this is seen, it becomes hard to understand how I can insist that there is no really developed science that does not depend for its existence on normal science as well as on revolutions. The latter is absolutely essential to its development. One thing that seems to me decisively to distinguish, say, physics from a number of the social sciences is just that in the latter normal science, or its apparent equivalent, is pedestrian and has no significant role in the further development of the field.

Moving back to p. 7, I have some severe reservations about the implications of the first full paragraph on the page. I agree, of course, that in the sciences I know best, mainly physical sciences, the insulation of communities from the outside world is no longer at all what it was. But I am prepared to argue quite vehemently that the effect of the breakdown has been to make those communities more conservative than they were before rather than less. The contrast here with the social sciences is again quite marked. The radicalism and other special phenomena in the outside world are for the social scientist both subjects for study and sources of new ideas. The same movements present nothing of the same sort to people in fields like physics. On the contrary, they combine with special patterns of government funding to make scientists in the fields I know hew even more rigidly than usual to prevailing traditions.

My last remark is a triviality concerning the first footnote on p. 12. I believe that meteorological <u>theory</u> is just as applicable to the weather a million years ago as it is to the weather today. The rules of thumb which supplement the theory are of course somewhat different from what they would have been a million years ago, but that does not render the theory banal. Nor is that trivialization achieved by the admitted fact that one cannot utilize current meteorological theory to make very long term weather predicitions. Your remark does of course apply accurately to the case of a tribal community and an industrial megalopolis, but I think that is a remark about the current state of sociological theory not a point of principle.

I shall be very glad if any of this proves helpful to you and am perfectly willing to settle for the discovery that it has, at least, not been annoying. In either case, I wish you luck with your book and should be very glad to see a copy when it appears. Meanwhile, do you mind if I hold on to your manuscript. Your footnotes include the names of some books and articles I should like to have time to have a look at. If necessary, though, I can perfectly well copy those down and return the manuscript to you.

Sincerely,

Thomas S. Kuhn
Professor of the History of
Science

TSK/ec

From the study of . . .

Jessie Bernard, 4200 Cathedral Ave., N. W., Washington, D. C. 20016

December 10, 1969

Dear Dr. Kuhn,

I can well imagine that you receive more requests than you can manage; your book was a powerful potion. But I think my request is as much for your protection as for my own. You may wish to dissociate yourself from my use of your work. What I have done is build a whole book around the concepts of "community of scientists" and "scientific paradigms." You may not like what I have done with them. Particularly you may not like what I have done with paradigms. If so, this will give you a chance to disavow it. Where I am merely quoting you or paraphrasing you, there is probably no problem. But where I am extending what you say and applying it in a quite different context, you may disapprove. In Chapter 2, what I consider genuine paradigms, (how scientific they are you might challenge) are presented, which have undergirded all research on communities: Adam Smith's paradigm of pre-industrial capitalism and the structure-functionalist paradigm associated with the names of Merton and Parsons (they both used the term paradigm). After that, the ecological model of the Adam-Smith paradigm, the Warner class-structure paradigm, the community-power-structure paradigm, and the Gemeinschaft-Gesellschaft paradigm. But the whole thrust of the book is that the old paradigms are so loaded with anomalies which changes have brought on that they no longer serve as adequate guides for research or policy. It is a call for revolutionary approaches. OK? With warmest regards,

Appreciatively,

Jessie Bernard

Chapter 1. Paradigms in Crisis

Definitions
The Scientific Community
The Key Role of Paradigms
Are There Scientific Paradigms in the Social Sciences?
Community Paradigms
The Natural History of Paradigms
Normal Science and Scientific Revolutions
Extrinsic Influences on Paradigm Changes
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Chapter One. Paradigms in Crisis

Definitions

Definitions of community are not hard to come by. One review of the literature netted 94 such definitions.[1] A more extensive search could undoubtedly have netted even more. But there is probably no single definition whose "set of characteristics...is simultaneously applicable to all members of the class and to them alone."[2] It is not always easy, that is, to see what the diverse entities accepted as communities have in common. What, for example, do nomadic tribal communities of Bedoins and a megalopolis like New York City have in common?

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1. George A. Hiller, Jr., "Definitions of Community: Areas of Agreement," Rural Sociology, 20 (1955), pp. 194-204.
2. Thomas S. Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1962), p. 45.

Three characteristics are usually agreed upon as common to all communities, namely: locale, common ties, and social interaction.[3] Another student, on the basis of another survey of the literature, finds at least 8 dimensions and arrives at a definition which includes space, interdependence and integration manifested in consciousness of unity and ability to exercise control over the social, cultural, and biotic processes within this space.[4] Greater precision could doubtless be achieved by adding variables.

The problem of definition is complicated by the fact that the term community is used in two quite different though related ways, each essential to an understanding of the other. "The community" is not the same as "community." The first term usually refers to settlements of the kind encompassed in the definitions referred to above. The second, to a subtler set of phenomena. In this second sense, community is "the most fundamental and far-reaching of sociology's unit-ideas."[5] It is characterized by "a high degree of personal intimacy,emo-

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3. George A. Hiller, Jr., loc.cit.
4. Christen T. Jonassen, "Community Typology," in Marvin Sussman, ed., Community Structure and Analysis (New York: Crowell, 1959), pp. 20-21.
5. Robert Nisbet, The Sociological Tradition (New York: Basic Books, 1966), p. 47.

tional depth, moral commitment, social cohesion, and continuity in time...founded on man conceived in his wholeness rather than in one or another of the roles, taken separately, that he may hold in a social order."[1] The German term "Gemeinschaft" is sometimes used to refer to community in this sense.[2] It has become a veritable touchstone for societies in the second half of the century.[3]

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1. Ibid., p. 47.
2. The concept of Gemeinschaft is discussed in greater detail in Chapter 6 below.
3. The "quest for community" among modern young people is discussed in greater detail in Chapter 10 below. "Togetherness" had been almost a shibboleth in the 1950's.

The two conceptions have been related because in the past Gemeinschaft has depended on propinquity. It was not easy for social relationships to have intimacy and emotional depth or for social cohesion and continuity to persist in the absence of such propinquity. Locale was fundamental in the conception of both "community" in the Gemeinschaft sense and "the community" in the settlement sense. But improvements in transportation and communication have profoundly changed the significance of space for human relationships. Distance no longer precludes close and frequent contacts; nor does nearness guarantee them. Some students conclude therefore that "we may not be far from the time when the vernacular meaning of 'community' will be archaic and disappear from common usage.... If it is retained, it may be restricted to the provisions of children and of those adults who have not gained access to modern society."[4]

It is, indeed, true that for the "leading edge" of our society space has only a minimal effect on social contacts. A business man in New York City can telephone his counterpart in Chicago and propose that they have lunch together in Washington, or dinner in San Francisco. Professionals from all parts of the country meet one another in conferences, workshops, committees as casually and easily as they meet their local colleagues who live on the other side of town. Students of community belong to this highly mobile class of men, this "leading edge"

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4. Melvin M. Webber, "The Post-City Age," Daedalus, Fall, 1968, p. 1099.

of our society and it is therefore understandable that to them the very concept of local community seems obsolescent, rapidly becoming anachronistic, suitable only for women and children. Similarly communities of dispersed young people gather together with increasing frequency at festivals or demonstrations, much more at home at these meetings than they are in areas where they live.

Still, for the foreseeable future, at least until transportation facilities are considerably better able to handle large numbers, locale or territoriality will continue to constitute an important aspect of community. The boundaries may change, to be sure; there are many who already insist that cities must be viewed in a regional context. But space is nevertheless important. The fact that spatially dispersed people can come together and interact easily and frequently is important and it is undoubtedly the wave of the future for more and more people. But for the present community in the settlement sense remains important. It is accepted in the present volume.

Rather, then, than hold up the discussion by a long diversionary detour to arrive at a definition, we stipulate at the outset that the term community is, indeed, equivocal and almost impossible to define in a manner satisfactory to all. Some research has emphasized one aspect of the subject; some, another. Some, for example, the spatial aspect; some, the Gemeinschaft. An attempt is made to encompass both in the discussions here.

The Scientific Community

The community of scientists, or, perhaps better, the several communities of scientists, are among the most important of the "social communities comprised of spatially dispersed members."[1] Science is the work of such communities of scientists and, according to Thomas S. Kuhn, the "sociology of the scientific community,"[2] is the key for understanding science, including

1. Ibid., p. 1092.
2. Thomas S. Kuhn, op.cit., p. x.

the problems it attacks, the solutions it accepts, and the direction in which it moves. Thus scientific knowledge in any field depends, or has so far,[3] on the community of scientists who develop it. This community determines and maintains its boundaries; it decides who is in and who is not. It specifies what it will accept in the nature of problems, methods, interpretations, and what it will reject. It specifies the language any idea must come clothed in if it is to be acceptable. The accepted member must use only the prescribed vocabulary, style, and

3 See below for recent challenges to the autonomy of communities of scientists.

literary conventions. Communities of scientists define their discipline; they are the "disciplinarians."

In the area of community study, as in any other, this means, therefore, that what we know has depended on the interests of specific scientific communities. What they have wanted to know has determined what has been studied. We know about prehistoric and preliterate communities because for about a century and a half dedicated archaeologists and anthropologists have been studying them. We know a fair amount about ancient and medieval communities because there have been historians interested in them. We know a considerable amount about the industrial towns and cities of the 19th century primarily bedause they were of interest to reformers and city haters. We know a great deal about certain aspects of rural communities because, for reasons specified in Chapter 7 below, there has been a specialized community of scientists devoted to research in that area.

The Key Role of Paradigms

Scientific communities become organized around scientific paradigms. The term paradigm in this scientific-community theory of science is used in a special sense. A paradigm is more than an example, exemplar, model, mold, or pattern, as the dictionary defines it. Paradigms "are universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners."[1] Such achievements may include almost any aspect of the practice of science such, for example, as "law, theory, applications, and instrumentation together [which][2] provide models from which spring particular coherent traditions of scientific research." These achievements are acknowledged by some scientific community "as supplying the foundation for its further practice."[3] A paradigm is, then, a body of beliefs, ideas, theories, and data accepted by a community of scientists which then guides their selection of problems, methods of attack, choice of instruments, "hardware," "software," techniques, and forms of explanatory conceptualization.[4] It is, in brief, the "fundamental unit for the student of scientific development."[5] Different paradigms may compete for acceptance, some attracting one set of scientists, some another; but, once established, the community consolidates itself and ensures the safety of its paradigm by its control of admission to the community. Until recently their autonomy was unchallenged.

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1. Thomas S. Kuhn, op.cit., p.x. 2. Ibid., p.10. 3. Ibid., p. 11. 4. Ibid., p.11. 5. Ibid., p. 11.

Are There Scientific Paradigms in the Social Sciences?

Kuhn emphasizes the difficulties involved in arriving at paradigms and is not certain how may there are, if any, in the social sciences today. "It remains an open question," he notes, "what parts of social science have yet acquired such paradigms at all. History suggests that the road to a firm research consensus is extraordinarily arduous."[1]

1. Ibid., p. 15. On this point see also, G. J. Stigler, "The Nature and Role of Originality in Scientific Progress," in Essays in the History of Economics (Chicago: University of Chicago Press, 1965), pp. 1-15.

The answer to the question whether there are genuine scientific paradigms in the social sciences depends largely on the way one defines "scientific achievement." If the criteria are as rigorous as those in the physical sciences, the answer is that there probably are not; certainly not many. But if the criteria are relaxed, the answer is, yes there are.

For example, in the area of Kuhn's "instrumentation," the Simon-Binet achievement of tests of intelligence which inaugurated a vast research tradition of measurement of all kinds of social-psychological variables such as attitudes, opinions, judgments, institutionalization, alienation, to mention only a few, seems to qualify as scientific. The "law of diminishing returns" seems also to be worthy of such characterization. DeTocqueville's analysis of the effect of democratic institutions on a society would have a harder time to qualify as rigorously scientific, but it was based on careful observation and served many of the functions which Kuhn tells us scientific paradigms perform. Perhaps it would be better to call such cases quasi-scientific paradigms or scientific meta-paradigms (in the sense of lagging behind). But for the sake of simplicity, they will be referred to here as paradigms.

If, further, we specify that all members of a scientific community accept the achievement, the existence of paradigms in the social sciences can well be challenged. But Kuhn himself admits that different and even incompatible paradigms, such, for example, as the corpuscular and the wave theories of light, can exist side by side. And, in the life-history of a paradigm there may be some who accept the achievement on which it rests long before others do. All that is really necessary is that there be a community of scientists who do accept it and use it as a guide for research.

Whether or not such achievements in instrumentation, theory, or law are actually scientific in the strictest sense, many have served the function of paradigms for communities of scientists. They have attracted communities of scientists and supplied them with the foundations for a research tradition.[1] They are therefore accorded the status of scientific paradigms here.

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1. Not all scientific achievements attract communities of scientists. Among such "paradigms manqués" might be mentioned Floyd Allport's J-curve of conformity, whose acceptance might well have been useful in research on deviance; W.I.Thomas' "bohemian-philistine" typology which corresponded roughly to the later "hip-straight" typology. Since by their nature they do not survive, there are probably a number of such "paradigms manqués" buried in the literature of almost any science. The classic instance is the long-delayed acceptance of Gregor Mendel's achievements in genetics.

Community Paradigms

In the sociology of community, four classical paradigms encompass most of what we know. Their rise and decline constitute the major burden of the present volume. All have served as the focus of a community of scientists, sometimes known as "schools," and have given rise to "coherent traditions of scientific research" and provided "model problems and solutions to these communities of scientific practitioners.

One such community of scientific practitioners, sometimes referred to as "the Chicago school" was organized around the so-called ecological paradigm which explained how populations have distributed themselves, and how the resulting settlements have become spatially structured, and how they varied sociologically in structural components (Chapter 3). Another community of scientists, sometimes referred to as "the Warner school," was organized around the ranked-status or class-as-status paradigm based on research originally done on Yankee City (Chapter 4). A third scientific community was organized around paradigms of community power or class-as-power, shared by both sociologists and political scientists (Chapter 5). A fourth classic community paradigm goes under the rubric Gemeinschaft-Gesellschaft, and deals, as hinted above, not with the territorial aspects of community, except secondarily, but rather with the meaning of such spatial aspects of settlements for human relationships, with the social-psychological aspect of "community" rather than with "the" community. This paradigm has been bifurcated, so that

two communities of scientists have shared it, one specializing in the study of rural communities and one in the study of urban communities (Part III).

Communities of scientists are no longer insulated as they once were, from the outside world. But for many years they had complete control over the course of science. And the tendency was in the direction of scientific conservatism. Kuhn analyzes some of the intrinsic processes within the community of scientists which tended toward inertia.

The Natural History of Paradigms

Inertia and conservatism tended to be built into scientific paradigms by the very nature of the communities organized around them. Men learn paradigms in school from teachers who also learned them in school before them, so there is an unbroken tradition received and passed on. "The study of paradigms...is what mainly prepares the student for membership in the particular scientific community with which he will later practice. Because he there joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom evoke overt disagreement over fundamentals. Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, that is, for the genesis and continuation of a particular research tradition."[1] The neophyte, in our day a graduate student, is usually socialized into his particular paradigmatic community by means of his dissertation. He becomes a member when it is published and accepted and thereafter he is known as identified with it. To become and remain a member he implicitly agrees to use only the accepted language, ask only acceptable questions, and use only acceptable methods for answering them. He subjects himself to all the subtle controls of his community.

1.Thomas S. Kuhn, op.cit., pp. 10-11.

The scientists who share a paradigm become an in-group, "clubby" in an intellectual way. They refer to one another's work in their own, understand one another's references, know who is recognized and who is not. The scientific community can be very cosy to those on the inside and very punitive to those on the outside. Its conventions constitute a kind of boundary-maintenance device. Members are loathe to violate its canons and thus invite sanctions. For it can ex-communicate members, read them out of the club. If, for example, some practitioners

do not accept a paradigm, "they are simply read out of the profession, which thereafter ignores their work."[1] Or they can be, figuratively speaking, "denounced."[2] The community can thus de-

1. Ibid., p. 19.

2. In 1968, the Radical Caucus of the American Sociological Association, for example, called for censure of all members who had participated in a research project they disapproved of. See American Sociologist, 3 (Nov.,1968), pp. 322-323. In 1969 at the Massachusetts Institute of Technology, scientists in one community were calling for a veto on research on counterinsurgency by colleagues in another community of scientists. See Noam Chomsky, Washington Post, Nov. 26, 1969.

termine the fate of paradigms. It is because communities of scientists are so powerful in their control of members and in the conservation of paradigms that Kuhn assigns so much importance to them for an understanding of science.

None of this implies that communities of scientists are notionate or irresponsible in their acceptance and transmission of paradigms. They are not./ It is just that some limits must be imposed. "Observation and experience can and must drastically restrict the range of admissible scientific belief, else there could be no science."[3] But observation and experience do not "alone determine a particular body of scientific belief."[4] For "an apparently arbitrary element, compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time."[5]

3. Thomas S. Kuhn, opx.cit., p. 4.

4. Ibid., p. 4.

5. Ibid., p.4.

In the case of the sociology of community, this "arbitrary element compounded of personal and historical accident may well be illustrated by two quite disparate research traditions which developed in the United States, one concentrating on rural communities and one on urban. For reasons to be presented in Chapter 7, enormous resources of personnel were invested in research on the rural community; rural sociology became a major field of the profession. Not until well into the present century did the sociology of urban communities begin to attract commensurate research attention, and for many years it still did not receive the same amount of government support. It came to seem "natural" that there should be two separate scientific communities studying communities, one with a rural and one with an urban orientation.

Another arbitrary element which influences the acceptance or rejection of paradigms of any given scientific community is the prestige of the university or scientist presenting it. Kuhn tells of an occasion when a paper by Lord Rayleigh which inadvertently did not bear his name was rejected by the British Association as the work of a "paradoxer." When the name of the author was supplied, it was accepted with profuse apologies.[1] Still another arbitrary factor is the sex of the person who offers a paradigm, those presented by women being less likely than those presented by men to receive a hearing.[2]

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1. Ibid., p. 152.
2. Jessie Bernard, Academic Women (Pennsylvania State University Press, 1964), Chapter 10.

Normal Science and Scientific Revolutions

Some members of a scientific community are content merely to receive and transmit its paradigms. Some add a little here, subtract a little there, and some are rewarded by having their little contributions incorporated into the received tradition. Kuhn labels what they do as "normal science." It is "the activity in which most scientists inevitably spend almost all their time."[3] It is the plodding, conforming, replicative, additive, cumulative, pedestrian kind. The communities of all the classic community paradigms have engaged in it. Literally scores of competent, well-designed, and well-executed research projects have resulted.

But this placid picture is deceptive. The course of science is not serene. Anomalies begin to show in research findings. Exceptions become numerous. Results that do not fit the accepted paradigm cloud the picture. Or positively conflicting results appear.

Many members of the scientific community, especially the older ones, are willing to make the adjustments necessary to maintain the old paradigm. But others, usually the younger, are less acquiescent.[4] Finally, when "the profession can no longer evade anomalies that subvert the existing tradition of scientific practice...begin the extraordinary investigations that lead the profession at least to a new set of commitments, a new basis for the practice of science."[5] These shifts of professional commitments constitute scientific revolutions. They are "tradition-shattering complements to the tradition-bound activity of normal science."[6]

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3. ~~Khúxxxxxxx~~ Thomas S. Kuhn, op.cit., p. 5.
4. For a personal account of the induction of a neophyte into his scientific community and his defection from it, see "A Sociology Student's" letter to the editors of The Journal of Social

Issues, 25 (Summer, 1969), pp. 158-162. He objected to a conception of desire for the esteem of others as a cultural universal. In his opinion, such a conception "missed an important new development...happening amongst the youth of this country: namely, they want out of the performance bag, to escape the necessity of basing their whole sense of self-esteem on the evaluations of others" (personal letter, Nov. 20, 1969). See Chapter 4/below for further elaboration of this point.

5. Thomas S.Kuhn, op.cit., p. 6. 6. Ibid., p.6.

Such scientific revolutions are not easy. There may be resistance, for quite human and understandable reasons. Older members especially may find it difficult to change their way of thinking and jettison the old and anyway the old seems just as good for practical purposes as the new one. At least it works; they are used to it. So they cleave to it. What difference does it make, really, whether the sun goes around the earth or the earth around the sun?

Younger members, however, less identified with the old are more hospitable to innovation. For a while there may be controversy. In time, the older more resistant members die off or are read out of the profession or are isolated, their articles are not accepted by the journals or their books by publishers, and the new paradigm takes over. It takes over, it should be noted, not necessarily because it is superior, though it usually is, nor because it has been "proved"[1] or validated against "nature" but because it has succeeded in the competition for adherents.[2]

Because new paradigms require reconstruction of accepted commitments and even re-evaluation of prior facts, they take time. Scientific revolutions are not sudden. The old hangs on at least until the surviving generation takes it with them from the stage.[3] In the meanwhile the processes of normal science -- improvement, modification, adjustment of the new paradigm -- take over until once more it, too, proves less effective than another one and the process repeats itself. An example of a scientific revolution in the field of the social sciences is offered in Chapter 2, where the story of the paradigm of pre-industrial capitalism is sketched.

1. Ibid., pp. 147, 151. 2. Ibid., Chapter 12.

3. Ibid., p. 19. "There is no learning among doctors," the dean of a medical school is quoted as saying, "only biological replacement by those with new ideas" (John R. Seeley, "Remaking the Urban Scene: New Youth in an Old Environment," Daedalus, Fall, 1968, p.1125.

Normal science, then, accepts the received paradigm and tries only to improve it; it does not seek to be innovative, creative, original. It is mundane and modest. Revolutionary science, on the other hand, is not merely an accretion of knowledge. It seeks better ways to look at problems and better ways to explain or interpret findings; it does not fundamentally alter old paradigms. It seeks to create new ones.

Extrinsic Influences on Paradigm Changes

Kuhn's major emphasis is on the sociology of the scientific community itself in determining the process of paradigm change but he does not rule out the influence of extrinsic factors, especially technological advance and external social, economic, and intellectual conditions.[1] Conservative influences have been strong in the case of community paradigms; they have been characterized by normal-science research rather than by scientific revolutions. But extrinsic forces are now powerfully at work driving them close to crisis.

With respect to technological innovations, paradigms in the social sciences respond in ways not very different from the ways in which paradigms in the physical and biological sciences respond. Computers, for example, have an impact in the social sciences as great as in the physical and biological sciences. With respect to external conditions, however, there is a difference.

It is true that despite the protection of ivory towers all kinds of scientific paradigms are subject to outside influence. The treatment of Galilleo at the hands of the Church was only one of many examples. Darwin's theory of evolution had to fight the same kind of battle. Dubois withdrew his Pithecanthropus Erectus bones to protect himself against calumnious attack. The Freudian paradigm of infant sexuality had hard sledding not only because of the rejection by scientists but also because of popular outcry against it. There are still great lacunae in our knowledge about sex and sexuality because until recently they were taboo subjects for scientific inquiry.[2] Paradigms in the social sciences face the same kinds of hazards.

In addition they have peculiar problems of their own. In the physical and biological sciences the new data or "novelties of fact,"[3] which lead to paradigm shifts tend to result

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1. Thomas S. Kuhn, op.cit., p. xii.
2. Jessie Bernard, "Technology, Science and Sex Attitudes," Impact of Science on Society, Vol. 18 (Oct.-Dec.,1968), pp. 213-228.
3. Thomas S. Kuhn, op.cit., p. 52.

from the research of normal science. In the social sciences, the new data which force modification or supplanting of old paradigms come not only from the researches of normal science but also from historical experience. That is, current "history" as well as controlled or improved observation and researches provide the anomalies that have to be incorporated into paradigms. A modern industrialized community generates such "novelties of fact" even faster than a scientist does in his laboratory. It is, to use an analogy, as though millions of years of change in the evolution of the universe were telescoped into a day, or hundreds of thousands of years in the evolution of life were telescoped into an hour. Perhaps the nearest analogue among the physical sciences would be metereology. The paradigms which interpret weather today refer to a world of land masses with certain sizes and shapes and distributions, different from those of milennia ago, as sociological paradigms of modern industrial communities today refer to a world quite different from that of preliterate communities of only 20,000 years ago. If the land masses had been then as they are today, today's paradigms would fit; the same may be said of "population masses." Communities have changed more rapidly than land masses have; community paradigms are therefore more perishable than metereological ones.[1]

1. It is hardly an answer to say that ideally a paradigm would explain equally well the weather a million years ago and today, a tribal community and an industrial megalopolis. But any paradigm this general would be banal, little more than "common sense." See Chapter 16 for further comment on this point.

For the most part, the result of novelties of fact in the form of current history have produced tinkering rather than revolutions in community paradigms. As a result, current community paradigms do not tell us how cities function; we are at a loss to interpret the nature and operation of power in the community; old status criteria no longer seem to fit the current scene. Events have overtaken old paradigms. And research-as-usual will not do what has to be done, for "paradigms are not corrigible by normal science at all."[2] The upheavals of the

2. Thomas S. Kuhn, op.cit., p. 121.

last third of the century in the United States as well as in the Third World have produced novelties of fact which are so anomalous as related to old paradigms that merely tinkering with old paradigms does not seem to be enough. Paradigm shifts seem to be called for. We need paradigms which "tell us different things about the population of the universe [of communities] and about that population's behavior." 3/

3. Ibid., p. 102.

The lag between new data and paradigm change, whet er by normal-science tinkering or by revolutionary shift, tends to be greater in the social than in the physical and biological sciences. We have to resign ourselves to the fact that in the social sciences we never know the world as it is. It takes an enormous number of specialized personnel to tell us what it was like this morning, yesterday, last year, a generation ago, a thousand years ago. It takes the efforts of leg-men, newspaper columnists, political commentators and analysts, mass media, census enumerators, as well as scientific researchers, to keep us au courant. In time we get a fairly good idea of what it was like. The commercial revolution had begun in earnest in the 12th and 13th centuries, had already vastly extended the market even for international trade, the feudal system had already disintegrated, the Church had already lost its monopolistic hegemony over men's minds when intellectuals began to tell about these things. Adam Smith could see what led to the wealth of nations and the Frendh philosophes could discern the release of men from serfdom. They were talking about what had already happened. This process of catching up with events took considerably longer before modern mass media were available; but even today paradigms tend always to be behind the times. They show us how to see what was rather than what is. No matter how much tinkering we do on them, anomalies persist and grow. The suspicion grows that the classic paradigms are not guiding our researches in the most useful way.

The Call for New Community Paradigms

A call for new community paradigms is therefore coming from a variety of sources. Alfred Dean, a sociologist, warns us that "a first-order problem is a reexamination of the concept of the community.... The complexity of modern communities defies traditional conceptualizations and similarly taxes empirical techniques and strategies. The key problem is that of attempting to link individuals or groups to an ill-defined, inadequately described, and insufficiently understood, social grouping. The resolution of this [paradigmatic] problem is essential to the understanding of individual and group adaptation, and will demand innovation and increased sophistication."[1] Another sociologist, Roland L.Warren, makes the same urgent plea: "the crying need in community theory, for practical, as well as theoretical reasons, is for a relatively simple model of the community which can permit meaningful analyses and testable research hypotheses.... Changes are taking place which render older conceptions of community living inadequate.... Students of the community have come to doubt the adequacy of earlier community theory even to describe the situation which existed several decades ago.... When all such difficulties are considered, a few sociologists throw up their hands and urge that the whole concept of the community be discarded as a sort of useless theoretical will-o'-the-wisp."[2]

Members of other communities of scientists interested in community study join in the call. Melvin M. Webber, a planning expert, tells us that our thinking is confused because of "deficiencies of our language and the anachronistic thought-ways we have carried over from the passing era...; old labels...are no longer fitting."[3] And he concludes that social-science theory is inadequate for understanding or interpreting the new communities of spatially dispersed members made possible by the dissolution of spatial barriers. Daniel Patrick Moynihan, an urbanologist, diagnoses the failures of the Great Society's war on poverty as due to lack of adequate guidance from the social sciences, from faulty theorizing, that is, faulty paradigms.[4] Kenneth E. Boulding, an economist, calls for new knowledge and ideas about cities.[5] A city planner tells us that we are stymied in dealing adequately with our problems until we have learned how to

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1. Alfred Dean, in review of Community Dynamics and Mental Health by Donald C.Klein in Social Forces (Sept.,1969), p. 142.

2. Roland L. Warren, The Community in America (Chicago: Rand McNally, 1963), pp.ix,2,6,9. For some of his suggestions for a new model see Chapter 5 below.

3. Melvin M. Webber, loc.cit., p. 1093.

4. Daniel Patrick Moynihan, Community Action in the War on Poverty (Free Press, 1968).
5. Kenneth E. Boulding, "The City and the International System," Daedalus, Fall,1968,p.1122.

view communities today: "the failure of cities is an intellectual one. It is brought about by the failure of the intellectuals [read scientists] to generate a viable concept of a modern city and modern region." (1) Kenneth Keniston, a psychologist, is one among many calling for paradigms which emphasize quality of life rather than, as current ones do, maximizing, minimizing, optimizing quantifiable values; he concludes that "one of the critical needs today is for a new vocabulary and rhetoric, adequate to the special revolutionary needs of the post-industrial world most of us live in." (2)

In addition to these expressions of dissatisfaction with current modes of viewing communities by responsible members-in-good standing of established scientific communities, there are others emanating from young radicals. One representative of their point of view states that it is the provinciality of the communities of scientists that renders them incapable of supplying adequate ways of viewing our communities. Such men can see the mote in the eyes of others but not the beams in their own. We use a paradigm of "social pathology," he says, by way of example, to interpret the ills of our cities but invoke government and economic policies to interpret similar ills in Russia. (3)

~~1. Alfred Dean, review of Community Dynamics and Mental Health by Donald C. Klein in Social Forces (Sept., 1969), p. 142.~~

~~2. Kenneth E. Boulding, "The City and the International System," Daedalus, Fall,1968,p.1122.~~

(3) Edmund N. Bacon, "Urban Process," Daedalus, Fall, 1968, p.1165. This author rebukes the community of scientists for interfering with the search for new paradigms. "The root issue -- basic concepts and ways of thinking -- is not even discussed." Such discussion is discouraged. "The happy assumption is made that we already know how to think.... Current teaching in the universities has much to do with this." For it insists on thinking in terms of manageable numbers and kills in the bud any creative thinking by labelling it "intuitive"(Ibid). This is an accusation example of the processes of disciplinary control described above.

(2) Kenneth Keniston, "Student Unrest and the Two Revolutions," Washington University Alumni News, March, 1969, p. 7.

3. Nicholas Von Hoffman, "How to Look at Ourselves," Washington Post, April 7, 1969. "Our systemic analysis of Russian society makes us see why their cars don't run right and a proportion of their people are hungry. Our lack of systemic analysis of American society has blinded us to hunger here and made us oblivious to the defects in our own social and economic processes. One of the reasons we haven't looked at our society as a number of interlocking systems is that few tools to do it have been available. You can't analyze a social system if you accept all of its deep-down, unspoken assumptions. You have to get outside of it unless you're content to be a tepid reformer, a person who patches, fixes, and fusses over details.... Both Americans Left and Right are laboring to build suitable systems of analysis, but it's hard to do, coming out of 30 years of high-pressure national consensus." A similar point of view has been expressed also by John R. Seeley: "A way would have to be found to 'get out of the culture' in order to find as a new person a new and different direction.... The first problem is to find the meaning of getting out and the means to do so" ("Remaking the Urban Scene," Daedalus, Fall, 1968, p. 1133).

In the absence of suitable paradigms, much of the research on community might be compared to the behavior of the Roman generals who plotted "reparative wars abroad and placatory redistribution of bread and amusement at home, just as a double, irresistible assault striking at the roots of the very idea of Rome was being mounted by 'barbarians' abroad and 'riff-raff' in catacombs in the heart of the heartland at home."[1] The "irresistible assault" was an at-

1. John R. Seeley, loc.cit., p. 1126.

tack on some of the most time-honored paradigms by the "barbarians" and "riff-raff" who were mounting it were the so-called Radical Caucuses in the several communities of social scientists.

Rebellion in Communities of Scientists

In addition to the call for revolutionary paradigms in the traditional setting of the natural history of paradigms, there was beginning in the last third of the 20th century a rebellion against the power of communities of scientists to control the direction of science. There was a feeling that the direction of science ought not to be left to the communities of scientists themselves. Science, to paraphrase the old cliché about war, was too important to leave to scientists. The rebels attacked not what Kuhn had called the "unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life,"[2] but rather their too-great involvement in such demands of the laity, especially the segments of the laity

2. Thomas S. Kuhn, op.cit., p. 163.

which could pay for research. Although sociologists had been agitating the question, "knowledge for what?"[1] for many years, the movement began in earnest among atomic scientists only after WW II. By the late 1960's social scientists were deeply involved. The whole issue of academic freedome was raised. In the professional associations of all the social sciences Radical Caucuses were asking for greater concern for the social and political implications of research. In the social sciences they claimed that communities of scientists had served exploitative interests[2] that could afford research rather than exploited groups that could not.

1. Robert S. Lynd, Knowledge for What? (Princeton: Princeton University Press, 1939).

2. Management, for example, rather than labor; white people rather than black people; the "Establishment" or status quo rather than reformers or rebels; military rather than non-military problems. As an example of the use of social-science research in behalf of management was a Unesco volume edited by Rensis Likert and Samuel P. Hayes, Jr., Some Applications of Behavioral Research (1957) which presented research on administrative leadership, leadership for effective human relations, marketing and public relations, and the like. See also the discussion of racism, Chapter 11 below.

In the words of the Radical Caucus in the American Sociological Association:

> Since the rise of large-scale social research, social scientists have accepted money to study and solve the "problems" of the powerful organizations in our society, be they industries faced with dissatisfied workers or governments faced with dissatisfied citizens. Masquerading as disinterested university scholars, they have provided their sponsors with the knowledge needed for controlling modern society. As sociologists we demand that sociologists not engage in research designed to perpetuate existing power structures by oppressing the dissatisfied, through crowd pacification, counterinsurgency research, etc.[3]

If the control of the direction of research should, indeed, be taken out of the hands of the communities of scientists, what we know about communities may, indeed, change markedly. How or in what direction we have as yet no way of knowing. For the present the sociology of community rests on the work of traditional communities of scientists.

3. American Sociologist, 3 (Nov.,1968), p. 322. To achieve their goal, the members of the Radical Caucus demanded that (1) sociologists not engage in research that cannot be made available to all; (2) sociologists communicate their findings to those affected by them in ways that make them available to all; (3) all "strings" attached by funding organizations be publicized in reports of findings; (4) researchers funded by non-university sources label themselves as

employees of the funding organization, not the university; (5) "sociologists should devote an equitable portion of their energies to the solution of 'problems' as defined by those who cannot pay for research;" and (6) sociologists who had participated in a project they disapproved of be censured (pp.322-323). All but the 4th and 6th were accepted by the Council of the American Sociological Association.

Paradigms, Policy, and Prediction

To speak of the function of sociological paradigms as guides to policy does not imply that it is also their function to predict.[1] On this tired old subject only this will be said here, that

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1. "...the social sciences...have not developed adequate predictive theory in most fields of national concern. It is sobering that no sociologist predicted the magnitude of the Negro Revolt, that no prewar urbanist anticipated the postwar development patterns in American cities, and that, most troubling of all, no one has yet written systematic alternative futures seeking to chart the possible course of events in these fields"(Melvin M. Webber, loc.cit., p. 1091).

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rather than a paradigm's predicting the future, the reverse seems to be the case. The "future" in the form of new data from the research laboratory in the case of some paradigms, from "history" in the case of others, "predicts" the past. Thus, the more we know about the consequences of, let us say, the first industrial revolution, the better our paradigms are in interpreting it. The more we know about the 19th century, the better our paradigms of even pre-industrial capitalism. We know more about the Civil War or the French Revolution than people knew at the time it took place because we know the ramifications over time; we know more about the Roman Empire than people knew about it when it all but covered the known world, because we know what happened afterwards. The more we learn about the 1960's, the more we know about the 1950's. And the more we learn about the 1970's, the more we will know about the 1960's. And so on.

This reading of history backwards presupposes that at any one historical moment there are several options open to a community. The fact that one rather than another is followed tells us a great deal about the community, then and now. There comes a time when this "game-in-normal-form" -- that is, a tree-shaped diagrammatic schema illustrating strategic choices available to the community at every decision point -- has become so long and so multi-branched that it no longer pays to go back to a distant starting point. Perhaps only three or four "generations" of decisions are relevant enough to bother with. The implication is not that

one particular present was predetermined in the past -- perhaps scores of probable futures, including our present were implicit in it -- but that we can understand it better when we know the consequences of any particular "strategy" or choice.[1] We will understand the communities which have provided the research data for our present paradigms better when we have shifted our paradigms to take account of the anomalies or novelties of fact which the 1960's and 1970's provide.

1. For an interesting discussion of the significance of the Civil War as interpreted in terms of its consequences at different points of time, see John S. Rosenberg, "Toward a New Civil War Revisionism," American Scholar, 38 (Spring, 1969), pp. 250-272.

Plan of the Book

The general "scenario" of the present book may be sketched as follows. Part II presents three classic community paradigms dealing with community in the settlement sense: the ecological, the ranked-status or class-as-status, and the community power structure paradigms. Part III deals with the fundamental paradigm (or set of paradigms) which goes under the rubric Gemeinschaft and Gesellschaft referred to above, which has guided thinking and research in rural and urban communities. Part IV deals with the paradigmatic crises precipitated by the accumulation of anomalies and the need for "modernizing" communities today; and Part V deals with the communities of the Third World, especially with the failure of paradigms developed in connection with communities shaped in a Western context to serve adequately for either research or policy in a different context.

Before proceeding to a detailed discussion of these topics, however, there is needed at least a skeleton presentation of two over-arching paradigms which have guided all community research, one the paradigm of capitalism and one the structure-functional paradigm. Although they are not specifically community paradigm, they have constituted the encompassing matrix within which most community research has taken place.

December 8, 1969

Miss Jessie Bernard
4200 Cathedral Avenue, N. W.
Washington, D. C. 20016

Dear Miss Bernard:

My thanks for your note of 29 November. It is a pleasure to hear of your work, and, if a "few pages" is all I need read to be helpful to you, I should be glad to do so. I do not mean to be stand-offish, but requests like yours are now reaching me more often than I can easily manage. It would give me great pleasure to look at a bit of your manuscript, and I urge you to send it along. I shall not, however, be able to do more than that.

Sincerely,

Thomas S. Kuhn
Professor of the
History of Science

TSK/ec

From the study of . . .

Jessie Bernard, 4200 Cathedral Ave., N. W., Washington, D. C. 20016

November 29, 1969

Dear Dr. Kuhn,

I have just completed a manuscript on the sociology of community which is organized around your conception of the community of scientists and the function of paradigms. I wonder if you would be willing to glance over the few pages in Chapter 1 that refer to your work, primarily as a protection against any mis-use of your ideas, but also as a way to inform you in advance. I would greatly appreciate any courtesy you may be willing to extend in this connection.

Sincerelyk,

Jessie Bernard

19 June 1968

Miss Zane Berzins
182 Tremont Street
Somerville, Massachusetts 02143

Dear Miss Berzins:

My thanks for your letter of 9 June. Assuming that you have worked out the necessary formalities with the Chicago Press, which controls "rights," I have no objection at all to your quoting from the pages listed in your letter. Good luck with the anthology.

Sincerely,

Thomas S. Kuhn
Director

TSK/jt

June 9, 1968

Professor Thomas S. Kuhn
History of Science
Princeton University
Princeton, N.J.

Dear Sir:

In collaboration with Professor Norman F. Cantor of Brandeis University, I am editing an anthology of readings to be called The Generation of Relativism and Irrationalism. This will be a paperback volume, approximately 200 pages in length, intended for the college market, to be published by Thomas Y. Crowell Co., New York. Professor Cantor and I would like your permission to reprint the following excerpts from one of your books:

Thomas S. Kuhn, The Structure of Scientific Revolutions, (Chicago: The University of Chicago Press, 1962) pp. 52-53, 64-65, 67-68, 73-74, 84-85, 91-93.

The selections amount to approximately 1,500 words.

The University of Chicago Press, with whom we have a contract in circulation for the above material, requires that the author's personal permission be obtained for all material used. We would very much appreciate hearing from you as soon as possible.

Sincerely,

Zane Berzins

Zane Berzins

Please Reply to:
Miss Zane Berzins
182 Tremont Street
Somerville, Mass. 02143

April 3, 1979

James G. Blight, Ph.D
Psychology and History of Science
Grand Valley State Colleges
Allendale, Michigan 49401

Dear Dr. Blight,

My thanks for your letter of 5 March and its enclosure, the missing page from your letter of 5 February. I have already said what I could about Part II of that letter and shall now see what I can do with the three questions of Part I. For reasons that you can imagine, I shall have to be brief, and I apologize in advance for the obscurity that it is likely to result.

I shall take the questions in reverse order, beginning with Mr. Trap's. I think it is possible to break out of the domination of a paradigm and that brilliance, effort, and often also luck are required to bring it about. But I think the break is always from one paradigm into another one, never into some sort of paradigmless view. I see no real difficulties about combining that belief with an equally firm conviction that a paradigm is often affected by experience. I use the term "experience" rather than Mr. Trap's word "facts" because, in my view, the facts of experience are themselves conditioned by the reigning paradigm. But that does not mean that a paradigm can force experience into any mold at all or that it can forever suppress anomalies. The situation my book describes as crisis is the result of an acknowledged difference between the paradigm and experience. Furthermore, a successful new paradigm, if one evolves, must resolve the apparent misfit.

Calling attention to what I have said about crisis may also be a way to answer the questions posed by Ms. Lang and Mr. Bernstein. I think it is a mistake to say that my view "stresses nonfalsifiability or verification of scientific hypotheses," in contrast to Popper's emphasis on refutation. Crisis caused by the failure of theory to fit fact (from here on I shall use that problematic word) is fundamental to my model of scientific activity, no less so to Structure than to the article in Thinking. I think of myself, in this respect, as making the same use of the logical asymmetry between verification and refutation that Popper makes, and I may well have gotten the notion from him. But I do not believe (actually, Popper does not either, at least when pressed) that "a single refutation" does any good at all or even that there is any such thing. The line between a confirming and falsifying instance is by no means well defined. It is only after difficulties pile up that some scientists come to believe that they must look for another theory. And even then it is legitimate for others to disagree.

q I come finally to the issues raised by you and Mr. De Bruyn, and my overall reaction is to say that I do not altogether understand why you say certain of the things you do. Hopefully, my answers to the two preceding questinos may have prepared you for the difficulties your question presents. That is why I spoke to them first. In the first place, I do not for a moment suppose that the survival of theories "is unrelated to their worth." If one takes a relatively operational or instrumentalist measure of worth (roughly, theories are instruments for solving an increasing number of puzzles with increasing accuracy), then the worthier theory regularly wins out. You seem, however, (or perhaps you are just explaining Popper), to want to reserve the term "worth" for more metaphysical applications. I do not see how that is to be done. The term "error" seems to me to be in as much difficulty as the term "truth." Both terms function unproblematically when working within a specified system or paradigm. Neither of them seems to me free from difficulties when applied to the transition from one system to another. (I have written about this question at greater length in my lead article in the Lakatos and Musgrave volume, Criticism and the Growth of Knowledge.) You need not at all agree with that position, but I cannot see why it should lead you to say that it's effect is "to reduce the mind to a mere epiphenomenal illusion." The sociological environment is only one of the relevant ones within which scientists operate, and these environments only condition but do not control what individual scientists can do in their given environment. To say that creative intelligence is constrained by the environment is not to say that it does not exist or that it is not absolutely essential to scientific change.

I would be glad if these remarks were of use. If not, further clarification may be provided by my recent collection of essays, now to my pleasure available at a more reasonable price in paperback.

Sincerely,

Thomas S. Kuhn

TK:lk

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March 5, 1979

Prof. Thomas S. Kuhn
The New York Institute for the Humanities
19 University Place
New York, NY 10003

Dear Prof. Kuhn:

Thank you very much for your thoughtful, prompt and gracious letter of February 22. I am very sorry to have left out the entire middle section of the letter, especially since that erstwhile section contains most of my own special inquiry. I often jest with my students about absent-minded professors, premature senile psychoses and other academic hazards. Good heavens, can these actually be happening to me? In order to try to refute these scary hypotheses, and also because all of us in the Popper-Kuhn-Psychology seminar are very interested in your views, I have this time (I'm pretty sure, though you might check!) enclosed a copy of the complete original letter. We once again eagerly await your reply.

I will resist the temptation to respond in detail to your remarks of February 22, but I can hardly avoid a comment or two. I agree with your comparative historical analyses; there are certain remarkable parallel developments between the histories of the mature sciences and contemporary events in, say, psychology. We in psychology have nothing resembling a paradigm. This rather obvious fact led my own mentor, Robert I. Watson, to characterize psychology as a "prescriptive science," a collection of continua, a multidimensional analysis of which might allow one to describe most contemporary positions. Yet in Watson's analyses, and in my own related (though hardly identical view), there is very little suggestion that the situation in psychology has changed at all since the time of Plato. There has simply been no progress, either in the very strong sense of the positivists, or in Popper's sense, or, I think, in your own rather neutral (i.e., merely temporal, rather than evaluative) use of the term. We are and always have been asking what Popper has called "essentialist" or "what is" questions. One thing seems clear to me: the only sort of "psychology" that has even a slim chance of carrying the day is a radical behaviorism like Skinner's. Once we move away from his behavioral domain and we enter the world within the skin and, even, within the mind, we enter a whirling chaos of untestable, highly personal pluralities. These worlds seem logically as well as empirically immune from

College of Arts and Sciences, Thomas Jefferson College, William James College, College IV
F.E. Seidman Graduate College of Business and Administration, College of Graduate Studies

scientific progress. If, however, we try to circumvent the mind and its unique problems, we of course transform psychology into a physical science by edict alone. It becomes the technology of animal training.

Thus I am brought round to my original inquiry (with W.P. DeBruyn), pp. 2-3 of the original letter. If psychology is viewed as merely an infant in the broader scientific community, if it is held that its evolution will take the form you ascribe to Physics, Chemistry, Astronomy and Biology, then what sort of a person is it that must be described by this evolving discipline? Are there qualitative differences in the disciplines and their evolution which may reflect qualitative differences in their subject matter?

Well, enough for now. Once again, for all the seminarians, I express my admiration for your work, and my gratitude for your interest in our inquiries.

Sincerely,

Jim Blight

James G. Blight, Ph.D.
Psychology and History of Science

GRAND VALLEY STATE COLLEGES

Allendale, Michigan 49401 • 616/895-6611 • An Equal Opportunity/Affirmative Action Institution

February 5, 1979

Prof. Thomas S. Kuhn
Department of the History of Science
Princeton University
Princeton, N.J. 08540

Dear Prof. Kuhn:

I am keenly aware that you must be very, very busy, positively inundated with requests to examine this or that application of your provocative views to yet another aspect of the natural or social sciences. I am told by Bill Woodward, a former pupil of yours (and of Julian Jaynes), that it is indeed tiresome being an academic superstar; you have been forced, according to Bill, simply to ignore much of your mail.

Let me say straight off: this is not a request for your comments on the latest Kuhnian extravaganza; it is not an excuse to get your name in a footnote claiming a "personal communication" from T.S. Kuhn; nor is it a vindictive attack from positivistic or Popperian hooligans screaming loudly that a "pair-a-dimes" is all your wretched "paradigms" are worth. No, it's none of these. This is a letter containing a few, basic, probing questions which genuinely perplex a group of seminarians who have given many months of their time to the simultaneous study of Popper, Kuhn and psychology. They represent the distillation of many weeks, many discussions and many frustrations. We hope sincerely that you might find the time to address yourself to our perplexities. Our questions are, I think, just the sort of inquiries one might expect from earnest psychologists, of a philosophical turn of mind, who have begun to inquire into the foundations of their discipline. You will notice that many of our questions are framed in Popperian language; we have, as a matter of fact, been reading Popper (especially his essays on method and on evolution), thus we have inevitably ingested some of his terms and biases.

I have arranged the questions in outline form and, for convenience, added (more or less) appropriate titles.

I. The Role of Mind and Logic in Science

a) The Evolution of Mind and the Evolution of Science: A Paradox.

Loren Eiseley has suggested that the nineteenth century was "Darwin's Century", and it is certainly true that in the years following the publication of the Origin of Species students of the life sciences exchanged their belief in an unseen order and stability for an overriding concern with function, adaptation and development. In the history and philosophy of science, however, we appear to have just entered a Darwinian phase; perhaps in these fields the next fifty years or so may someday be referred to as "Darwin's half-century". Karl Popper has stated that Darwinism is his "Metaphysical research program;" Stephen Toulmin has, in the first volume of his Human Understanding, sketched an elaborate epistemological analogue to Darwin's theory of evolution ~~of~~ by natural selection and you, Prof. Kuhn, have contended that the evolution of scientific communities--its progress--occurs in a strictly Darwinian fashion.

> The developmental process described in this essay has been a process of evolution from primitive beginnings--a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature. But nothing that has been or will be said makes it a process of evolution toward anything. (SSR, pp. 170-171)

This epistemological view is necessitated because, according to a strictly (or neo) Darwinian account,

> Even such marvelously adapted organs as the eye and hand of man--organs whose design had previously provided powerful arguments for the existence of a supreme artificer and an advanced plan--were products of a process that moved steadily from primitive beginnings but toward no goal (SSR, p. 172).

It seems to me, therefore, that you and Popper agree, in general, as to the mechanism of scientific progress: it is Darwinian--it proceeds by blind trial and error--rather than Lamarckian--a function of effortful instruction.

Yet there is, I think, an enormous difference between Popper's Darwinism and your own. In your view, as I understand it, theories survive or are eliminated as a function of their ability to withstand the criticism of the scientific community but, and this seems crucial, the survival of the theories is unrelated to their worth. Rather their survival is a result of "the existing theory-data fit" (SSR, p. 146). In Popper's view, however, while science does not necessarily move toward the truth it may nevertheless move away from error. Surviving theories are, as a rule, better corroborated, less wrong, than those which are refuted. According to Popper, therefore, the creative power of the human mind is the most important aspect of the scientific process--the mind has evolved in order to solve problems, in order to initiate some halting relationships with the real world. He is therefore somewhat inconsistent; Darwinian science is controlled, driven by a free, goal-directed, Lamarckian mind. You, on the other hand seem to me far more consistently Darwinian--the environment, the sociological environment, is in total control; it is completely responsible for the movement of science. Yet in so doing you seem, unlike Popper, to reduce the mind to a mere epiphenomenal illusion. Scientists, that is, seem to themselves to make a difference but really they do not, they cannot. Popper is therefore, I think an emergent evolutionist while your position is rather strictly Darwinian--ultimately you seem to hold to a kind of radical behaviorism or materialism. The "scientific community" differs in no essential respects, from, say, the North European ice fields--some creatures survived, though they deserve no credit; others perished, though they deserve no blame. This form of evolutionism has been stated most unambigiously by Skinner and, recently, by E.O. Wilson.

My related questions are these: do you believe your view of the history and philosophy of science leads inexorably to epiphenomenalism or even to materialism? What, in your view, is a mind for, if not to improve our knowledge? Though the community may exercise profound (and irrational) control over the progress of science, what, if not goal-directed minds, are responsible for the origin and development of that community?

James G. Blight

William De Bruyn

b) The Logic of Refutation in Child Development and in Scientific Development.

Karl Popper subscribes to the rather homely expression: we learn from our mistakes. Your model of scientific activity, in general, stresses nonmistakes or verifications of scientific hypotheses. Yet not totally. In your article "A Function for Thought Experiments," (in Thinking, Johnson-Laird and Wason, eds.) you explicitly consider the role of conflict in Piagetian conservation tasks. Growth from, say, preoperatory to operatory thought, is the result of conflict, or one might say refutation of a previously held, but less adaptive hypothesis.

> By transforming felt anomaly to concrete contradiction, the thought experiment informed our subjects what was wrong. That first clear view of the misfit between experience and implicit expectation provided the clues necessary to set the situation right. (p. 291)

My question is this: how, without the logic of refutation can anything like the stable progression from preoperatory to operatory thought be understood? Surely the child's earlier hypotheses have had innumerable verifications--but, in keeping with the logical asymmetry between verification and refutation, it is only after a single refutation that growth occurs.

A more general, but related question is this: What is the role of logical systems in the evolution of scientific knowledge? Moreover, if one assumes that one goal of scientific training is to encourage "operatory" (or logical) thought, how might this best be accomplished? Is there, in fact, an identifiable mode of thinking which seems especially to foster discovery in the sciences?

Janet M. Lang

David Bernstein

c) Freedom for the Scientist: An Optical Illusion?

In The Structure of Scientific Revolutions, page 85, you explain that "scientists do not see something as something else;

instead they simply see it... the scientist does not preserve the gestalt subject's freedom to switch back and forth between ways of seeing".

It seems to me that a scientist may confidently, guiltlessly, and even deliberately overlook minor anomalies within his or her various problem situations due to the restrictive nature of paradigmatic boundaries. Thus the sanctuary of a paradigm closes its doors to many interesting and challenging problems; problems which should not be overlooked. My question is this: Is it possible, in your view, to, by effort or brilliance or luck or some fortuitous combination of these, break out from the domination of a "paradigm?" Can a scientist ever, under any conditions, see beyond the boundaries of a paradigm? Can a paradigm ever be affected by facts?

Larry A. Trap

II. On The Idea of a Social Science

The social sciences are themselves a kind of anomaly, a sore thumb, a recalcitrant problem in the nomenclature of the "sciences." Are they really scientific? This problem has of course not lacked for attention; it has been examined from many points of view: Winch, Popper, Hayek, Harré and Secord, and many others have reached various conclusions concerning the status of social science. In your view, as I understand it, a science is a discipline with an accepted, identifiable paradigm and a coherent body of "normal" scientific research. Thus, my related questions are these: Does any social science have a paradigm? What, if anything, prevents, any (or all) social science from achieving requisite, paradigmatic consensus? In short, is fractionation in the social sciences a temporary artifact or a permanent reality?

Douglas Toering

Having just glanced back at our questions, noticing the breadth of their probing and their general immensity, I shudder to think of my own reaction; were these inquiries of me from my students, I might, in a fit of frustration, tell them to read my book. Ah, but that won't do; your book

like all really good books, contributes to one's liberation from the appealing and pervasive myth that the product of careful writing, reading and deliberation is a string of answers. Wrong! The result is questions. You have stimulated us, Prof. Kuhn, into drafting this (admittedly) long, but, we think, hardly verbose letter. We hope, in turn, to have stimulated you to some modest extent.

We look forward to your reply.

Sincerely,

James G. Blight, Ph.D.
Psychology and History of Science
(For all the seminarians)

February 22, 1979

Mr. James G. Blight, PhD
Psychology & History of Science
Grand Valley State Colleges
Allendale, Michigan 49401

Dear Dr. Blight,

Your letter of 5 February has just caught up with me. What you have heard about my difficulty in keeping up with my correspondence is true, but I admire the effort your students have expended and, having a bit more than the usual amount of time this year, I shall try to reply, however briefly and cryptically. Even that I cannot undertake yet, however, for p. 3 (containing the second part of question 1a) has been omitted from your letter. If you will send me a copy of it, I shall see what I can do with the three questions in Part I together.

As to Part II, I do not know enough about the social sciences to have any firm answer, but I will venture the following. Many of the fields which, today, everyone would insist are sciences gained their present status comparatively recently. Earlier, they looked much more like the current social sciences, and many of the same sorts of controversies went on within them. Much of chemistry was in that state through the 17th century, many of the life sciences were still in it through much of the 19th. The transformation of these fields to mature sciences occurred at various times in an unexpected and relatively rapid manner. I see no reason to suppose that the same thing will not, from time to time happen to parts of what are now called the human sciences, but I cannot possibly tell which ones or at what times. Perhaps nothing of the sort will occur, but none of the reasons given by those who insist that the human sciences are necessarily different from the natural sciences strike me as at all convincing. Some of them have appeared before in the history of chemistry and all of them have played a role in the history of the life sciences. They turned out to be wrong in those applications, and I see no reason to suppose that they will be right this time.

The other questions will have to wait until I get the rest of Part I and then find a bit of spare time. Meanwhile, My greetings to you and your students.

Sincerely,

Thomas S. Kuhn

TK:lk

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February 5, 1979

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College of Arts and Sciences, Thomas Jefferson College, William James College, College IV
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J. Blight

James G. Blight, Ph.D.
Psychology and History of Science
(For all the seminarians)

3919 Bibbits Dr.
Palo Alto, Calif. 94303
January 21st, 1967

Dr. Thomas S. Kuhn
Dept. of Science History
Princeton University
Princeton, New Jersey

Dear Dr. Kuhn,

I have recently read your essay, The Structure of Scientific Revolutions. It impressed me enough to stimulate the enclosed effort. While I am not at all sure I can get it published, I am sure you will find it interesting. I would value your opinion.

Sincerely yours,

Louis Bogart

Louis Bogart

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PREFACE

During the thirties I, with many others, was convinced that Marxism had demonstrated the correctness of its assessment of social development. I believed (and still believe) that Marxism had achieved the status of a science. The facts behind that conviction were primarily: the economic crisis of the thirties; the rapid industrialization of the Soviet Union; the rise of fascism in Europe; and the Spanish civil war.

Viewed as a science, Marxism is more akin to astronomy than to chemistry. Experimentation is almost precluded, and Marxists are largely restricted to observation and analysis of historical data. Application, however, is not precluded, and the distinction between Marxism as science and Marxism as social technology has always been fuzzy. The inability of the science to handle many current problems in social technology, as well as the increasing urgency of those problems, has led some people to abandon Marxism. It has also led some to intensify the analysis of historical and contemporary social data in an attempt to develop Marxism.

Recently I came across a book which seemed pertinent to these questions, and which impressed me greatly--The Structure of Scientific Revolutions by Thomas S. Kuhn. In this work, Dr. Kuhn analyzes the history of various sciences. He demonstrates (although he does not state so) that their mode of development is dialectical, and is closely analogous to the development of society as a whole. I consider this work to be highly significant for Marxists on three counts; first, it analyzes a facet of social development; second, it provides an analogue of

social development from which significant parallels may be drawn; and third, it deals with revolutionary crises in the development of sciences--a phase which Marxism is currently undergoing.

I would like to see a dialogue on Marxism develop based upon this book.

To initiate such a discussion I have written the following suggestion. If it seems superficial, remember that it does not represent years of research. It is merely an opening gambit. If it can evoke intelligent criticism and alternative formulations it will have served its purpose.

Louis Bogart

A NEW APPROACH

In the last half century, evidence has been accumulating that Marxism as a science is approaching that need for a revolution, or shift in paradigm as Kuhn* calls it, that all sciences must periodically undergo. In support of this statement I shall mention three sets of problems which have presented great difficulties to classical Marxist theory, although I shall attempt to deal with only one of them.

The first significant anomaly to appear was the Russian revolution in which the first overthrow of capitalism occurred in a relatively backward country rather than in the highly developed capitalist countries with equally highly developed working classes. This anomaly was dealt with by Lenin with the theory that capitalism constitutes a world system, and that when broken the chain would break at its weakest link.

The class aspect of this anomaly, the fact that the main bulk of the revolutionary forces were not of the working class, was explained by saying that they acted under the leadership of the working class. The fact that many of the leaders were not from the working class was explained by saying that the ideology of these leaders represented the true interests of the working class, regardless of the accidents of personal background.

In the years since 1917, there have been many other socialist revolutions, and they have all been of the type of the Russian revolution--revolutions in relatively backward countries with small working classes. In these countries the Leninist development of Marxism has been an adequate guide to action. In the highly developed capitalist countries with large working classes there have been no socialist revolutions, nor have the working classes of these countries shown a tendency to develop in revolutionary directions. On the contrary, with increasing specialization in the division of labor there is an increasing trend away from unity, to be "bought off", to defend the economic enclave of this or that union. In these countries the Leninist development of Marxism has not been an adequate guide to action.

In sum, these facts make it extraordinarily difficult to trace the revolutionary potential from its presumed sources in the industrial working class to its flowering in the socialist revolutions.

Another phenomenon with which classical Marxism has difficulty in coping is the mode of development of society after the socialist revolution. The abolition of the market as the controlling entity of the economic machinery should have, and did, result in bringing that control into the hands of organizations expressly designed for the purpose, but the corresponding control in the development of other social and political forms did not appear. These frequently continued to follow their own uncontrolled modes of evolutionary development, as though the inertial and dynamic forces embodied in pre-revolutionary class structures are still operative.

These facts have been explained in terms of remnants of classes still existing within the new society, and by pressures from external capitalist societies, and in the immediate post-revolutionary period these explanations have indeed been adequate guides to action. As the post-revolutionary societies have developed and increased in number, however, these theories have become less useful. Many political struggles of which the Sino-Soviet conflict is, perhaps, the classic example, have never been adequately explained by orthodox Marxist theory, nor in terms of class.

The most serious divergences between fact and theory, however, come from the highly developed imperialist countries. In those countries in which the military-industrial complex** characteristic of the imperialist stage of capitalism has come to dominate the entire society, the classical cycle of increasingly severe economic boom and bust (with the prognosis of a complete breakdown of the market economy) seems to be suspended indefinitely. This stabilizing influence is possible because both halves of the military-industrial complex are shielded to some extent from the effects of the market--the first by its non-economic function, the second by its monopolist

position. The price paid for this stability is the dependence of the economy on wars and military adventures.

The apparent stabilization of the market economy was a severe blow to Marxist theory, but it was the concomitant dependence on war, that, with the emergence of nuclear weapons, precipitated the present crisis in Marxism. It then became glaringly evident that while on the one hand capitalism was not collapsing of itself, on the other hand the dictum that no ruling class ever surrendered without a military struggle (when possible) precluded any attempt at a forcible overthrow, since a solution to the problem which includes the probable extinction of the human race is not a solution.

Furthermore, the problem could not be abandoned, for the uncontrolled nature of all forms of capitalism ensured that if the problem of disposing of it were not solved, capitalism itself would eventually bring the world to an international nuclear war.

It is to this central problem of overcoming capitalism in the imperialist countries that I shall address myself.

It is evident that any solution to this problem includes the necessity of destroying the military-industrial complex, and furthermore, of destroying it by non-military means.

It is also evident that the market economy has been stabilized and made viable only by the superposition of the military-industrial complex, and that without it the economy may be presumed to be unstable and non-viable.

Since the destruction of the military-industrial complex is both necessary and sufficient to solve the problem of overcoming capitalism, it <u>is</u> the solution to the problem.

I submit the proposition that if one wishes to determine revolutionary potentials under these conditions, one should classify people as oppressors or oppressed, not with respect to the means of production as a whole, but with respect to that part of the means of production which constitutes the military-industrial complex.

Hence, under these conditions, a meaningful analysis of imperialism must start with this modification of the basic

definition of class: "a class is a group of people that stands in a definite relation to the military-industrial complex."

While I do not propose to make such an analysis here, a casual inspection of the contemporary American scene will show the outlines of it.

The group of people most oppressed by the military-industrial complex is currently the youth, some of whom are drafted and forced to risk their lives with little real motive or recompense, and most of whom find their lives disrupted by the overhanging threat of such treatment. Within this group, the Negro youth constitute an even more oppressed sub-group whose members are drafted in disproportionate numbers, and provided with even less motive and recompense than the average.

The industrial workers do not constitute a single class under this definition. Some of them find their incomes and welfare directly dependent on the continued prosperity of the military-industrial complex. Others find themselves indirectly dependent on the continued prosperity of the military-industrial complex but are adversely affected by the disproportionate drain of surplus value into the military-industrial complex, manifested by such things as taxes, inflation, high interest rates, high corporate profits, etc. Some groups may find themselves oppressed economically to the point of rebellion. In general, though, the oppressive features of the military-industrial complex are increasingly manifested through its military aspect and revolutionary potentials in the various groups of industrial workers are increasingly developed through family relationships with the draftable class.

In precisely the same way, this definition must cut across the line of the orthodox categories of bourgeoisie and petty bourgeoisie, and in precisely the same way revolutionary potentials in these groups must be sought primarily through their draftable members, and only secondarily through any direct economic oppression.

Seen from this viewpoint, the American scene ceases to be a hodge-podge of anomalies. The apathy of the unions, the

ferment on the campus, the riots in the Negro ghettoes, the dichotomy in war reporting by the major news media--these and other contemporary phenomena become as simply related to theory as the revolt of the grape pickers. Even more important, this viewpoint provides those interested in realizing and developing revolutionary potentials a guide as to where to go to find the men and money for the work.

* Thomas S. Kuhn: The Structure of Scientific Revolutions. University of Chicago Press, Chicago. 1962.

** I use this popular term because it refers to a bi-partite entity, rather than the more correct "state monopoly capitalism" which refers to a stage of social development.

The term "military-industrial complex" has the advantage of emphasizing the fact that "defense" is the most convenient slogan under which the power and wealth of the complex can be extended at home and abroad.

Nevertheless, the incorrectnes of the term should be recognized. It is not all industry, but precisely those industries that have merged with the finance houses to create monopolies, enclaves sheltered from the competitive pressures of the market, that constitute one half of the complex. It is not just the military, but the entire state apparatus that constitutes the other half. It is not just the protection and extension of foreign investment, but also the milking of the home economy, the exploration of new fields for private investment through state supported research and development, the appropriation of public funds through essentially non-competitive contract allocation, that characterize this parasitic entity.

31 January 1974

Dr. Radu J. Bogdan
Bd. Republicii 38
Sector 4
Bucharest, Romania

Dear Dr. Bogdan:

I am sorry to be late in answering your letter of 23 December but it has been difficult to find the time needed to put together the material you requested. Now I believe I can supply what you need. In any case, I am delighted to hear that the Romanian edition of Structure is so nearly ready for publication. My thanks and my congratulations to you.

I enclose a photograph, a brief Vita, and a bibliography. The latter is not quite up to date, however, and you may possibly want the following information. My first book, The Copernican Revolution, has recently appeared in French and in Italian. There are also now three additional translations of Structure: Spanish, Dutch, and French. The article "Second Thoughts on Paradigms" (item 38 on the list) is unfortunately still in press, but it should definitely be out during this year, 1974. Finally, the monograph series described in the closing paragraph has now somewhat changed its state. I am now revising a first volume titled The Black-Body Problem and the Quantum Discontinuity, 1894-1912. I do mean to go on to at least some of the other volumes mentioned in the sentences which describe my current research.

Again, my greetings and best wishes to you.

Sincerely,

Thomas S. Kuhn
M. Taylor Pyne Professor of the History
of Science

TSK/cm
Enclosure

Radu J. Bogdan
Bd. Republicii 38
sector 4
Bucharest, Romania

Professor Thomas S. Kuhn
Princeton University
220 Palmer Hall
Princeton, New Jersey 08540
U.S.A.

December 23, 1973

Dear Professor Kuhn,

Due to your very kind cooperation, the copyright problems concerning the Romanian translation of your SSR are now completely solved: the agreement was signed by both parties.

My translation is completed, and I am now expected to write, as I anticipated earlier, a small Introduction to your work and its influence on recent historiography and philosophy of science. I need therefore a Bio-Bibliography of yours and, if possible, copies from your recent works. Also, your photograph will be needed for the back cover of the Romanian edition. (I do not dare to hope for a short Preface to the Romanian edition!)

If all this is done rather quickly, I hope to be able to deliver the whole manuscript within two months. With good luck, it might be published during 1974.

Many thanks again for all you did for this translation - and for me.

With warm greetings and best wishes for the new year,

Sincerely Yours,

Radu J. Bogdan

Milano, 1.8.1980

Professor Thomas Kuhn
University of Princenton
USA

Dear Professor Kuhn,

herewith enclosed is a copy of the communication I presented at the "International Interdisciplinary Cycle Research Symposium", which was held in Trier (Germany) during last july.

As you can see, this work deals with the case of an Italian scientist; a case which I have tried to explain on the basis of your theory.

I do hope that this case will meet your interest. However, I would like to receive your comments, even if negative.

Because the reduction of the text for obvious reasons of time, I remain at your disposal for any further informations.

I am a chemist and I have for some years been taking an interest in science history.

I apologize for my imperfect English and I thank you very much for the kind attention you will give to this work.

Yours sincerely,

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GIORGIO PICCARDI: A KUHNIAN CASE OF SCIENTIFIC REVOLUTION

Abstract

Giorgio Piccardi (1895 - 1972) is one of the most significant figures in the field of "Fluctuating Phenomena". During 1930s, he had noticed that some chemical reactions were non-reproducible. He at first attributed this "anomaly" to experimental errors, and (after years of severe experiments) to the influence of environmental variables. According to Kuhn's theory, regarding the development of scientific knowledge, the overcoming of an anomaly - on the basis of a completely new hypothesis - would correspond to having a new

"paradigm" that opposes current paradigms.
Moreover, a new paradigm would give rise to new researches - characterized by specific methodologies.
In fact, Piccardi's "chemical test" was an original procedure for revealing the action of spacial forces upon complex chemical systems. The results obtained substantially confirm his hypothesis and contain the germs of a "scientific revolution" - for both, natural as well as human sciences.
Because these and other aspects (particularly in view of the missing acknowledgement of these important results by academic science) Piccardi's case may well be considered on the basis of Kuhn's theory.

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Abstracting keywords: 'Science History' , 'Fluctuating Phenomena' , 'Chemical Test'

GIORGIO PICCARDI: A KUHNIAN CASE OF SCIENTIFIC REVOLUTION

Introduction

Giorgio Piccardi (1895 - 1972) is one of the most significant and original figures in the field of "Fluctuating Phenomena" and natural cycles. However, the aim of the present communication is not mainly to show his scientific merits or to discuss the results of his pioneering research, but to point out that, on the whole, Piccardi's work may well be considered to be "a Kuhnian case of scientific revolution".

Some main aspects of Kuhn's theory

As is known, Kuhn has for many years been taking an interest in the topic of scientific discovery. He is the author of a very successful book, from which it is possible to extract the features of his theory. (Kuhn, 1969)

Kuhn's basic idea is that the development of scientific knowledge takes place in a discontinous way - with long periods of relative stability alternating with short, but intense, periods of crisis. This occurs on the basis of either the acceptance or the refusal of some models - which condition the various aspects of the research, and which by Kuhn are called "paradigms".

In the past, paradigms have been the immobility of the Earth, the Newtonian mechanics, etc.

To-day's paradigms are the conservation of energy, the expansion of the Universe, etc.

During stability periods, paradigms are not subjected to severe criticism, and by them day-to-day research (the so called "normal science") is carried out, textbooks are compiled, technological applications are planned, etc.
On the basis of the current paradigms, however, not "all" the experimental facts can be explained; there always remain some "anomalies" that scientists try to solve as if they were "puzzles" - namely through the ambit of known theories and by means of accepted logic and experimental rules.
Nevertheless, it may so happen that an "excess" of anomalies or even just one, but radical, anomaly, may convince some scientists into believing that current paradigms are no longer adeguate to existing facts and that it is necessary to fix new ones. This is the sympton of a crisis - which could lead to a "scientific revolution".
A scientific revolution however, can only be completely realized through a concurrence of several factors, either scientific or extrascientific: a better accordance between theory and experimental evidence, the "converting" of some school-leaders to new paradigms, and a social and cultural situation that is apt for winning-over conservatorism, etc.
In the present communication I do not intend judging the degree of validity of Kuhn's theory,

which has been skillfully done by several epistemologists, mainly with regard to Popper's theory. (Popper, 1972) (Feyerabend et al., 1976) The aim of this communication is simply to demonstrate how Piccardi's case can receive a good interpretation on the basis of Kuhn's theory.

The "activation of water"

In the first part of his career, from 1922 to 1935, Piccardi took an interest in certain physico-chemical problems, which were still open in the '30s : electronic potentials, spectroscopy, rare-earths, etc. Despite the high level of his contributions during this period Piccardi dealt with "normal science". In fact, all his work (discoveries, rectification of preceeding data, etc.) was carried out through the observance of the paradigms of time.

The turn of his thought occurred in 1935, when Piccardi acquired a chance knowledge of a strange device, constituted by a closed glass globe, containing a drop of mercury and neon at reduced pressure. This patented device was employed for the physical descaling in boilers. (Abbot, 1931) Piccardi noted that some chemical and biological processes in aqueous phase, were subjected to great alterations, if the water had been previously stirred by that globe. For example, the precipitation of some insoluble compounds occurred, either with a remarkable delay or prematurely - as with respect to the same

reaction in water that was not submitted to such treatment, and also the precipitate morphology could notably vary. Moreover, acquatic plants when supplied with water so treated, rapidly perished. (Piccardi, 1938)

Piccardi was the first to study this particular phenomenon (which he called "activation") in a rigorous way. But above all he was the only one who did not draw back in the face of such a baffling anomaly as the non-reproducibility of the results obtained.

The anomaly of the non-reproducibility

The non-reproducibility of the results obtained consisted in the fact that, even with operating under identical conditions, the activation produced different, and sometimes quite opposed, effects.

For many years Piccardi had been attributing this ponderous anomaly to experimental errors. But, after having carried out severe experiments, and having considered the persistence of the anomaly, he was finally convinced that its origin was to be found elsewhere.

Thus, he devoted his attention to space - namely to those forces that incessantly and inconstantly act in the surrounding ambient, and which cannot be kept under control. If a system (such that of activated water) is sensitive "also" to these external forces, its evolution produces different

results during the course of time.
As can be seen, the anomaly of the non-reproducibility is now explainable on the basis of a new paradigm - opposed to the current paradigms which exclude any notable influence of external forces upon ordinary chemico-physical systems.
With reference to this point, Piccardi writes that he was induced into formulating this hypothesis through a "careful observation" of the experimental results, and after having noted that the results actually reversed in concomitance with some important occurrence in the ambient (meteorological disturbances, sunspots, etc.). (Piccardi, 1951)
But such an unprejudical conclusion may only find its explanation if the "careful observation" is sustained by free exercising of thought.
According to Kuhn, the two main requisites for giving rise to an episode of discovery are self-evident here. The first requisite being the skill or genius for recognizing, despite the colleagues' opinion, the importance of what actually went wrong. The second requisite being the fact that the anomaly may emerge just as a violation of expectation, that is to say, only when scientists know well both how their instruments and how nature should behave.

The "chemical test"

According to Kuhn, a really new paradigm will

constitute a radical breaking-off with respect to the past. It will suggest the rectification of preceeding data and will open autonomous fields of research. In this particular sense, it is possible to understand Piccardi's subsequent activity in the field of "chemical test", starting from 1951.
The chemical test essentially consists in a systematic and standardized execution of a chemical reaction over long periods of time; it presents two interdipendent aims: to relieve and possibly to quantify spacial forces, and to study the influence of spacial forces upon complex chemical systems.
The study of a new order of phenomena requires of the researcher not only new mental patterns, but also the establishing of experimental procedures and equipment that are adeguate and which will conform to the new paradigm. In this case, the problem was creating a method that allowed for eliminating any accidental errors at play, and which enphasized of external forces at play; namely which distinguished chance from non-chance.
Piccardi solved this problem through a sophisticated methodology based on the simultaneous effectuation of a large number of "differential" proofs. He chose for the routine-test the hydrolysis of bismuth tricloride with the formation of insoluble oxicloride highly dispersed.
Through the hundred and thousands of tests carried out during more than twenty years, Piccardi

demonstrated that the pattern in the course of time in this reaction rate may be correlated with the patterns in the same period of time of spacial variables of terrestrial, solar and cosmic origin (electromagnetic fields, sunspots, the helicoidal motion of the Earth in the Galaxy, etc.). (Piccardi, 1962)

The "Fluctuating Phenomena"

According to Piccardi, "open" systems exist, which can respond not only to "traditional variables" (temperature, pressure, etc.) but "also" to "non-traditional variables" (known and unknown spacial forces). Because these latter variables fluctuate incessantly, the evolution of the open systems therefore gives place to results that are changeable in the course of time - and hence, may be called "Fluctuating Processes" or "Fluctuating Phenomena".

Piccardi has tried to define these systems through the following general law:

"The heterogeneous and out-of-equilibrium systems when sufficiently complex, respond to any external stimulus, even if its energy is extremely small".

This law may be completed by the following corollary:

"The common inorganic colloids in evolution are, in general, sufficiently complex as to possess the property just defined. The colloids of living organisms are even more so. Therefore, a large

part of the chemical reactions and all the biological processes, are sensitive to external stimuli". (Piccardi, 1969)

This Piccardi law does actually constitute a new paradigm, the validity of which extends to the field of all natural and human sciences. In opposition of an old idea, that is still held valid today, we have to consider as valid some chemical and biological processes that are not reproducible with time, and whereby time is no longer only a duration, but also a "coordinate".

Actually, the science of "Fluctuating Phenomena" is still lacking in some of its essential parts; namely, a unified explanation of the mechanism in the action of spacial forces, an accurate statistical elaboration of the available data, etc. For this reason, it may be said that Piccardi's work does not constitute a "scientific revolution" yet, but that however, does it contain the germs of a "scientific revolution".

Other aspects of Piccardi's case

There are other aspects of Piccardi's case which can find a coherent interpretation in the Kuhn's model.

First of all, the lacked acknowledgement of his work in academic science. Exceptions to this can be found as individual scholars - such as M.me Capel in Brussels, and also certain scholars in the Soviet Union (this latter nation has a long tradition in the field of the solar-terrestrial relationship). (Capel Boute, 1960)

Nevertheless, the correctness of Piccardi's idea was also verified whenever other scientists carried out chemical test (as it took place during the International Geophysic Year in 1957 - 1958). Moreover, Piccardi's hypothesis was never scientifically disproved.

According to Kuhn, a negative attitude on the part of academic science towards a really new paradigm, is a regular factor in science history. There may be several scientific and extra-scientific reasons for this. On the other hand, untill such time as the major part of the scientific community has not be become "converted" to this new paradigm, Piccardi's work will not be regarded as a "scientific revolution", but will remain rather, as a "scientific curiosity".

A second, and even more subtle aspect refers to a particular "blank test", carried out by Piccardi for verifying the validity of the test procedure, namely for verifying the absence of systematic errors. This "blank test", which did confirm said procedure, was executed in 1954, three years after the initiation of the routine test. Paradoxically, because of the positive results in the chemical test during the course of these three years, this "blank test" was no longer necessary.

This fact demonstrates how the development of

a research does not usually follow the logical and rigorous path attributed to it "a posteriori". Moreover, as Kuhn underlines, the early steps of a research that is not yet ruled by accepted paradigms, are often guided more by intuition and personal belief, rather than by a series of severely logical deductions.

Conclusions

As I have tried to demonstrate, Piccardi's work represents, besides a basic contribution in the field of "Fluctuating Phenomena", also a significant example of a Kuhnian case of "scientific revolution".
It is desiderable that this contribution should be more widely known; acknowledged and appreciated by academic science, for striving towards a better understanding of man and nature, and particularly as regard the complex relationship existing between them.

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UNIVERSIDADE ESTADUAL DE CAMPINAS

UNICAMP

Campinas, september 6, 1977.

Sent 9/10/77 TKS

Dear Professor Kuhn:

I have recently read, on a Brazilian newspaper article about the evolution of your ideas, of your two articles "logic of discovery or psichology of research" and "Reflections on my critics". These articles appeared, so it was said, on a book of which our library unfortunately does not have a copy. I then take the liberty of asking you to send me, if available, a copy of each of these two articles.

I am just a theoretical chemist interested in philosophy of science. I first knew of your ideas through reading a Brazilian translation of the 1st edition of "The structure of Scientific Revolutions". Now I am told that the second edition includes some significant changes, and since I was not able to find here a copy of it either, I am asking you the reprints above mentioned, or/and any others in which you "update" your views.

Last but not least, I must say that I liked very much "the structure of Scientific Revolutions".

Thank you.

Benício de Barros Neto

These are my name & address →

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Ackn. 26.XII.62

MEMORANDUM from EDWIN G. BORING

18 December 1962

Tom Kuhn:

Science has just reviewed your book. 14 Dec. Gillespie of Princeton. It's the leading review and runs 2500 words or a little more. That's distinction with Science running 60,000 circulation now, I think.

I wondered if Copenhagen got Science promptly and if your friends would at once send you a copy. So here is an electrostat, as I want to keep the original. If it duplicates something else, no harm is done.

Gillespie shows more dubiety about your thesis than I do, but then I see that you fit well into my developing "undercurrent of credence," as Gillespie specifies it at the very end of the last paragraph of the review.

All in all it seems to me an excellent review and you should feel pride. Besides you are rescued by the fact that this is a paradigm about paradigms and revolutions are slow affairs, so that Gillespie's dubiety is part of the proper picture for a revolution, the inertia of the Zeitgiest.

There are other self-sustaining arguments like this. An old one was that failure to accept the Freudian view is a demonstration of ~~the~~ the resistance to be expected ~~of~~ so denigrating a theory.

P.1252, middle column, top, "hidebound scientist." There I do not follow Gillespie as to what he found in your thought. I felt that you found the scientist eager for change and revolution. At any rate that is right, and I thought you were right.

Merry Chirstmas! Yrs., EGB.

The Nature of Science

Normal science is succeeded by a creative phase of revolution out of which new concepts emerge.

Charles C. Gillispie

This is a very bold venture, this essay, **The Structure of Scientific Revolutions** by Thomas S. Kuhn [University of Chicago Press, Chicago, 1962. 187 pp. $4]. Kuhn has been turning it over in his mind and developing it in his work ever since the beginning of his career as a physicist-turned-historian. That was some 15 years ago during the course of James B. Conant's program for imparting science through historical examples in the service of general education. Now Kuhn would thrust more deeply into science than pedagogy will reach. His opening sentence is unequivocal: "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed" (p. 1). For he is not writing history of science proper. His essay is an argument about the nature of science, drawn in large part from its history but also, in certain essential elements, from considerations of psychology, sociology, philosophy, and physics. The reader is not to expect philosophy of science in the usual Anglo-American sense of a study of logical problems found in scientific proceedings or systems. Rather is this a sketch for a genetic philosophy of science, presented in earnest of a fully developed study promised for the future.

The author starts in the conviction that what he calls the accepted concept of science is misleading. This he describes as a view that science consists in an aggregate of facts, observations, laws, theories, and techniques for getting more such results—in short, that science is a body of information which has accumulated in a linear series of discrete discoveries about how the world is made. Kuhn's critique of the very notion of scientific discovery may, indeed, be the strongest part of his argument, and is certainly at the heart of it. It was not, of course, difficult for him (though it is still very useful) to demolish the notion that inventions of theory —Maxwell's laws of the field, say, or Newton's laws of motion, or general relativity—were found like hidden treasure or a misplaced hat, there in the logic or structure of things, wanting mainly to be revealed. Neither, however, will he allow novelties of fact—the indentification of oxygen, for example, or of radioactivity, or even of Uranus—to be counted as discoveries, if we mean thereby elementary increments of information by which science grows in volume and, in growing, has its history and progress. He tends to annul the distinction between finding fact and inventing theory. A fact is itself only by virtue of theory and vice versa.

In what, for instance, did the discovery of oxygen consist? The gas appears to have been first isolated by Scheele, who did not publish and had no influence in the matter. Priestley prepared an impure sample from the red oxide of mercury in 1774, mistook it for laughing gas, and when he came to recognize it rather as a new species, dubbed it dephlogisticated air. Lavoisier, for his part, instantly recognized Priestley's gas (though not Priestley's priority) as what combines in combustion and calcination, initially took it for pure air, and when he did see it as a fraction of the atmosphere, always considered its essence to be the principle of acidity requiring to be combined with caloric to assume the gaseous form. Valence, diatomicity, specific heats—knowledge or definition of those and other properties lay in the future, and who may say at what moment oxygen took on the identity that it has since attained? This depends on what one means by oxygen. One could mean simply what we breathe, and what was fundamental for science was less the specification of the gas than the chemical revolution wherein study of its combinations played a forcing role.

That revolution consisted not just in a rush of new reagents, new reactions, new techniques, new methods of analysis, though these there were in plenty, but also in a new way of seeing these materials of a science. Very important to Kuhn's argument are findings of the modern psychology of perception which make the literal notion of seeing as ambiguous as that of scientific discovery. He refers to the well-known shifts of gestalt in which an observer switches back and forth between seeing a rabbit or a duck in some appropriate design, and he also calls attention to more elaborate experiments which make seeing a function of habitual expectation as well as of optics and physiology. A man may, for example, be conditioned by his expectations of the world to overcome the initial malaise inspired by inverting lenses and see things right side up, and only on repeated exposure will one recognize for what it is the anomaly of a black five of hearts planted in the deck of cards. Kuhn would extend the consequences to the sense of seeing in which the verb means conceiving.

A science, then, is how its practitioners as a highly articulated group see the ensemble of its phenomena, and this proposition takes Kuhn into considerations of the psychology and sociology of scientific communities and of what factors lead them to respond to, or to resist, innovation. They are the keepers of what the author calls *paradigms*. For, although science does not develop as a deposit of empirical discoveries economized now and again by theory, it nevertheless does have its evolutionary pattern. To a phase of "normal science," when scientists are agreed upon their paradigm and seek mainly to perfect it, succeeds, as a consequence of anomalies, a creative phase of revolution out of which emerge new paradigms to replace the old and run their course. What is more, the shift from one to another occurs rather as the conversion of a community than as the persuasion of persons by bits of new evidence or shorthands in theory.

The reviewer is chairman of the graduate program in the history and philosophy of science at Princeton University. His latest book, *The Edge of Objectivity: An Essay in the History of Scientific Ideas*, was reviewed in *Science* [131, 1203 (1960)].

Paradigms of Normal Science

This is an interesting schema. Let us develop and exemplify its elements a little more fully. Kuhn gives to the word *paradigms* a special significance and an importance all his own. One sees what he means, although he gives no precise definition. *Paradigms* are what give coherence to modes, or better, perhaps, to schools of science: to Ptolemaic or, contrariwise, to Copernican astronomy, to Aristotelian dynamics or to statistical mechanics, to phlogiston chemistry or to uniformitarian geology. Usually they are born in achievements like Newton's *Principia* or Lavoisier's *Traité élémentaire*, works exemplary enough to impart a tradition to a train of scientific research. They may be sufficiently comprehensive to contain a whole science like classical physics. Or—paradigms within paradigms—they may govern special and often quite restricted domains, such as the corpuscular picture of light which was displaced in the revolutionary way by the undulatory.

For Kuhn the concept has an importance going beyond a physical model, though it contains that. He particularly wishes to emphasize that paradigms are what lay down the law to neophytes: "The study of paradigms is what mainly prepares the student for membership in the particular scientific community in which he will later practice. Because he there joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom evoke overt disagreement over fundamentals. Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, that is for the genesis and continuation of a particular research tradition" (p. 11).

Nevertheless, a paradigm in Kuhn's sense is no closed set of propositions and practices, not quite an object for replication as it is in grammatical usage where one verb is the pattern for an entire conjugation. "Instead, like an accepted judicial decision in the common law, it is an object for further articulation and specification under new or more stringent conditions" (p. 23). Of such is the business of "normal science," each era of which is lived in service to some paradigm. Gathering fact, choosing that which is relevant, building and using appropriate instruments, developing applications, determining constants, formulating theory in more economical or more general expression—amid such "puzzles" do all scientists lead most of their lives and most scientists all of their lives. Normal science, moreover, is cumulative in the fashion which has been mistakenly attributed to the whole course of scientific progress. It solves its puzzles, extends its range, and refines its measurements. Not here, however, not in this work-a-day activity, does anything original happen. For normal science never innovates. Kuhn is consistent with his argument. He represents most scientists as rather a hidebound lot, not at all eager for fundamental innovation, as men who like to know where they are and where their work fits, who even tend to resist novelties which unsettle the paradigm and with it the intellectual security of the community.

Anomalies, Innovations, and Scientific Revolutions

Novelty will out, however, and inevitably. Anomalies do occur—the problem of what moves the mobile in Aristotelian dynamics, gathering complexity in Ptolemaic astronomy, augmented weight after combustion in phlogiston chemistry, contradictions required of the ether first after Fresnel and more generally after Maxwell. Logically anomalies are not to be distinguished from puzzles which are the business of normal science. Both may be described as counter-instances to some theory not quite adequate to the matter in hand. Anomalies, however, have the property of persistently suggesting, not simply that we do not quite know how to work the paradigm, but more seriously that at certain points the fit with nature fails. When these points begin to seem crucial or numerous enough, the failure becomes a scandal; the affair reaches the state of physics in Einstein's time or of astronomy in that of Copernicus. Then it is that a revolution occurs, a shift of gestalt into a new mode of seeing things which breaks with the established order, that of the old paradigm, and by first converting and then commanding the allegiance of a (usually new) set of practitioners becomes in its turn the paradigm of a new phase of normal science. Kuhn is very severe with positivists, however. These events are never brought about by the simple sort of methodological precept which has it that a theory is abandoned or modified should some instance of it fail: "The act of judgment that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world. The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature *and* with each other" (p. 77).

Thus, Kuhn sets great store by the necessity for scientific revolutions. He will have none of the sort of reconciliation which makes classical physics a special case of relativistic physics. Newton's laws are derivable from Einstein's work only by presupposing the latter, which created a quite incompatible system of physical referents. "What had previously been meant by space was necessarily flat, homogeneous, isotropic, and unaffected by the presence of matter. If it had not been, Newtonian physics would not have worked. To make the transition to Einstein's universe, the whole conceptual web whose strands are space, time, matter, force, and so on, had to be shifted and laid down again on nature whole. Only men who had undergone or failed to undergo that transformation would be able to discover precisely what they agreed or disagreed about. Communication across the revolutionary divide is inevitably partial" (p. 148).

I do not think that it is overstating Kuhn's argument to say that he regards the revolutions of which he writes as the only creative episodes in science. They shape the research by which the emergent paradigm is perfected throughout the course of its usefulness. Even more fundamentally, the work of imagination and discipline which the men who make the revolutions bring to bear on phenomena are constitutive of nature itself. Afterwards science transpires in a different world, heliocentric as opposed to geocentric, sequential and curvilinear rather than enduring and rectilinear.

Summary

It is not for a historian of science to pass judgment on the central critique of Kuhn's essay, since that is directed to science itself. A few reservations may be ventured, however, of a sort which the author of so searching a discussion will certainly expect, the more so since,

in his own terms, he proposes nothing less than a revolution in our concept of science, if not of nature. But it is not clear to me that anyone really holds the view of science which he would demolish. I for one find a great deal more in this book to agree with than might be expected in an exponent of a counterrevolutionary school. The argument depends very heavily on the viability of the terms—*paradigm, normal science, revolution, anomaly, crisis*, and the like. So it has been with many a philosophy of history from Comte to Toynbee. So it has been with many a chapter in the history of science—phlogiston, calorie, ether—and the student of either of these genres (which Kuhn, like Comte, combines) will have learned to be wary of mistaking the terms he gives his subject for its elements, the definitions for the happenings. The argument sometimes comes perilously close to circularity: that is, normal science does not aim at novelty, ergo what is novel is not normal science but an anomaly. On strictly historical grounds, moreover, strong cases might be made for considering books like Newton's *Principia* and Lavoisier's *Traité élémentaire* as summaries of a heritage rather than as models shaping the future. The reader may be referred, for example, to E. J. Dikjsterhuis's treatment of Newton in his recently translated *Mechanization of the World Picture*, where it appears that Newton himself did not adumbrate the laws of motion in the sense in which they were fundamental to classical physics. For example, the proportionality of force to the product of mass into acceleration was imported into the second law in the development of analytical mechanics, not forced upon a school by a revolutionary law-giver. Newton was thinking of impact.

Still, there are not many books which find one making eager jottings in the margin, nor fortunately need one act on these; one may instead, and indeed in candor must, await the full development that Kuhn intends to provide. Meanwhile there can be only admiration for the erudition, the scholarship, the fidelity, and the seriousness that the enterprise reflects on every page. One is safe in predicting that whatever the final success, there will be no petty faults to find. Every historian, moreover, will surely applaud one recurrent and fundamental emphasis, which is that the development of science must be set into the context of a Darwinian historiography and treated as a circumstantial evolution from primitive beginnings rather than the ever closer approach to the telos of a right and perfect science. It is odd, and Kuhn is absolutely right about this, that by instinct scientists tend to see it the latter way. At least their students do, and who else could be responsible for that?

Subsidized Irrigation

The Value of Water in Alternative Uses. With special application to water use in the San Juan and Rio Grande basins of New Mexico. Nathaniel Wollman, Ralph L. Edgel, Marshall E. Farris, H. Ralph Stucky, and Alvin J. Thompson. University of New Mexico Press, Albuquerque, 1962. xxii + 426 pp. Illus. $10.

Is traditional irrigated agriculture really the most efficient use of water in arid regions? If industrial use were expanded, would an adequate labor force and sufficient investment capital be available? With development of the San Juan River and a diversion to supplement flow into the Rio Grande in prospect by 1975, these questions are pertinent for the State of New Mexico, and indeed, for other arid regions. Resources for the Future financed a study, carried out at the University of New Mexico, directed by Wollman and a committee (his coauthors) with the support of several subcommittees composed of faculty members and representatives of the state government. A summary of the study constitutes the first 125 pages of this book; thirteen appendixes, written by the subcommittees or individuals responsible for specific phases of the study, present supporting data and detail the methods employed.

Eight possible patterns of water use were tested. Two rates of diversion from the San Juan River to the Rio Grande and four different schemes of allocation between municipal-industrial and agriculture-recreation were analyzed. Estimated benefits for each pattern of use were obtained by estimating the primary value added for agriculture and industry plus the value added by purchases for all three uses. That industrial water yields by far the greatest return per unit of water use will surprise no one who has considered this problem. That recreational uses rank second and agriculture a poor third may be more surprising. In fact, the conclusion is that irrigation is not possible without subsidy. Utilization of all new water in New Mexico for industry is not feasible, however, because neither adequate labor nor capital can be anticipated. The so-called "high industry" models utilized only about half of the available water for industrial purposes.

The reader may be troubled to find that a situation exists that suggests that the water must be put to use, even if a subsidy is necessary, in order to avoid its diversion to other areas. The reader may also be troubled by evidence that a truly adequate basis for evaluating recreational benefits is lacking, and he may question the inclusion of secondary benefits in the calculations. Methodology, the impact of the various models on the labor force, and the differences between the state and national viewpoints are discussed in an interesting manner.

Since the project would be built with federal funds, the reader may wonder how it would rank among all possible projects which the nation might contemplate. No matter how much one may wish to debate the details of the analysis, it serves to make its intended point without ambiguity—no rational water plan can be developed without a careful study of the value of water in competing uses.

RAY K. LINSLEY
Program in Engineering-Economic-Planning, Stanford University

Earth Science

Petrology. Walter T. Huang. McGraw-Hill, New York, 1962. vii + 480 pp. Illus. $9.75.

This book, which is intended for beginning students and others who need a comprehensive introduction to the broad field of petrology, is not a distinguished effort, although it does have some advantages when compared with many older texts. Its advantages include some use of recent experimental studies on oxide and silicate systems, abundant illustrative material from recent geological literature, and a more up-to-date treatment of the rock-forming minerals from the viewpoint of crystal chemistry. The numerous illustrations are rather well selected,

AIRMAIL

Copenhagen, Denmark
December 17, 1962

Professor Edwin G. Boring
Psychological Laboratories
Harvard University
Cambridge 38, Massachusetts

Dear Professor Boring:

I was proud to have your review even before you forwarded the good news that it would be printed. You continue to have all my current thanks. As for the emmendation you have introduced, I am grateful for it, but would not have protested the original formulation at all. There is an amusing (?) ambiguity here, for which I am entirely responsible. I read the phrase "as art, political theory and philosophy do not" to mean "in ways that art, etc., do not." That is what I intended in the sentence that you quote back at me from the book. Your reading is, of course, at least as proper, but I had no intention of permitting it. I can now start hoping for a second edition.

Since you generously invite other comments, I shall be so ungracious as to make one, though not of a sort that should evoke anxiety. My pp. 170-172 are largely devoted to the parallel between the Darwinian revolution and the one I'm advocating in epistemology. Obviously there's much more to be said on this point, and I have played it down too much. But is it quite correct to suggest (your p. 5) that I missed the chance to make that point. I shall quite understand if you leave this passage alone. In fact, I rather hope you will. If I persuade you to remove the one critical remark you have made, you may replace it with something far more devastating.

I am also entirely pleased with the changes you have made in your Presidential Address. Though I should have been content without them, they do minimize the likelihood of misunderstanding. In addition, I plead guilty to putting my point about determinism very clumsily. You are quite right to spank me. But about the essentials I am still not entirely convinced.

You have isolated the crux of the difficulty wonderfully. When I wrote you, I was, for purposes of that discussion, strapping myself into "the Laplacian straightjacket." I had assumed that you were using the term 'determinism' in that sense, and it was exactly the rigidity which would have resulted that I aimed to protest. Having read your answer, I am convinced that we agree entirely <u>about history</u> though I am not sure your auditors may not share my initial misunderstanding.

But that agreement does not really resolve the basic problem. I would say that what we agree about is something less than determinism, and I think you will not follow me so far. Probably we shall not come to terms through the mails, but let me at least try to make my reservations clear. If you claim, as I think you do, that we display only our ignorance when we suppose we see chance in history, then you are yourself donning the Laplacian straightjacket, or at least I am aware of no alternative position for you. Inevitably you seem to be saying: if only we knew more, we would see that things could not have been other than they are without a violation of natural law. I am far from sure that that would be right, and have no recourse but to suggest you leave some room for chance.

Let me add immediately that I suspect I am being victimized by false alternatives. There ought to be some "higher" sense of determinism which would avoid the Laplacian straightjacket. But I think no one has yet developed it. Furthermore, I suspect that when someone does, they will dissolve the whole issue of chance vs.determinism in the process. At that point we will see a sense in which an event can be simultaneously due to cause and chance or due to neither. Meanwhile, though, even posing the issue raises the spectre of Laplace. I am not convinced you have layed his ghost, but I hope neither it nor I will haunt your Christmas table.

Again my very warmest thanks for all you have done to help.

As ever,

Thomas S. Kuhn

HARVARD UNIVERSITY
Psychological Laboratories

Memorial Hall
Cambridge 38, Mass., USA

11 December 1962

Dear Tom Kuhn:

Sanford writes this morning that he is glad to accept my review of your book for publication in Contemporary Psychology, and that it ought to come out "about May." So now I have read the review through again to see where it is wrong from your point of view, and have changed the sentence on p. 4, line 11, so that it reads: "Science does progress, more certainly and more obviously than do art, political theory, and philosophy." It is my complacent impression that this is the only misstatement in my review that is so far above your response threshold that you would undertake to comment upon it. I wait now with anxiety to see whether the lesser evils on which you do not comment now seem important because they have become maximal.

The University of Chicago Press has done itself proud in graciousness. Both Shugg (Director?) and Mrs. Carroll (Promotions) have written me enthusiastically about my comment on your book. I have now sent Mrs. Carroll a copy of the review with this amended sentence in it, but told her that she ought not to quote it until the review is printed unless she has permission from Sanford. Obviously if she liked a sentence or two better than what was in the other thing, Sanford would not object, but it ought not to be possible to read my review in any substantial degree before CP publishes it.

Merry Christmas!

Yours,

EGB

Edwin G. Boring

Dr. Thomas S. Kuhn
Sources for History of Quantum Physics
Sekretariat of Professor Niels Bohr
Gl. Carlsbergvej 15
Copenhagen-Valby, Denmark

Edwin G. Boring
Memorial Hall, Harvard University
Cambridge 38, Mass., U.S.A.



Dr. Thomas S. Kuhn
Sources for History of Quantum Physics
Sekretariat of Professor Niels Bohr
Gl. Carlsbergvej 15
Copenhagen-Valby, DENMARK

FIRST FOLD

SECOND FOLD

AIR LETTER • AÉROGRAMME • PAR AVION

HARVARD UNIVERSITY
PSYCHOLOGICAL LABORATORIES

Memorial Hall
Cambridge 38, Massachusetts

7 December 1962

Dear Tom Kuhn:

What a good time we are having, or perhaps not, for by now you will have received my review of your book and may have become quite discouraged with my education. It is much harder to train the old than the young, and goodness knows the young are hard enough. This is what Max Planck had to say about the acceptance of new theories.

But to your points. There are three of them.

(1) Your substantive reservation about determinism (your p. 2, top). What you said troubled me until I got to your third line from the end: "I should myself still leave some small but appreciable role for chance or for individual genius." Oh my goodness! Nor are those two things the same, unless one straps himself into the Laplacian straightjacket, which I take it makes the concept of possibility nonsense. So let me separate them. (a) There is just all sorts of chance for individual difference in genius. My paper says so, but then turns away from it to discuss the artificial build-up of prestige in the eponym. See lots of places, but especially p. 22 beginning at the top. "Are there geniuses or are there not?...Here the answer is most certainly affirmative. Indeed there are geniuses who affect scientific progress decisively, especially when they are individually concerned with changes in important paradigms of science." In other words, I am admitting intracranial determinism and individual differences in respect of it. So of course things would have gone differently if Newton had not lived, or if Bohr had not lived, even though the inevitability of history might have realized itself quite similarly as it does in discoveries as to how to advance the bomb. It is quite possible that Fermat really did have the elegant solution to that theorem which he did not write in the margin of the book, and nobody has been able to get it since. That did not rule out determinism. There was something in the past that made it possible for Fermat to achieve this end and that has not happened again since. That is to say, there was that something if you work under that paradigm. (b) But then you say there is room for chance. This is what makes me say "Oh dear!" For, if you are working under the paradigm of determinism, then chance becomes a nonsense-word, the equivalent of ignorance. Do you know about Boole and "the principle of the equal distribution of ignorance," his cute phrase for what has since been called the law of insufficient reason? I have left plenty of room for the physicists to deal with chance and have not tried to answer that question, but I did deal with determinism in order to rule chance out, and so, you see, I am not going to change this fundamental business that runs through the paper. I am just one of those opponents of the wave of the future who has to die so that scientific psychology can advance--or are there some new paradigms that never win out but start life and die in infancy simply because they don't work well?

(2) Your personal reservation about the use of paradigm. Well, I thought I had it right, but I see that you object and it is hard for me to pin you down by going through your book. So I have substituted for lines 4-7 on p. 8 the following:

> ...and to adjust the model for closer fits. The model has no actual existence but it may be as concrete as the model of a molecule or the mathematical formula for the learning curve. Kuhn prefers to use the word paradigm, a formal model like amo, amas, amat, which is part of the paradigm for the first Latin conjugation. The paradigm is, however, more complex than the model, less concrete, and, as Kuhn thinks of it, less fully conscious, being in large measure carried in the stream of the Zeitgeist, the current of credence, which is so often not recognized by him who shapes his thinking in respect of it. The business of science revolves about its paradigms, and,...

Of course I offer this a little diffidently; having failed once, I may again not have succeeded. But I have certainly tried to use teacher's help.

(3) Then your objection about whether art and the humanities progress as does science. My first thought here is that you don't remember what you yourself have written, so I recommend that you look at your own page 159 to find the following:

> "If this description has at all caught the essential structure of a science's continuing evolution, it will simultaneously have posed a special problem: Why should the enterprise sketched above move steadily as, say, art, ~~clinical~~ critical theory, or philosophy does not? Why is progress a perquisite reserved almost exclusively for the activities we call science?"

That seems to me to commit you, except for the saving "almost." Perhaps I made the mistake of taking the isolated sentence instead of the full discussion, of choosing two sentences instead of all of Chapter 13. Still, it would be better for me to say you think what you say you think than to stick arbitrarily on a sentence that you made more extreme than you really intended. So on the last two lines of p. 19 and the first two of p. 20, I have changed to:

> ...future. It is the sciences that most clearly progress, so much so that Kuhn wonders whether the statement might not be inverted so as to define science as what progresses. Doubtless art and the humanities, which do not stand still, must have some complementary kind of change or of development even, but progress is no sine qua non for them. Kuhn fails to perceive a goal for science. It...

I won't do anything about trying to make my review more truthful, since that has not even been accepted and may never be. This long paper has got through my three critics who I thought would tear it to pieces most and who have required only minor changes. You are my fourth and last. Not expecting to hear so promptly from you, I started to have the paper fair-copied, so I could move it along in this process which leads to what is called publication. But I am delighted to have your comment and ever so grateful to you.

I have just looked up Voltaire on Newton and the apple. It was Newton's niece who told Voltaire, and it was a "fruit," not an apple. Apple seems to be the genotype for fruit. Cf. Eden.

The publicity man at the University of Chicago Press has just written me with perfect thanks for what I sent him about your book. So that's all right. Should I send him the review if it is accepted, or shall we let it rest where it is?

Edith Annin was enormously pleased to have you and your wife remember her. She told me a little about you, but I see now that it was less than she knew. So many fortunate things can happen to a man in his eighth decade, and I am just having the time of my life in this period. Edith Annin is one of them, for I have never had anyone so efficient and helpful and wise. I keep discovering new abilities in her that I did not know existed. Why she is content in so unimportant a job is a question I never raise. When the eighth decade comes to men as it comes to me, the thing to do is to accept it and say Hallelujah.

Yours,

EGB

Edwin G. Boring

Dr. Thomas S. Kuhn
Sources for History of Quantum Physics
Sekretariat of Professor Niels Bohr
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Copenhagen-Valby, Denmark

P.S. from Edith: Greetings to Tom and Cass Kuhn! When EGB first expressed his enthusiasm about Tom's article in Science, I mentioned that I had known you in Cambridge during graduate-student days, and that Cass and her sister had been contemporaries of my sister Sue's at Vassar. Do you remember, Cass, that you and two friends met Sue and my father one rainy night at Tanglewood and came back to our house to spend the night? I think it was you who sent us our priceless recording of Florence Foster Jenkins, apropos of a hilarious breakfast the next morning. Sue does editorial work at the Austen Riggs Center, a sanatarium in Stockbridge, Mass., but expects to go to New York sometime next summer to work for some psychiatric people there. She has been much interested to have news of you, and I know she would like to send greetings also.

Edith

P.S. from EGB:-It's so hard to get closure. There are two things I left out.

(a) I think you wish to put the paradigm further down into the unconscious than the model, but I just do not believe that the line between the conscious and the unconscious can be drawn in respect of the stream of the Zeitgeist = the current of credence. There are conclusions we can not accept and do not know why because they are reduced to habits of thought, that is to say, based on unconscious premises. But there are the cases where the premise is right up on top, like the four students of Johannes Müller who swore the oath in the 1840s against vitalism. See the red-marked paragraph on the enclosed sheet torn from my 1955 paper on the Zeitgeist.

(b) As to chance's being ignorance, I wrote on this matter at length in 1921 and again in 1942, and have been wondering whether this is a ~~21-~~year cycle with something due in 1963, but I shall not burden you with papers. I do send along my brief discussion of Handlin on chance, but it edges over a little too far toward your view that chance is a sensible concept to use in the contexts of history. It is useful in common sense but a cloak for ignorance, and it seems to me unscientific in the context of the history of science.

EGB.

the influence of materialism, the animal spirits came to be regarded as a *vis viva* and presently a *vis nervosa.* Next Johannes Müller, seeing that every sensory nerve always produces its own quality, substituted for the *vis nervosa* five specific nerve energies, using the word *energy,* in the days before the theory of conservation of energy, as equivalent to *force* or *vis.* He said that the mind, being locked away in the skull, cannot perceive the objects themselves, or their images, but only the states of the nerves that the objects affect, and he fought a battle against the Empedoclean theory—as indeed had John Locke and Thomas Young and Charles Bell before him, and as still others were to do after him. After a while it was seen, however, that the specificity of the five kinds of nerves lies not in the peculiar energies that they conduct to the brain but in where they terminate in the brain. Thus there arose the concept of sensory centers in the cerebrum. Nowadays we see that a cerebral center is nothing more than a place where connections are made and that sensory quality must be understood in terms of the discriminatory response in which stimulation eventuates—or at least many of us see this fact while we fight a *Zeitgeist* that still supports the theory of centers (*9*).

Is there any reason why Galen in A.D. 180 or Albrecht von Haller in 1766 should not have invented the modern theory of sensory quality? None, except that most of the supporting evidence was lacking and, being contrary to the accepted notions of the time, it would have sounded silly. Yet each contributor to this strand of scientific maturation was original, and several contributors had to fight again the battle against the notion that the mind perceives an object by embracing it, or, if it cannot get at the object itself, by getting itself impressed by the object's eidolon or simulacrum. Nor has the *Zeitgeist* even yet been thoroughly disciplined in this affair, as you can tell whenever you hear the remark: "If the lens of the eye inverts the image of the external world on the retina, why do we not see upside down?"

The *Zeitgeist* was hindering progress in this piece of history. It made originality difficult and it made it necessary to repeat the same arguments in 1690 (John Locke) and in 1826 (Johannes Müller) and, if one may believe current advertisements of a scientific film, nowadays too. Yet let us remember that this *Zeitgeist* also helped progress. The continuity of development lay always within the *Zeitgeist.* It was a conservative force that demanded that originality remain responsible, that it be grounded on evidence and available knowledge. Had Galen espoused a connectionist's view of sensory quality in the second century, he would have been irresponsibly original, a second-century crank, disloyal to the truth as it existed then. Loyalty may be prejudice and sometimes it may be wrong, but it is nevertheless the stuff of which responsible continuous effort is made. Science needs responsibility as well as freedom, and the *Zeitgeist* supports the one virtue even though it may impede the other.

4) What may be said of the big *Zeitgeist* may also be said of the little *Zeitgeister* of schools and of the leaders of schools and of the egoist who has no following. They have their inflexible attitudes and beliefs, their loyalties that are prejudices, and their prejudices that are loyalties. Every scientific in-group with strong faith in a theory or a method is a microcosm, mirroring the macrocosm which is the larger world of science.

Take egoism. Is it bad? It accounts for a large part of the drive that produces research, for the dogged persistence that is so often the necessary condition of scientific success. So egoism yields truth. It accounts also for the hyperbole and exaggeration of the investigating enthusiast, and then it may yield untruth. When two incompatible egoisms come together, they account for the wasted time of scientific warfare, for the dethronement of reason by rationalization. Egoism is both good and bad.

Take loyalty. Think how it cements a group together and promotes hard work. Yet such in-groups tend to shut themselves off from other out-groups, to build up their special vocabularies, and so, while strengthening their own drives, to lessen communication with the outside, the communication that advances science. Loyalty is both good and bad, and with loyalty a person sometimes has to choose whether he will eat his cake or will keep it.

This dilemma posed by the little *Zeitgeister* of the in-groups and the scientific evangelists has its root in basic psychological law. Attention to this is inevitably inattention to that. Enthusiasm is the friend of action but the enemy of wisdom. Science needs to be both concentrated and diffuse, both narrow and broad, both thorough and inclusive. The individual investigator solves this problem as best he can, each according to his own values, as to when to sell breadth in order to purchase depth and when to reverse the transaction. He, the individual, has limited funds and he has to sell in order to buy, and he may never know whether he made the best investment. But posterity will know, at least better than he, provided that it troubles to assess the matter at all, for posterity, having only to understand without hard labor, can assess the

effect of prejudice and loyalty and enthusiasm, of tolerance and intolerance, as no man ever can in himself.

Coda

This is a broad meaning for the word *Zeitgeist* —the total body of knowledge and opinion available at any time to a person living within a given culture. There is, certainly, no rigorous way of distinguishing between what is explicit to a scientist and what is implicit in the forms and patterns of communication, between what is clear conclusion and what is uncritically accepted premise. Available knowledge is communicated whenever it becomes effective, and this is the *Zeitgeist* working.

The *Zeitgeist* is a term from the language of dualism, while its definition is formally physicalistic. That paradox is for the sake of convenience in the present communication and is allowable because every statement can be transformed into physicalisitc language when necessary. Dualism has the disadvantage of implying a mystery, the existence of a *Zeitgeist* as a vague supersoul pervading and controlling the immortal body of society. We need no such nonsense, even though this abstinence from mystery reduce us to so ordinary a concept as a *Zeitgeist* inclusive of all available knowledge that affects a thinker's thinking.

That such a *Zeitgeist* sometimes helps progress and sometimes hinders it should be clear by now. As a matter of fact, the distinction between help and hindrance can never be absolute but remains relative to some specific goal. The *Zeitgeist* hindered Copernicus, who, resisiting it, helped scientific thought onward and presently changed the *Zeitgeist* on this matter to what it was in Newton's day. Did the *Zeitgeist* that Newton knew help relativity theory? No; relativity had to make its way against that *Zeitgeist*. The newest *Zeigeist*, which will include the principles of relativity and uncertainty and complementarity, presumably exists today within the in-group of theoretical physicists. It will become general eventually, and then it will reinforce progress, and after that, much later, perhaps our posterity will find today's truth tomorrow's error. The one sure thing is that science needs all the communication it can get. The harm communication does to progress never nearly equals the good.

References and Notes

1. The term *Zeitgeist* seems to have originated in this sense in 1827 with Goethe who, in discussing the way in which Homer had influenced thought, remarked in the last sentence of his essay, *Homer noch einmal,* "Und dies geschieht denn auch im Zeitgeiste, nicht verabredet noch überliefert, sondern *proprio motu,* der sich mehrfältig unter verschiedenen Himmelsstrichen hervortut." *Himmelsstrichen* can be translated "climates," thus justifying the figure of the text, but it must also be noted that Goethe meant to use the term *Zeitgeist* when the effect is "self-determined," brought about "neither by agreement nor fiat." See, for instance, *Goethes sämtliche Werke,* (I. J. Cotta, Berlin, 1902–07), vol. 38, p. 78.
2. The discussion of this paragraph and all the references will be found *in extenso* in E. G. Boring, "Great men and scientific progress," *Proc. Am. Phil. Soc.* **94**, 339 (1950). The reference to Tolstoy is, of course, to his *War and Peace*. For the longest list of nearly simultaneous inventions and discoveries, see W. F. Ogburn and D. Thomas, "Are inventions inevitable?" *Polit. Sci. Quart.* **37**, 83 (1922).
3. On the social criterion of truth, on scientific truth's being what scientists agree about, see S. S. Stevens, "The operational basis of psychology," *Am. J. Psychol.* **47**, 323 (1935), especially p. 327; "The operational definition of psychological concepts," *Psychol. Rev.* **42**, 517 (1935), especially p. 517; E. G. Boring, "The validation of scientific belief," *Proc. Am. Phil. Soc.* **96**, 535 (1952), especially pp. 537 f.
4. On the point that a self cannot observe itself, that in self-observation a person must regard himself as if he were another person, see M. Meyer, *Psychology of the Other One* (Missouri Book Co., Columbia, Mo., 1921); Stevens, *op. cit.*, especially pp. 328 f.; E. G. Boring, "A history of introspection," *Psychol. Bull.* **50**, 169 (1953), especially p. 183.
5. On the sincerity of cranks in science, see I. B. Cohen, J. L. Kennedy, C. Payne-Gaposchkin, T. M. Riddick, and E. G. Boring, "Some unorthodoxies of modern science," *Proc. Am. Phil. Soc.* **96**, 505 (1952).
6. On Newton's difficulty in changing the *Zeitgeist* with respect to the complexity of white, see E. G. Boring, *Sensation and Perception in the History of Experimental Psychology* (Appleton-Century, New York, 1942), pp. 101 f. This discovery of Newton's was exceptional in that it had no anticipations (unless my wisdom is at fault). In other words, the *Zeitgeist* was strongly fixed, and to break it Newton must have been very stubborn—as indeed other evidence indicates that he was.
7. On Helmholtz' trouble with the *Zeitgeist* with respect to the velocity of the nervous impulse, see E. G. Boring, *A History of Experimental Psychology* 2 ed. (Appleton-Century-Crofts, New York, 1950), pp. 41 f., 47 f.
8. On Priestley and Lavosier and the *Zeitgeist's* support of the phlogiston theory, see J. B. Conant, *The Overthrow of the Phlogiston Theory,* Harvard Case Histories in Experimental Science, Case 2 (Harvard Univ. Press, Cambridge, Mass., 1950).
9. On the history of the physiological theories of sensory quality and the retardation of progress in thinking by successive phases of this *Zeitgeist,* see E. G. Boring, *Sensation and Perception* (*op. cit.*), pp. 68–83, 93–95.

High Art and Kitsch

Bernard Rosenberg and ***David Manning White*** (Eds.)

Mass Culture: The Popular Arts in America. Glencoe, Ill.: Free Press, 1957. Pp. x + 561. $6.50.

Reviewed by FRANKLIN FEARING

who is Professor of Psychology in the University of California at Los Angeles and a social psychologist, or perhaps more accurately a cultural psychologist. His chief interest just now is communication and he has been affected by Benjamin Whorf. His researches on movies and comic strips entitled him to speak with some competence about Kitsch, and, if you do not know the meaning of this new immigrant to the English language, read the review.

CULTURE, it seems, is either "high" or "mass." Without defending this neat dichotomy we may note its rather remarkable consequences. It classifies and characterizes the content ("art" and the pseudo-art that is produced for mass consumption and called "Kitsch" by the Germans), the two kinds of consumers (the "elite" and the "masses"), and the two kinds of producers (the dedicated artist and the producers of Kitsch). Its most impressive consequence, however, is the intense conflict in attitude and opinion engendered in nearly everybody, and especially in those who do not themselves (with certain exceptions) produce art either "high" or "popular," but are professionally concerned with its critical evaluation. It may be, in fact, that the dichotomy itself is but an expression of basically different attitudes toward life and people, a difference not confined to those who are professionally articulate on the subject.

The larger proportion of the essays in the present volume are contributed by these professional commentators. The names of some of the better-known members of this group—Alexis De Tocqueville, José Ortega y Gasset, Gilbert Seldes, Edmund Wilson, George Orwell, David Reisman, Walt Whitman—indicate the great diversity in background and point of view. In spite of these differences, these critics firmly, even passionately, subscribe to one or the other of two positions.

I. Mass culture—popular music, best sellers, mass circulation magazines (the "slicks" as well as the "pulps"), movies, radio, television, the comics—is debased and decadent. It is "manufactured" for persons who either were never capable of appreciating high (real) culture, or are so rapidly becoming dehumanized, standardized, and vulgarized that they prefer Mickey Spillane to Shakespeare, Tin Pan Alley music to Mozart, a Rockwell Kent cover on the *Saturday Evening Post* to Braque. Kitsch is manufactured mostly by hacks for the mass market by formula and, as it spreads, destroys high culture. At this point there is some divergence of opinion as to what, if anything, should be done. There are those who view mass culture as hopeless and the masses with contempt, and those who would inaugurate some form of censorship in order to protect the masses (especially children) from contamination.

II. In an era of extended leisure and highly developed communication-technologies a cultural revolution has occurred, and it is good. It has become possible for more people than ever before in history to understand their world and to have contact with every form of high art. Impressive statistics can be quoted. Twenty million recordings of Toscanini and 60 million records of Mozart's music have been sold. The National Broadcasting Company spent $500,000 to present a three-hour long TV premiere of *Richard III* watched by 50 million persons. Enormous numbers of low-priced reprints of the works of such writers as Plato, Cassirer, Stendhal, William James, and such contemporaries as Pearl Buck, John Steinbeck, and Albert Schweitzer, are sold yearly. Admittedly there is much that is banal and vulgar in popular culture; nevertheless the picture as a whole affords no basis for alarm about the state of popular taste.

Between these two positions, of which the foregoing are much over-simplified statements, there seems to be no middle ground. The editors themselves are sharply divided and each has stated his case in the two essays which introduce the volume.

THERE remains, of course, a third group concerned with mass culture. These are the social scientists, well represented by such names as Bernard Berelson, Leo Lowenthal, Arthur Broderick, Patricia Salter, Hortense Powdermaker, Paul Lazarsfeld, Robert Merton, and T. V. Adorno. Late-comers to the fray, and wrapping themselves firmly in their mantles of objectivity, they examine the content of specific examples of mass culture as partial reflections of existing values, beliefs, and stereotypes. Or, using empirical methods, they endeavor to discover the nature and extent of the social impact of the great mass media on our society. Although the tools of their analysis have varying degrees of precision, and in spite of the limitations (penetratingly discussed by Leo Lowenthal) which the assumptions underlying this type of study impose upon interpretation, these researches have yielded important insights which could be had in no other way.

It is almost certain that every reader of the forty-nine selections in this book—twelve of them are published for the first time—will be deeply annoyed by some and warmly approving of others, depending, we may suppose, on whether he is Viewer-with-Alarm or a Pointer-with-Pride, or perhaps—and this is pure speculation—on whether, in terms made familiar by the *Authoritarian Personality*, he is a High Scorer or a Low Scorer. This reviewer is no exception. He found, for example, the pieces by Seldes and David Manning White hopeful and acceptable, and the snobbery and arrogance of Edmund Wilson detestable.

Two questions: Why didn't the editors provide a brief biographical note for each of the contributors, and why, oh why, did they not give us an index?

The writer of art has in mind the psychology of his characters; the writer of trash, the psychology of his readers.

—SHLOMO BICKEL

CP SPEAKS . . .

ENGLISH, German, and French. Not to mention Russian. *CP* is still worried about the language barriers and whether American psychologists accept too readily an isolationism which has become possible because the magnitude of the current American psychological endeavor permits complacency. (See *CP*, 1956, **1,** 331f.; 1957, **2,** 119f., 105.) Now, however, C. M. Louttit has given us some data which compare Anglo-American, German, and French psychologists, physicists, and chemists with one another. (*Amer. J. Psychol.*, 1957, **70,** 314–316; cf. 1955, **68,** 484–486.) See the table on this page, which gives Louttit's data with the decimals rounded off and the figures adjusted after omitting the category "Other." You read the table this way: 26% of the references cited in articles written in the French language were to books or articles in the English language.

The measures of provincialism for the psychologists are 94% English references in the English articles, 91% German references in the German articles, and 66% French references in the French articles. The French psychologists read and use English much more than the Anglo-Americans or the Germans use a foreign language. The French physicists and chemists refer even more often to English articles than to French, and the French chemists almost as often to German articles.

So far as these data go, the psychologists appear more ethnocentric than the physicists, and the physicists than the chemists. This is the sort of thing that Louttit was always doing for his profession. He is going to be missed a lot.

So what? Nothing. *CP* has no grandiose ambition to alter the culture. W. F. Hill wants psychologists to be sure, when they study languages, that they are not neglecting more important things, like statistics and electronics, and *CP* is not the evangelist to gainsay him. It knows too much about human prejudice to be enthusiastic about its enthusiasms. But it does like to point out facts, and just now it has got hold of a couple of paperbacks that ought to help those graduate students who still wish (or have) to learn scientific German or French or both. Both of these books are published in 1957 by John Wiley and Sons, and they are:

George E. Condoyannis. *Scientific German: A Concise Description of the Structural Elements of Scientific and Technical German.* Pp. x + 164. $2.50.

William N. Locke. *Scientific French: A Concise Description of the Structural Elements of Scientific and Technical French.* Pp. x + 112. $2.25.

The two authors have taught languages at Massachusetts Institute of Technology for years. Dr. J. G. Beebe-Center, who for thirty years has been Harvard's trilingual psychologist, likes these books. The books aim to teach the students to read, not to speak or even to translate the language. They are superbly simple, he says. He quotes Condoyannis as an example: "There are three large divisions of structure [in German]: The noun-adjective system, the verb system, and the word order." That, says Beebe-Center, has the brevity and clarity of Caesar's comment on Gaul.

And don't, says Beebe-Center, forget to notice the quality and interest of the contemporaneous excerpts that are used as exercises. "No old chestnuts. Little second-hand science. Excerpts from Einstein and Otto Warburg. Hardly could the student help feeling that, if the books open writings like these to him, they will open to him the other writings he needs in his scientific work." At any rate, if the linguistic frustration of young scientists is one of your worries, take a look at these two books and maybe you will want to ask John Wiley to make the duo into a trio by adding a Russian complement.

To those who are interested in the psychology of the history of science, culture, and thought, Oscar Handlin's *Chance or Destiny: Turning Points in American History* (Little, Brown, 1955, $3.75) provides the occasion to reread the similar discussion in Tolstoy's *War and Peace* (1869), for the two discussions, if you take the trouble to read them both on the same afternoon, are surprisingly alike. It is true that Tolstoy makes more of the inscrutable forces of History that enslave uncomprehending kings and that promote the trivial royal intentions into the service of History's indiscernible designs, but Handlin too recognizes the existence of great trends like industrialization and democratization, trends that advance themselves—perhaps by some kind of positive feedback—and are insusceptible to influence by the individual person.

The main undertaking of Handlin's book is to show that important crucial events in history are often determined by chance, by unpredictable events that are apt to seem trivial in themselves. Cornwallis surrendered because his attempt to withdraw his army from Yorktown was ruined by a sudden storm that wrecked many of his boats and left his army divided across the York River. But for the storm, would the U. S. A. now be a Dominion? Handlin does not

How Provincial Are Anglo-American, German, and French Psychologists, Physicists, and Chemists?

Adapted from C. M. Louttit. *Amer. J. Psychol.*, 1957, **70,** 314–316

Read: 26% of the citations in French articles are to articles and books in English.

	Psychology			Physics			Chemistry		
	Eng.	Germ.	Fr.	Eng.	Germ.	Fr.	Eng.	Germ.	Fr.
English	94	5	1	92	6	2	83	12	5
German	3	91	6	37	61	2	24	69	7
French	26	8	66	52	16	32	39	30	31

Underlying thesis:
Originality = chance = unpredictability = ignorance.

say that, yet the course of American history in 1781 would have been greatly changed but for the storm. The Revolution could have failed. Not all revolutions succeed.

The new gun aboard the Princeton, being displayed in 1844 to President Tyler and his Secretary of State, Upshur, burst on being fired and killed Upshur, who had been appointed because he was thought to have the tact and skill needed to bring Texas into the Union without raising the issue of slavery. So the delicate task fell to the aging and disappointed Calhoun whose acerbity exceeded his tact, and the issue of slavery was raised. Texas presently came in, but it is taking more than the Civil War to settle this issue that is still with the United States.

Lieutenant Commander Schweiger, commanding the German submarine U-20, was returning to his home port on May 7, 1915, discouraged at his lack of successes, when he saw through his periscope a large vessel in British waters which the Germans had proscribed. He let go a torpedo, and, as the vessel went down stern-first, he could just make out the name on the bow: *Lusitania*. And so—is or isn't *so* the right word here?—the United States came into that war.

The United States Navy thought Pearl Harbor impregnable. They were prepared for attack anywhere else in the Pacific. And the Japanese, following out Admiral Tojo's perilous plan on December 7, 1941, were anything but confident. Their slim chance succeeded and the United States then was in another war. What if the majority opinion about Pearl Harbor's being impregnable, an opinion widely held in Japan too, had been right?

Is the case the same for science? What if Newton had not been born?

> "Nature and Nature's law lay
> hid in night:
> God said, Let Newton be! and
> all was light."

Alexander Pope, who made that remark, lived in the century when the Divine Will handled the unpredictable and natural law the predictable. Without Newton there would have been no *Principia* in 1687, but presently there would surely have been gravitation, just as surely as the American physicists knew that the Soviets too would have the atomic bomb, given time.

You can find chance operating in scientific discovery and in the happy insight that leads to an important discovery, and one argument for basic research is founded on the unpredictability of the useful applications of fundamental laws; nevertheless, given motivation, there is a rational orderedness to scientific discovery that introduces into the historical process a predictability that is hindered or sped up by chance events but is not ordinarily completely stalled. This is why we speak of the Zeitgeist when we seek a concept to explain synchronous independent identical discoveries, inventions, and insights. History lords it over the scientist because history—Tolstoy's History, Handlin's Destiny—is the culture, the total accumulation of fact and value available to the individual scholar. By them is he influenced without his knowing how or how much. Because of them his contribution becomes a 'next step,' and often it is in spite of them that the 'next step' is the unpredictable one of creative originality. So is originality necessarily chance? It can be. Remember Fermat's theorem that no one else ever proved, the theorem with the lost proof? But what about an originality's happening twice in the same year? What about Alfred Russell Wallace's and Charles Darwin's formulating the theory of natural selection independently by 1858? Adams' and Leverrier's independent discoveries of the planet Neptune in 1845? Gray's and Bell's invention of the telephone in 1876?

Handlin's book is delightfully written. Read it. It is a good thing to have the case for the operation of chance in history, for unpredictability, made so clearly and irrefutably. The notion of a Zeitgeist as imperative Destiny ought to be scotched. Progress is what happens, usually what has happened, as Handlin says; its direction is never certain or clear until it has entered the past. But after—with Handlin's help—you have got rid of superstitions about the inevitability of history and have fully accepted the fact that unpredictable chance may in important ways intervene in the causal nexus, there still remain the various problems as to how the total mass of available fact and value, too complex ever to be specified, influences and determines human conduct and thought, discovery and originality, governed, it seems at times, by some positive feedback that makes their course appear purposive because it is consistent and not wholly unpredictable.

TURKISH psychology presses ahead on the scientific path after much delay. Carroll C. Pratt and John Volkmann were successively at the University of Ankara and now Spaulding Rogers is there. The University of Istanbul had visiting professors from Germany irregularly—Wilhelm Peters from 1937 to 1952. Now it has for three years Walter R. Miles, emeritus from Yale, and he has sent *CP* volume 1 of the *Istanbul Studies in Experimental Psychology* (Istanbul: Baha Matabaase, 1956, 160 pp.). This is a little volume of which a third is in English and two-thirds in Turkish. Half of the 160 pages are written by Miles (two-fifths of these in Turkish, three-fifths in English) and the other half by three Turkish authors. It's only a start, but then what do you expect of a baby?

NOW the second of the five volumes of Alexander Grinstein's *Index of Psychoanalytic Writings* (International Universities Press, 1957), Freudenberg to Lampl, items 10715 to 19521, is out. The appendix of this volume has a special bibliography of Ernest Jones. Three more volumes to come.

THE Dryden Press has just reprinted Theodore M. Newcomb's *Personality and Social Change*. *CP* asked Dr. Robert B. Zajonc of the University of Michigan to examine the reprint and tell *CP* what comment it should make on this event. He says that the book is a reprint and not a new edition, and he comments that "the work, which is known as the *Bennington Study*, examines attitude change within the context of a college community and is a classic in social psychology. Since it has been out of print for nearly ten years, its reappearance," he assures us, "should be welcomed." *CP*, knowing Newcomb but not this book, is confident that Zajonc's comment must be correct.

—E.G.B.

To Choose a Mate, Understand Yourself

James A. Peterson

Education for Marriage. New York: Charles Scribner's Sons, 1956. Pp. xxiii + 429. $5.50.

Reviewed by LOUIS KERDMAN

Mr. Kerdman is Assistant Director of the Marriage Counseling Service of Greater New York, the first service of this kind in New York, one that he helped establish. He is also Clinical Case Supervisor of the Out-Patient Clinic of the Veterans Administration in Brooklyn. He is a former Fellowship recipient from the American Association of Marriage Counselors, is now majoring at Columbia in the field of Education for Marriage and family life. In short, he knows a lot about the subject.

THE author, James A. Peterson, is an Associate Professor in the Department of Sociology and Marriage Counselor at the University of Southern California, where he established one of the first programs for marriage counselors on the doctoral level in any American University and one of the first successful marriage counseling clinics in California. He is a past Vice-president of the Marriage Counseling section of the National Council on Family Life.

One of New York's leading newspapers recently carried an article describing how Univac, the electronic brain used for solving complex business problems, was employed as a stunt by a television program to bring together two people on the basis of the similarities of their dislikes and desires. Out of several thousand cards, a twenty-three-year-old receptionist and a twenty-eight-year-old advertising executive were selected as the ideally suited couple. When they met for the first time, both immediately acknowledged that they were 'truly' suited to each other. They became engaged and announced plans to get married. About six months later, the engagement was off! What went wrong?

According to the girl, her ideal prospective mate lived up to her expectations as long as the "television people were paying for it." He was kind and sweet; he took her to nice places to dine and dance. Once the excitement was over, he did not take her out so often. When he did, it was to an inexpensive movie. He bought a house in which they were to live, but his parents announced they were going to move in and live with the couple.

Much can be gained from the conclusions this young girl arrived at as a result of her experience. She is quoted as saying: "It takes time to find out about the other fellow. I guess you might say it is the human element."

It is precisely this 'human element' with which Dr. Peterson is so vitally concerned and which he so expertly discusses in his book. The human complexities inherent both in the selection of a mate and in the adjustment process following marriage are more than fully appreciated by the author, who records, from a wealth of data, the results of his elaborate and detailed research.

While the purpose of the author's effort is to provide a text for students, one that will help them achieve the personal adjustment and the broad insight necessary for a permanent and happy marriage, the discussion can very well serve another function: It may provide a few sobering thoughts for those whose inclination it is to experiment with the happiness of others by using mechanical devices for mating, and for those whose loneliness drives them to become subjects of such experimentation.

These individuals will discover in *Education for Marriage* that there is a host of factors—psychological and sociological—which demand recognition. Keenly conscious of these forces and sensitive to their implications in interpersonal relationships, the author has documented his array of data in a systematic and comprehensive manner. He discusses, although not in equal detail, every facet of human interaction inherent in the preparation for marriage and in the achievement of togetherness in the marital relationship.

While drawing from his own rich experience, Dr. Peterson tends to lean heavily on the research done by such sociologists and psychologists as Burgess, Wallin, Terman, Bowman, Harvey J. Locke, and Landis. Consequently, his emphasis seems to be more upon the necessity to relate to social 'norms' than upon the psychological factors influencing individual behavior. In this respect the volume follows the established pattern used by so many other authors on the subject. What is lacking in the discussion is consideration of the kind of dynamic interaction between partners so warmly and perceptively described and analyzed in the *Happy Family* by Levy and Munro. There is, however, more than speculative justification in the author's approach to his subject. Since his primary audience are students and since it is his hope that they will use his material for developing a self-awareness, some basis for comparing and contrasting is required.

WITH this in mind, Dr. Peterson introduces at the end of each chapter an exercise in "self-analysis." It is his hope that, "if the reader will be faithful in relating these conditioning factors [such as recreational, religious, socioeconomic attitudes] to himself, he may develop an awareness of those aspects of his personality which will be hindrances or assets in adjusting to marriage." This development of insight seems to me to be the underlying theme of the book. Many clinicians may question the actual benefits an individual can derive from such self-analysis. One argument proposed by Dr. Peterson is that the exercises in self-analysis are primarily intellectual. This approach frequently becomes the modus operandi against overcoming one's resistances. More important it is, however, that self-

HARVARD UNIVERSITY
PSYCHOLOGICAL LABORATORIES

Memorial Hall
Cambridge 38, Massachusetts

4 December 1962

Dear Dr. Kuhn:

Well, here is an unsolicited review of your book which I am sending to Fillmore H. Sanford, Mezes Hall, University of Texas, Austin 12, Texas, as the present Editor of Contemporary Psychology. Unsolicited reviews are in a peculiar status because most of the reviews are solicited, and he might find various reasons for not accepting this. One would be that it is too long, that I discuss general issues too much. But I am trying it out on him, and, if he decides to publish it, then I shall tell you that so that you will know, and I shall send another copy of it then to the University of Chicago Press, in case they should find advertising matter in it that was more suitable to them for use than the paragraph that I sent them.

Very much do I hope that you will not find this review too great a distortion of the reality which you wrote.

Yours,

Edwin G. Boring

Edwin G. Boring

Dr. Thomas S. Kuhn, Director
Sources for History of Quantum Physics
Sekretariat of Professor Niels Bohr
Gl. Carlsbergvej 15
Copenhagen-Valby, Denmark

The University of Chicago Press
5750 ELLIS AVENUE · CHICAGO 37 · ILLINOIS

December 4, 1962

Dear Professor Boring:

On behalf of this Press I want to thank you for your letter of November 26 commenting on Thomas S. Kuhn's latest book, THE STRUCTURE OF SCIENTIFIC REVOLUTIONS. The highest opinion of this book that you have given us will go a long way in winning readers for Mr. Kuhn's work. We are especially grateful to you for permitting us to quote your comments.

Yours sincerely,

Roger W. Shugg

Professor Edwin G. Boring
Harvard University
Psychological Laboratories
Memorial Hall
Cambridge 38, Massachusetts

cc: Mrs. Sandra Carroll

h

bcc: Mr. Thomas S. Kuhn

Rec'd

HARVARD UNIVERSITY

PSYCHOLOGICAL LABORATORIES

Memorial Hall
Cambridge 38, Massachusetts

26 November 1962

University of Chicago Press
Publicity Department
5750 Ellis Avenue
Chicago 37, Illinois

Gentlemen:

I have just read with great enthusiasm T. S. Kuhn's Structure of Scientific Revolutions, and wrote him in Copenhagen my enthusiasm for what he has done. He asked me whether I would not send you parts of one of my paragraphs of praise ~~to him~~, in order that you might use it in publicity if you cared to do so. Well, here is a paragraph which you may use in whole or in part if you wish to have it. I do hope you will promote the book, because its success could do great good in the thinking of the history of science. Here follows the permissible quote:

> "Dr. Kuhn shows how a science in any period is conducted under certain basic presumptions which he calls a paradigm. A scientific revolution is a change in a fundamental paradigm, a radical change of view from one paradigm to another without intermediate steps (it's like a perception which shifts discretely from one meaning to another), yet a change that occurs slowly and usually against conservative resistance. Well, this brilliant book undertakes to change the paradigm about paradigms. It is exceptionally wise and speaks with clarity and charm, deserving to have an enormous effect. Will it establish a revolution in the way in which the history of science is viewed? Posterity will know about that. Wait around and see."

Yours truly,

Edwin G. Boring

Edwin G. Boring
Edgar Pierce Professor
of Psychology Emeritus

TSK
EGB.

Thomas S. Kuhn

The Structure of Scientific Revolutions. Chicago: University of Chicago Press, 1962. Pp. xv + 172. $4.00. (Also: International Encyclopedia of Unified Science, vol. II, no. 2, University of Chicago Press.)

Reviewed by Edwin G. Boring

The author, Thomas S. Kuhn, a bright young man of forty, took his bachelor's degree summa cum laude at Harvard in 1943, spent four years in graduate study at Harvard, years during which he shifted his interest from physics to the history of science, and then with an early PhD under his belt was chosen to join for three more years the free intellectual community which is the Harvard Society of Fellows. He went to the University of California, Berkeley, in 1956, expanded presently under a Guggenheim Fellowship, and then met the social scientists at the Center for Advanced Study in the Behavioral Sciences. He is now Professor of the History of Sciences at Berkeley.

[There are more data in Amer. Men Sci., Social & Behavioral Sciences, 10th ed., 1962, p. 606.]

The reviewer, Edwin G. Boring, was born in the year Mach published the Analyse der Empfindungen, crept in under Titchener's wing at Cornell one month after William James died and so also a month after Wertheimer got off the train at Frankfurt to found Gestalt Psychology, was granted a PhD one day before he married but a whole year after Watson delivered behaviorism to a waiting world, went to Harvard just after the Psychologische Forschung got going, and thinks he founded CP in the centennial year of Freud's birth.

[If used at all, too long. Cut.]

Science used to have resilient theories that persisted for long years defying contradictory facts. Does not every academic psychologist now over fifty years old remember teaching Sophomores the Hering theory of color vision in which the complementary red and green were bluish and not the primary hues, a theory which Christine Ladd-Franklin tried to fix up with another theory that would not explain other anomalies? It was of such theories that B. F. Skinner quipped that C.N.S. must mean the Conceptual Nervous System, fashioned in the imagination of psychologists rather than from the observations of neurologists. Yet these theories proved amazingly viable. Max Planck remarked that often a theory stands up against contradicting facts until its authoritative author dies. J. B. Conant has said that a theory withstands contradiction until a better theory arrives to replace it. That phenomenon is the science's horror vacui in respect of theories. J. J. Thompson commented away back in 1907 that a scientific theory is really a policy and not a statement of truth after all. Theories persist as long as they are usable.

In the last couple of decades the model has been brought out to replace the theory, which has fallen into disrepute because it has claimed to state truth without assimilating its contradictions. Truth is all-or-none. There are would-be truths but no half-truths. The model does not claim truth-value. It is an aspiration for a generalization. It may be employed for a limited universe. You see how well you can get your data to fit, perhaps adjusting the model to make the fit better. If the fit is good, you have a good summary of these data and then you may use the model to predict other data and test it empirically. If the prediction is borne out, the model gains in dignity and importance.

Now here comes T. S. Kuhn with his paradigms. A paradigm is a model for the way scientific thinking should go, the presumption implicit in investigation, taken for granted when it is being used, carried most of the time unconsciously in the stream of the Zeitgeist. A scientific revolution is a change of such a paradigm, and the more fundamental and inclusive the paradigm, the bigger the revolution. Actually paradigms are of all sizes, but the big ones are the best known and thus the most instructive.

It was a change of paradigm from Ptolemy to Copernicus. The earth is the immobile center of everything. The earth is relatively small in an enormous universe and is going like Jehu. There was a paradigmatic change from Aristotle to Galileo. The pendulum is a body falling under complicated restraints. It is a freely swinging weight with its timing independent of its amplitude. The change with Newton was from the interaction of small particles of matter to universal mutual attraction acting at a distance without contact. Attraction joins size, shape, position and motion as a primary characteristic of matter. Darwin officiated at the change from the goal-directed universe to a merely caused world. Organisms no longer changed in accordance with a Divine plan but merely as the inevitable outcome of conflict.

And then -- leaving Kuhn behind since he does not deal with psychology in this book -- we may note the change of paradigm from Cartesian dualism to modern behavioristics, a change not yet fully accomplished, and the

change from a Wundtian morphology of consciousness to the field dynamics of Gestalt psychology, a change which this reviewer thinks is fully accomplished -- for everyone takes field theory as a matter of course today. These revolutions were small and altered no world view. Psychology has had as yet no big revolution, and perhaps that is why it seems to have had no Great Men. Freud may yet turn out to have shifted the largest paradigm, but your reviewer leaves its specification to posterity -- precious, eagle-eyed, unmyopic posterity to whom so much of present confusion will become scrutable.

A revolutionary change is both abrupt and slow, that is to say, it is of an all-or-none character but may require many years before it gains general acceptance. Kuhn uses Jastrow's duck-rabbit ambiguous figure from Fliegende Blätter as his illustration. You see the duck or the rabbit, never something in between, never a rabbuck. So with the paradigms. You fluctuate between them, now the old, now the new, and finally in the mind of the Great Man who becomes the Eponym of this change -- Copernicus, Newton, Darwin -- the new prevails. The slowness results from the fact that the change is continuous although all-or-none, for it has been conceived before the Great Man contrived to validate it, yet it began as an occasional minority view. It ends up eventually, of course, when the revolution is resolved as the majority view. Toward the end of a revolution the new paradigm has come to be the view of nearly all the experts -- although Einstein could still wonder whether God himself was not able to transcend

complementarity and know both the position and the velocity of the electron at once. Then the paradigm filters down to the public who almost universally had accepted the conservation of energy and repudiated perpetual motion before Einstein changed the world by writing $E = mc^2$. Kuhn is wise in choosing perception for his paradigm of paradigms: in perception the view of the world shifts suddenly and completely, for meanings are discrete. "Why, how did that camel ever get over there in that field? Oh, no; it isn't a camel after all; it's a pile of stones."

How do revolutions get resolved? They get resolved by succeeding. Many little ones die in infancy. But Kuhn has this to say. Science does progress, as art, political theory and philosophy do not. That, indeed, is almost a definition: science is that discipline in which progress is the distinguishing feature. Progress means that all of the past remains embodied in the present. When a fundamental paradigm changes, then the scientists are not only busy fitting what were anomalies under the old paradigm into the new, but they are also to be found industriously rearranging all the old consistent facts within the new setting. The new paradigm does not disintegrate the old observations; actually it gives them new life in a new significance. It is this capacity of science for the new to absorb and revitalize the old that makes a paradigmatic revolution irreversible. A revolution may fail, but there is never an exact counter-revolution. A new revolution may eventually yield to a newer, but finally the past paradigm is found to be gone forever, eviscerated of its facts by the new paradigm that absorbs them.

Kuhn thinks that science has no goal, that its progress aims no whither. Certainly the acquisition of truth is not its goal and Kuhn uses that word truth in his essay only to note its inappropriateness. To this reviewer it seems that Kuhn missed his chance to point out the analogy between the Darwinian biological revolution in paradigm and the Kuhnian one in epistemology. "La science n'est pas; elle devient," said Flourens long ago. So science keeps on becoming, just as organisms do. Organic evolution is controlled by the ecology of living. Scientific evolution is controlled by the economy of thinking. If wisdom -- not knowledge but wisdom -- stretches further and further beyond the range of man's possible apperception, still the paradigms keep it under noetic control. They have to, for man creates the paradigms for this very purpose. Except for paradigmatic restraint, man's irresponsible idiosyncrasies would scatter science to the four winds. Thus the paradigm has that dual role: it conserves the past and yet leaves escape possible for him who is wise and who dares.

It's a brilliant book, this. If this paradigm of paradigms could ever filter down to general public acceptance, how the misapprehension of scientific work would be diminished!

AIRMAIL

Copenhagen, Denmark
November 29, 1962

Professor Edwin G. Boring
Psychological Laboratories
Harvard University
Cambridge 38, Massachusetts

Dear Professor Boring:

You are being extraordinarily kind and helpful, and I am more grateful to you than I know how to say. Your interest and support will certainly gain attention for my book among a group that I had hoped, but not altogether expected, to reach. At this moment, there is nothing that interests me more. My very warmest thanks to you.

I thoroughly agree with all three of your "crazy little complaints," but am no surer than you how far I would have gotten with the Press about them. On the other hand, I have to confess that I did not even try, and I surely should have. Fortunately, however, with respect to your first complaint, I think the situation may not be quite so bad as you suppose. The duck-rabbit figure has by now become almost a cliché in a great many circles, particularly since Wittgenstein discussed it at such length. (That is why, since I did not plan to print an actual figure, I selected the duck-rabbit rather than the far far more striking wife-and-mother-in-law.) Still, you are surely right that it would have helped significantly to scatter a few of the relevant illustrations through the appropriate pages.

I have now read your draft paper with great interest though at a higher speed than it deserves. You have put an important and difficult point with great clarity and wit. Since this problem is one that students repeatedly raise with me, at which point I take a line very much like yours but far less developed, it will be very helpful to have your piece available. I look forward to seeing the final version.

Actually, at least so far as suggestions from me are concerned, the final version may well be the one you have sent me. I have only one substantive and two personal reservations, all of them very minor indeed. You certainly need not pay any attention to them at all.

As to the substantive reservation, I think you may push "determinism" too far or, at least, that your use of the term may lead readers to a conclusion you do not quite intend. Personally, I suppose that crises are engendered by processes that are determined virtually entirely by conditions internal to the sciences and their socio-economic milieu. Also, though it is hard to be sure, I suppose that there are probably always enough men of first-rate talent available to ensure the ultimate resolution of a recognized crisis. In this sense, too, I take it that paradigm-change selects the man rather than vise-versa. On the other hand, the personal stamp of the man who gets selected may also make a real difference in subsequent development. There are cases of "multiples" in which the various simultaneous dicoverers have produced rather different things and in which the end product has depended as much upon the differences as upon the similarities among their various discoveries. One can imagine that subsequent history would be rather different if one or another of them had not lived or had not become involved with the development. I think particularly of the simultaneous invention of matrix mechanics amd of wave mechanics, two radically different approaches to quantum physics. If one or the other had not been discovered (an implausible but not impossible hypothesis) physics might today look quite different. In any case, a more plausible hypothesis, I am prepared to argue that certain aspects of contemporary physics (not, however, any of its formulas) might be quite different if Niels Bohr had never lived. You will, I feel sure, see what I am getting at. I should myself still leave some small but appreciable role for chance or for individual genius. I suspect that you may agree but think your readers will not recognize the point.

As to the personal reservations, they coneern two of the things you have said about my book. Neither is a bit serious, since in general you have represented me better than I could possibly have hoped. On the other hand, I might as well mention them for what they are worth. On page 8 you say, "Since the model has no actual existence, Kuhn prefers to use the word <u>paradigm</u>." I wonder whether you would consider adding a sentence indicating the respects in which a paradigm is both more and less than a model. More, in that it includes a variety of methodological and instrumental commitments as well as commitments to theory or model; less, because so very much of it is implicit -- the question is less whether the model actually exists than whether one could possibly describe what it would look like if it did exist.

My other problem arises at the bottom of page 19. Here, I have probably not made myself so clear as I ought to have. On the other hand, I do think that there are many important parallels between the way science and art develops, and I hope that my point of view may call attention to them. Surely, we have known about revolutions in art (and also that they change the past as well as the future) for a great deal longer than we have known the equivalent point about the sciences. That being the case, I cannot, myself, distinguish between the two enterprises in at

all the way you attribute to me in the sentence that begins on page 19 and continues to the top of page 20. Surely the arts and sciences must still be distinguished, and I am not altogether sure I yet know how to do it. On the other hand, I am pretty well convinced that the particular technique you have chosen for the purpose will not work.

I shall be delighted to hear from you again at any time, but hope you will not feel that this letter has got to be answered. I know what a nuisance a correspondence that will not be terminated can become. Would you please relay my wife's and my greeting to Edith Annin. She may well remember us from old Cambridge days.

Again, all of my most heartfelt thanks.

Sincerely,

Thomas S. Kuhn, Professor
of the History of Science

HARVARD UNIVERSITY
PSYCHOLOGICAL LABORATORIES

Memorial Hall
Cambridge 38, Massachusetts

26 November 1962

University of Chicago Press
Publicity Department
5750 Ellis Avenue
Chicago 37, Illinois

Gentlemen:

I have just read with great enthusiasm T. S. Kuhn's Structure of Scientific Revolutions, and wrote him in Copenhagen my enthusiasm for what he has done. He asked me whether I would not send you parts of one of my paragraphs of praise to him, in order that you might use it in publicity if you cared to do so. Well, here is a paragraph which you may use in whole or in part if you wish to have it. I do hope you will promote the book, because its success could do great good in the thinking of the history of science. Here follows the permissible quote:

> "Dr. Kuhn shows how a science in any period is conducted under certain basic presumptions which he calls a paradigm. A scientific revolution is a change in a fundamental paradigm, a radical change of view from one paradigm to another without intermediate steps (it's like a perception which shifts discretely from one meaning to another), yet a change that occurs slowly and usually against conservative resistance. Well, this brilliant book undertakes to change the paradigm about paradigms. It is exceptionally wise and speaks with clarity and charm, deserving to have an enormous effect. Will it establish a revolution in the way in which the history of science is viewed? Posterity will know about that. Wait around and see."

Yours truly,

Edwin G. Boring
Edgar Pierce Professor
of Psychology Emeritus

HARVARD UNIVERSITY
PSYCHOLOGICAL LABORATORIES

Memorial Hall
Cambridge 38, Massachusetts

26 November 1962

Dear Dr. Kuhn:

Coming back from a few days at our farm in Maine and being about to start off on a lecture circuit, I find your note of 17 November from Copenhagen, a communication which pleases me very much.

I enclose a carbon of what I have written the University of Chicago Press about your book. I hope they will promote the book and that this will be of some use to them.

A preliminary draft of my proposed Presidential Address before the International Congress of Psychology next August has just been dittoed for me while I was away in Maine, and I venture to presume upon your cordiality by sending you a copy of this ditto. It may interest you--in fact, I think it should. But please do not feel that I am putting pressure upon you to read it, for I know too well how everyone is under continued pressure to read these many, many drafts that people are sending around to each other these days. If you do read it in whole or in part and have comment that you would like to make to me, then be assured that I would welcome it. If this paper is to be published in this book of my writings that the editors want to get out at the time of the Congress, then the publisher wants a fair copy of the MS by 14 January. Unfortunately that does not leave much time for revision, especially with the Christmas mails piling up.

Having read your book and feeling enthusiastic about it, it occurred to me that it would be fun to write a review of it. I did that, in about 1200 words, addressing it to Contemporary Psychology (the journal which I got started and of which I was editor until the end of 1961). I had told them last June that if they wanted me to review your book, I would be glad to do it, but it seems to me probable that the publisher does not know that this is a book about psychology. I know that it is, and I can make it clear to psychologists that it is. On the other hand, it will be an unsolicited review, and it might even be that the new editor has got another reviewer, or that he will not want to take this for any of about half a dozen reasons that I could mention. I just hope to make it interesting enough for him to be unable to refuse it. It needs another revision and to be fair-copied, and my assistant, Edith Annin, is up to her eyes in other things just at present. But I shall send you a copy when I have one, whether it is to be accepted or not.

Apart from the major matter, I have three crazy little complaints about your book, none of which, I suppose, is your fault, but all of which presumably arise from your having to use the format of the Encyclopedia of Unified Science.

(1) You just ought to have printed the duck-rabbit ambiguous figure, for your readers will not know what this is and you keep referring to it. More than that, this is not a very good figure. It would have been better to have used the Rubin goblet-profiles or some of the many variants of these, or some of Rubin's ambiguous nonsense figures, or the much-reprinted wife-and-mother-in-law. In this last, you just simply cannot see the two figures at the same time--the black strip that is the mother's mouth and the ribbon around the wife's neck.

(2) It would have been easier reading the book if your frequent references to other sections in the book had been complemented by your having the section numbers in the right-hand page headings. It's very difficult to find what you are referring to.

(3) My current complaint about most publishers is now their silly habit of putting the title of the book on every left-hand page, as if the reader had constantly to be reminded what book he is reading.

I tell you these things now because none of them appears in the review. They are too silly and I suspect are not your fault.

Sincerely yours,

Edwin G. Boring

Edwin G. Boring

Dr. Thomas S. Kuhn, Director
Sources for History of Quantum Physics
Sekretariat of Professor Niels Bohr
Gl. Carlsbergvej 15
Copenhagen-Valby, Denmark

AIRMAIL

Copenhagen, Denmark
November 17, 1962

Professor Edwin G. Boring
Psychological Laboratories
Harvard University
Cambridge 38, Massachusetts

Dear Professor Boring:

Your letter of 9 November has just reached me here, having been forwarded from Berkeley in something close to record time. Except for a few notes acknowledging complimentary copies, it is the first letter that the appearance of my book has stimulated. You will, I think, guess how deeply moved and grateful I am. With respect to the sorts of questions discussed in the book, there are very few people whose opinion I value so much as your own. Since I have, in any case, been awaiting early reactions with a good deal of trepidation, your immensely kind and flattering letter is doubly helpful to me. Many thanks for taking the trouble to write it.

I am delighted with the topic you have chosen for your Presidential Address next summer and very much hope that you will send me a reprint when your talk is published. Assuming that no copies will be available until after next July, you had better send it to my Berkeley address, which you already have. If by any chance copies are ready before that, I should be grateful if you would send one to me here.

Sincerely,

Thomas S. Kuhn
Director

HARVARD UNIVERSITY
PSYCHOLOGICAL LABORATORIES

Memorial Hall
Cambridge 38, Massachusetts

9 November 1962

Dear Dr. Kuhn:

Now I have read your book on The Structure of Scientific Revolutions and I have come away from it with great excitement that you could have put a new paradigm on the account of scientific progress that is as effective and convincing as this one is. What more is there left for me to say? I am working hard at the moment on my Presidential Address for the XVIIth International Congress of Psychology next summer and changing the title for it every few days. It deals with the sense in which Merton's multiples are consistent with the Great Man theory of progress and the sense in which the two are incompatible, and I find myself quoting you or using your words repeatedly. After all, this is a necessary outcome of your thought, because the paradigms are carried in the stream of the Zeitgeist or the "current of credence," as I think it might be called.

Your book is brilliant and deserves to have an enormous effect. The gracious bit of modesty that you wrote on the reprint that you sent me is not appropriate at all, even though you are young and I am old. You say very much that seems to me to be exceptionally wise, and you say it with clarity and charm. Whatever little bit I can do to get people to read you shall be done.

Sincerely yours,

Edwin G. Boring

Edwin G. Boring
Edgar Pierce Professor
of Psychology Emeritus

Dr. T. S. Kuhn
Department of the History of Science
University of California
Berkeley 4, California

March 14, 1966

Dr. John Z. Bowers, President
Josiah Macy, Jr. Foundation
16 West Forty-sixth Street
New York, New York 10036

Dear Dr. Bowers:

I am very grateful for your kind note of March 8th about my Structure of Scientific Revolutions. We are indeed talking about the same Herman Boerhaave. As a chemist, he was also probably more influential through his teaching and writing than for his own original contributions. But he did make some original contributions as well, both to the theory of heat and to an understanding of the role of gases in chemical reactions. And his chemical textbook was probably the most influential work in the whole field during the middle decades of the 18th century.

Sincerely,

Thomas S. Kuhn
Professor of the History of Science

jt

JOSIAH MACY, JR. FOUNDATION
16 WEST FORTY-SIXTH STREET
NEW YORK, N. Y. 10036

JOHN Z. BOWERS, M. D.
PRESIDENT

CABLE: MACYFOUND, NEW YORK
TELEPHONE: 212 PLAZA 7-7705

March 8, 1966

Dr. Thomas S. Kuhn
Professor of the History
of Science
Princeton University
Princeton, New Jersey

Dear Dr. Kuhn:

I have enjoyed very much reading your book, The Structure of Scientific Revolutions. On page 15 you refer to the studies of Chemistry by Boerhaave. Recently, I have been studying and writing about famous physicians at Leyden, and, of course, Herman Boerhaave was, by all odds, the most outstanding of the group.

However, he is generally recognized as a superb clinician, a distinguished teacher and a great human being who made little, if any, scientific contribution to medicine. I should be most interested to learn if we are talking about the same Herman Boerhaave, or if you are referring to another Hollander by that name.

Yours sincerely,

John Z Bowers

JZB:jlk

John Z. Bowers, M. D.
President

27 March 1969

Dr. Saul Boyarsky
Department of Surgery
Duke University Medical Center
Durham, North Carolina 27706

Dear Dr. Boyarsky:

You will know how gratifying it is to any author to receive a letter like the one you addressed to me on March 4. Though I am not going to be able to reply in kind -- the question you raise would demand an article, and one I am not yet really prepared to write -- I do want you to know how very grateful I am. Your letter makes many things worthwhile.

I am in entire agreement with a great many of the things you say, above all with your insistence on the impossibility of categorically eliminating philosophy from science. Whether they know it or not, scientists do possess a philosophy and it does get deeply involved with their work. Furthermore, at least from time to time, they must dig out and deal with these philosophical elements explicitly for the sake of their own continuing research. Whether they would be better at doing so if they had more philosophical training, I am less certain. But that is not at the moment the point most at issue.

About your method of distinguishing the scientific community from the philosophic community, I am less optimistic. The difference between "knowing" and "understanding" is by no means altogether clear. Also, if it is the case that the distinction can be made in terms of methods and goals, then it is hard to see why so many of the sciences should have begun as parts of philosophy. In a sense, I see even more similarities between the two enterprises than you do and would suggest, rather than a difference of gaals, that a given philosophical field, once it does gain a certain degree of development, becomes autonomous. When it does, it ceases to be responsible for its coherence with the balance of philosophy, though it undoubtedly carries with it a philosophy of its own.

If there is a difference between us, and I suspect there is, it comes at the point where you ask whether the philosopher is not also a "puzzle-solver." I think the answer to that question is no. (I, of course, exclude logic

which has now become a largely independent field.) The philosopher is a problem-solver, not a puzzle-solver. The distinction is not altogether easily drawn, but what I have in mind is that quite special characteristic of the fields whose right to the title "science" no one doubts. Normally, the practitioner of such a field knows that, if only he is clever enough, he will be able to solve a selected puzzle within the rules of the current game. The philosopher, on the other hand, knows what his problems are, but has no assurance that they have solutions within the game as currently played. Failure to persuade others that he has found a solution is a mark against the whole enterprise,not against his own ability. That is not, I know, an adequate characterization of the demarcation I have in mind, but it may indicate to you the line a fuller answer would take. The difference between a puzzle and a problem, though not altogether clear, seems to me still extremely important.

Again, many thanks for your letter. I should be glad if this incomplete reply were of any use to you.

Sincerely,

Thomas S. Kuhn
Professor of the History of Science

TSK:lv

Duke University Medical Center
DURHAM, NORTH CAROLINA

March 4, 1969

DEPARTMENT OF SURGERY
DIVISION OF UROLOGIC SURGERY

POSTAL CODE 27706
TELEPHONE 919-684-8111

Dr. Thomas S. Kuhn
Professor of Philosophy
Princeton University
Princeton, New Jersey

Dear Dr. Kuhn,

This letter is in the nature of a fan letter and a lengthy question from the same admiring reader of "The Structure of Scientific Revolutions".

Dr. Marshal Spector introduced me to your book in the course of my planning for an interdisciplinary bioengineering conference on urodynamics. I had consulted him while he was still in our philosophy department at Duke for help with bioengineering modelling.

I found your thesis extremely important and pertinent to my scientific and medical experience. As a matter of fact, I enjoyed it so much that I have gone on to propound (inflict?) my version of its contents to two discussion groups and several friends.

Based on its contents, I would like to discuss with you the relation between science and philosophy.

Let us view science and philosophy as two overlapping, formalized, and partially institutionalized endeavors of mankind to know. Science is an intellectual endeavor, in which man attempts to know exactly and publicly. Emphasis is laid upon testing and harmonizing definitions, theses, descriptions, facts and generalizations.

Philosophy deals with similar subject matter but represents man's intellectual attempts to understand. Emphasis is placed upon definitions, background, derivations, and logical coherence. Intellectual and verbal tools, dialogue and discussion, predominate over testing, experimentation, probing, or other comparison with the realities of the market place and world.

Each uses all the methods of the other freely.

Each field has been shaped by its methods and history (philosophy emphasizes the use of verbal and symbolic tools, whereas science emphasizes the manual

and manipulative tools of our intellect) and show bias in content best diagrammed by two concentric ellipses whose major axes are perpendicular.



There is an exceedingly large area of overlap and common content.

The disciplined efforts of the scientific community, which you described so well in your book, are visible because of the size, ease of communication, investment in equipment and training, and quasi-familial relationship of this community. Successful scientists have been members of prominent laboratories. (How few successful scientists have been outside the mainstream of a scientific effort all their lives?).

Communication over a very specific topic is more rapid in the scientific community; meetings, mail, telephone, and the literature. There are many such whirlpools of activity all the time. By contrast, the philosophic community depends more upon books and journals. Communications are slower. True, there are issues and hot discussions in philosophy, but not so many. Federation meetings annually attract 16,000 scientists to Atlantic City in one April week. The literature is a year "behind" and testbooks are several years "behind" in science.

Science is often a later, more detailed outgrowth of prior philosophic consideration (nor can it survive without philosophy) and not just at the beginning.

But subdividing and fragmenting a problem further and further to facilitate its scientific study is not a linear, progressive or branching process. As you point out in your monograph, there come times when it is necessary to go back to the very beginning, to reexamine all the elements, to retrace one's intellectual past and to start over again with a fresh viewpoint. So the thinking scientist must return to philosophy from time to time. Indeed, a study of vagueness and definition shows us exactly why there must be a great deal of backtracking along the path of specialization.

Another difference between the two fields comes from the specialization exacted by scientific manipulation. The scientist is a specialist in the use of one or more laboratory techniques. As you pointed out, his training and career start in this manner. He is an expert "gadgeteer" and technician; this is valued highly in our society. I wonder if our society admires his "prowess" and if this may not be part of the reason for the high status of the scientist.

Philosophers can be compared to composers. They are not so subjective as poets, they do not deal with such individualized, unique experiences. Their realm of activity seems free, but confined on a cosmic scale by a broad framework; by man, his senses, his thoughts, and his environment. In spite of the

latest exploits of our astronauts, we must still acknowledge that man remains confined to the interface of the earth and atmosphere, a relatively small part of the cosmos. The breadth, depth, and variability of any man's memory, knowledge, recall and foresight are modest in comparison to the numbers of men who pass back and forth along the same path every year, every generation on every continent, and who exercise roughly the same mental capacities.

How many different philosophies can man create? Certainly the number is finite and can be described by a mathematical symbol, whether it be 10, 100, or 10^{20}.

Man has limits and constrictions on his ability to perceive, he has constrictions to his mental power to process his perceptions, no matter how high we raise his upper limit, whether it be to an IQ of 160, 200, or 200+.

Viewed in this perspective, the philosophers are members of a community with circumscribed tools. They are subject to limits and boundaries of efforts. They are subject to a discipline, to a need for definitions, to selection of subject matter, to desire to communicate, and to concern for man. The community may be more far-flung in time and in space than the scientist's but it remains a community. And, there have been philosophic revolutions, just as there are scientific revolutions.

I cannot conceive that the philosophic community is not disciplined, cohesive, communicative, or structured. I believe that the philosophic community operates over a wider span of time, space, and with fewer numbers. Because the members of the army are fewer and man isolated outposts, rather than marching four abreast, things seem to go differently.

I believe that the differences between science and philosophy have developed first from the differences in tools, then from the subject matter, and later frozen by the fashion of the century or decade.

The flow of human knowledge can be likened to a stream overflowing its banks. Each dry area becomes surrounded, rivulets encroach on each dry spot from all sides and finally engulf it.

Someday, I wonder if we will rewrite a history of knowledge in which we may focus on philosophy and ignore the flow of science. It will hop and skip across time, ignoring the dark ages when people did nothing but measure, and didn't think. It will point to the bright spots in history, of man's realizations, awarenesses, and definitions of his environment, and treat the "grubby" scientific manipulations as necessary tedium.

Dr. Thomas S. Kuhn March 4, 1969

It seems to me that the philosopher is also a puzzle solver. But he works on a grander and more subjective scale and with more scholastic methods. He uses humanistic, verbal, logical and emotional tools better, by necessity and practice. He may be further from technology than the scientist. By using fewer machines he is less a slave to them. He may appear to be less "objective" but is he not using one of the finest machines of all, the human mind?

Does not the philosopher also submit to the rules of the community, by insisting upon definitions and language tools? He uses the data or conclusions of the scientist, as the lessions of science become common knowledge.

It seems to me that there is a positive side to this discussion. We must teach more philosophy to the scientist. We need more philosophers. Philosophy should be taught in the schools at an earlier age and it should be extended into the latest fields of all walks of life, including science. The scientist needs to use philosophy every day.

It seems to me that all of science is susceptible to philosophic analysis and just as philosophy must consider the latest scientific techniques and "facts".

You have undoubtedly spent more time and skill expounding this than I, and I fear that I have merely shown how late in life I have learned the importance of philosophy.

I believe that it serves the best interest of science and philosophy to strive to increase the areas of overlap to the greatest possible coincidence.

I hope that I have not bored you. The question? How would you compare the philosophic community with the scientific?

Sincerely yours,

Saul Boyarsky

Saul Boyarsky, M. D.
Professor of Urology

SB:h

AIRMAIL

Copenhagen, Denmark
May 28, 1963

Professor R. B. Braithwaite
King's College
Cambridge
England

Dear Professor Braithwaite:

Your very kind letter of 16 May was twice forwarded before it caught up with me in Copenhagen. I am altogether delighted with it, and am answering almost by return mail.

I suspect that you are still giving Eddington somewhat too much credit. Isn't it likely that Eddington, when he refers to Rutherford as "the real villain of the piece" in the passage you cite, is actually thinking of the quantum theory and not of relativity at all? As to your own position, I find it both new and extremely plausible. Men who, studying physics after the First War, took the Bohr-Rutherford atom seriously would almost have to favor Lorentz-Invariance to Galilean-Invariance at the points of conflict. On the other hand, one should remember that there was an escape hatch for anyone really motivated to use it. The Bohr-Rutherford atom was in large conflict with the electrodynamics of Maxwell and Lorentz. Presumably a new electrodynamics was needed, and it might conceivably have had different invariance properties. Probably, the real force of your point comes in the period before the first War. Rutherford's was by no means the first electrical theory of matter. The electron theory of Lorentz or the atom of J. J. Thomson could by themselves have provided a basis for the choice you suggest. It would be thoroughly worth trying to discover to what extent they did so. I am far from clear that I shall have the chance to pursue the lead, but I am extremely grateful for it.

I, too, very much enjoyed and profited from our talk. I only wish that there could have been somewhat more of it. Also, I very much wanted the chance to thank you again for the fine dinner at King's. Only, you dashed away from the seminar too soon. Please do accept my thanks in this form. And if you should ever have any further thoughts about the book or the other issues we discussed, I have been known to answer letters about issues that excite me.

Sincerely,

Thomas S. Kuhn

KING'S COLLEGE,
CAMBRIDGE.

16 May 1963

Dear Kuhn,

Last night, in attributing to Eddington the thought that the essence of the 1905 Einsteinian Revolution lay in a realization of the incompatibility of the Lorentzian and the Galilean principles of invariance followed by an opting for the former because Maxwell's Equations satisfied it, I was indulging in false reminiscence and was, perhaps, minimizing my own originality in having the thought. I published it in a long review-article on Eddington's The Nature of the Physical World (1928) which appeared in Mind 38 (1929), where I wrote:

> "The strength of the relativist position cannot be fully realized without a knowledge of its scientific background: Mr Eddington puts this in a paradoxical form by saying that Sir Ernest Rutherford and not Dr Einstein is 'the real villain of the piece' (p.1). For when it is accepted that 'the atom is as porous as the solar system' and that the forces holding the atom together are electrical, the laws of electricity immediately take precedence over laws about rigid bodies. The essence of the first Theory of Relativity is that, in the conflict which arises between the possibility of the applicability to all systems of Maxwell's electromagnetic equations and of Newton's Laws of Motion, we accept the claims of the former and consequently modify the latter. And to a physicist convinced that material bodies are electrical systems and are not rigid in the old sense (which Mr Eddington surprisingly calls being a 'substance'), there can be no doubt as to the right choice. Mr Eddington is certainly approaching the subject in the best way when he treats it in close connexion with the electrical constitution of matter, though this treatment assumes some knowledge of the development of physics during the last century" (p.412)

KING'S COLLEGE,
CAMBRIDGE.

On looking at Eddington I see all that he said, in his first chapter entitled "The downfall of classical physics" was:

> "Between 1905 and 1908 Einstein and Minkowshi introduced fundamental changes in our ideas of space and time. In 1911 Rutherford introduced the greatest change in our idea of matter since the time of Democritus. The reception of these two changes was curiously different. The new ideas of space and time were regarded on all sides as revolutionary..... The new idea of matter underwent the ordinary experience of scientific discovery; it gradually proved its worth....No great shock was felt. And yet when I hear today protests against the Bolshevism of modern science and regrets for the old-established order, I am inclined to think that Rutherford, not Einstein, is the real villain of the piece" (p. 1 of original edition).

Eddington does not refer in the book (written of course for laymen) to principles of invariance. And the argument as I gave it does not appear (in that form) in his earlier books. I may have got to it from the emphasis given to comparing Galilean and Lorentzian transformations in a semi-popular exposition of relativity theory Space Time Motion by A.V.Vasiliev, of which an English translation was published in 1924 (of which I believe I wrote a review in The Nation or The New Statesman).

I don't write this to boost my own originality, but because I gave false information to a historian! [Nevertheless I do think it's the best way of regarding the 1905 Einsteinian Revolution: unless I am entirely mistaken in my memory, it was the clinching argument that, in combination with the Michelson-Morley experiment (and its successors), made me accept it.]

I have very much enjoyed, and profited by, meeting you; and hope we shall meet again in the not too distant future.

Yrs & yours back. Love
given me much to
think about.

Yours sincerely,
RBBraithwaite

P.T.O.

You've misspelt my name on p. 99. It's a Norse name:
brai = large, thwaite = cleared space (in the forest).
[My Norse ancestors came to the Lake District
about 1000 A.D. having sailed round the Hebrides
& perhaps touched at America en route].

Structures

May 26, 1967

Professor J. Bruce Brackenridge
Department of Physics
Lawrence University
Appleton, Wisconsin 54911

Dear Professor Brackenridge:

I am very grateful to you for repeating the invitation which Professor Tjossem sent me a year ago. I am even more tempted this time than before (particularly, because I entirely grasp the implications of your last paragraph), but I am going to have to give you the same answer that I gave him. My problem is double. During the last couple of years, as requests like yours have multiplied, I have increasingly and painfully realized that I must drastically restrict my activities as a propagandist for my views if I am ever to get the time to write another book, whether that sort or a different one. More particularly, next year is going to be a particularly busy one for me, since my colleague Gillispie will be on leave and I must take over the entire administrative responsibility for the Program here. Since I have already agreed, in addition, to give a few more outside talks than I should be undertaking, I am forced to be altogether rigid.

All I can add it that, since I am still very deeply committed to the ideas in my last book, it hurts me very much whenever I have to write a letter of this sort. Your own very good and flattering letter is a particularly difficult one to refuse.

Sincerely,

Thomas S. Kuhn
Professor of the History of Science

jt

LAWRENCE UNIVERSITY
APPLETON, WISCONSIN 54911

May 23, 1967

Professor Thomas S. Kuhn
Department of Philosophy of Science
Princeton University
Princeton, New Jersey

Dear Professor Kuhn:

Last year Professor Tjossem wrote to you describing the freshman studies course at Lawrence and inviting you to lecture to the faculty and students participating in the course. This year, I am responsible for the organization of the course and would like, once again, to invite you to participate as a lecturer. The class will read and discuss The Structure of Scientific Revolution during the third and fourth weeks of the first term. The date set for the lecture is Tuesday, November 7. The lecture would be given in the morning, followed by a discussion meeting at lunch. Appleton is served with excellent direct air transportation to O'Hare field in Chicago (eight one-hour flights each way each day). You should be able to leave New York late Monday afternoon and return by early evening on Tuesday. We will provide expenses and an honorarium of $100.

Your book worked very well in the course this year. It was, in fact, one of the two works that the staff elected to repeat this year (the other was the Republic). It was followed in the course by Asimov's Genetic Code and Engel's Socialism, Scientific and Utopian. The former provided an interesting case study and most students elected to discuss it from the vantage point suggested by your work. The latter book provided an interesting contrast in the reading of history. Next year we are going to use the section on planetary systems from Holton's Introduction to Concepts and Theories in Physical Science as auxiliary material.

You may be interested in knowing that the lecturer this year on your book was Michael Scriven. If you have read his writings, you already know that he defends his own ideas and very few others. I hope very much that you can give the lecture this year; I would much prefer to hear what you now think to what others think you should think.

Sincerely,

J. Bruce Brackenridge

J. Bruce Brackenridge
Department of Physics

JBB:emw

December 21, 1970

M. Michel Braudeau
Éditions du Seuil
27, rue Jacob
Paris VI^{e}, France

Dear M. Braudeau:

Your inquiry of 9 December 1970 has just reached me. I am not at this time working on a new version of my Structure of Scientific Revolutions. However, the edition of the book which appeared last April contains a few minor revisions and also one large additional chapter. Undoubtedly it is that revised version which your informant had heard about. Undoubtedly the copy you receive from the University of Chicago Press will be of the new edition with revisions.

Sincerely,

Thomas S. Kuhn
Professor of the History of Science

TSK/ec

ÉDITIONS DU SEUIL
27, RUE JACOB, PARIS-VI^e^ - 326-84-60, 61, 62, 63

MB/ML Paris, le 9 décembre 1970

Cher Monsieur,

Nous avons demandé à votre éditeur (University of Chicago Press) de nous faire parvenir un exemplaire de votre livre : The structure of scientific revolution. Est-il bien vrai, ainsi qu'on nous le dit, que vous préparez une nouvelle version de ce livre ? et est-elle achevée ? Nous vous serions reconnaissants de bien vouloir nous en informer.

Dans l'attente de votre réponse, nous vous prions d'agréer, cher Monsieur, l'expression de nos meilleurs sentiments.

Michel BRAUDEAU

Monsieur Thomas S. KUHN
c/o University of Chicago Press
5 750 Ellis avenue

CHICAGO Ill. 60637 (U.S.A.)

S. A. AU CAPITAL DE 1.500.000 F ● C. C. P. PARIS 3.042.04 ● R.C. SEINE 54 B 5772 ● PROD. SEINE 8.985
20.030/6/69

September 16, 1965

Professor John C. Burnham
Department of History
The Ohio State University
216 North Oval Drive
Columbus, Ohio 43210

Dear Professor Burnham:

I have just received your letter of September 7, together with the student papers that it introduces. Though I am not at all sure how quickly I shall be able to get to them or that I shall manage to read them all, I am most grateful to you and Professor Rule for sending them along. Just a glance at outline and bibliography leaves me much impressed by the the effort and intelligence that has gone into the work. And even if the performance did not seem impressive, I should want very much to get some taste of what my book does for students at this level and in this situation. You will guess that I am still deeply involved with the enterprise, and I collect all reports, both positive and negative, with considerable interest.

That being the case, I should be particularly grateful if you or Professor Rule had a chance to write me briefly about the manner in which you used the book and on how it worked out in the application. Or perhaps you have a copy of some report that you submitted to the National Science Foundation. I have the general idea already, and I know how hard it is to find time at the beginning of the term. Therefore, you should not have this nuisance request on your mind. But if there is any easy way to give me a bit of additional information, I shall be grateful. But, then, I already am.

Sincerely,

Thomas S. Kuhn
Professor of the History of Science

jt

THE OHIO STATE UNIVERSITY
DEPARTMENT OF HISTORY
216 NORTH OVAL DRIVE
COLUMBUS, OHIO 43210

September 7, 1965

Professor Thomas S. Kuhn
Program in History and
Philosophy of Science
70 Washington Road
Princeton University
Princeton, New Jersey 08540

Dear Professor Kuhn:

This past summer The Ohio State University, Department of History, with the support of the National Science Foundation, has offered a special full-time summer program to ten undergraduates in various sciences. The participants were chosen competitively from all of the several dozen Ohio colleges and universities.

The reason that I write to you is that the program was designed to introduce each of the students to the history of science and in particular to the history of his or her own science--the one in which the participant intends making a career. The procedure that we used was to ask each of them to test a scientific revolution or an alleged scientific revolution to see if it conforms to the--you will excuse the term--paradigm that you have stated in your lovely book, The Structure of Scientific Revolution.

My colleague, Professor John C. Rule, who with me jointly conducted the program, thought that you might be curious to see the result. At any rate, in case you should be at all interested, we enclose copies of the students' final (but uncorrected) papers.

Sincerely yours,

John C. Burnham

John C. Burnham
Assistant Professor

ae/

Comments on "On the Distortion of the History of Science in Science Education"*

STEPHEN G. BRUSH
Department of History and Institute for Physical Science and Technology, University of Maryland, College Park, Maryland 20742

Harvey Siegel's paper, "On the distortion of the history of science in science education" (Siegel, 1979), expresses views so similar to my own that it may seem ungracious for me to question some of his arguments.

He says that we both reject (but for different reasons) Thomas Kuhn's recommendation that the history of science should be distorted in the training of scientists; and he claims that the Project Physics Course is a counterexample of a successful use of an *accurate* historical account in science education.

It is difficult for me to disagree with the last point since I wrote part of the Project Physics text myself—although not the unit on astronomy which Siegel singles out for praise. As he notes, students who take this course do as well on physics achievement tests as those who take more conventional courses, even though this group probably includes many students who would not otherwise have chosen to take physics at all, and may have lower aptitudes and motivation for it. This result certainly indicates that the introduction of history need not hinder the acquisition of scientific knowledge and skills. But it is premature to conclude that the course is "successful in producing competent scientists," since it has only been available for a few years and I doubt if more than a handful of its graduates have completed their doctorates.

The main reason for this paper is to correct the impression that I am a critic of Thomas Kuhn. While we certainly disagree on some points, I think his *Structure of Scientific Revolutions* (Kuhn, 1970) is the most important book on the history of science published in recent decades. Unfortunately it has been eagerly accepted by sociologists, and disdainfully rejected by philosophers, without having been subjected to serious critical examination by other historians of science. It presents science educators with a paradox: if one accepts Kuhn's theory, one should ban his books. One cannot escape the paradox, as Siegel tries to do, simply by arguing that an historical account of how an earlier paradigm was replaced "for specifiable reasons and shortcomings" would "demonstrate . . . the effectiveness of the new paradigm." That argument ignores one of the crucial and most controversial points of Kuhn's thesis: paradigm changes *cannot be rationally justified* in a way that is completely convincing to adherents of the old paradigm. It is this *downgrading of the objective basis* for a revolution that Kuhn's opponents (e.g., Israel Scheffler) find distasteful.

* Siegel, H. *Sci. Educ.*, 1979, *63*(1), 111–118.

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In order to refute Kuhn's inference that a certain type of distorted history is *functional* in science education (I do not think Kuhn has claimed that it is morally desirable) one must either attack his general theory of how science works, or one must decide that efficiency in solving puzzles (as he characterizes normal science) is less important than other values such as truthfulness about history, or a critical attitude toward accepted theories, or a more favorable public conception of scientists. Neither Siegel nor I have yet attempted the first kind of refutation.

Now let us consider another paradox: the account of the Copernican Revolution given in the Project Physics text (which Siegel seems to think would be acceptable to Kuhn) is historically *more* accurate than Kuhn's own account, inasmuch as it avoids Kuhn's overstatement of the objective reasons for the abandonment of the Ptolemaic system. The basis for this assertion is an article by astronomer-historian Owen Gingerich (1975), refuting Kuhn's statement that by the time of Copernicus geocentric theories had been made excessively complicated in order to fit recent observations, thus creating a need for a simpler, more accurate theory (Kuhn, 1970, pp. 67–69). Gingerich finds no historical evidence to support this statement, and argues that the initial advantage of the heliocentric system was aesthetic—it was "pleasing to the mind" as Copernicus said (Copernicus, 1959, p. 57). This confirms the account given by the Project Physics course (1975, Unit 2, pp. 38–39)—not too surprisingly, since Gingerich is one of the consultants to Project Physics (1975, p. A22).

Gingerich's analysis suggests that those who attack Kuhn for portraying paradigm-change as too *subjective* have overlooked the real weakness in his theory—it assumes revolutions begin with a "crisis" caused by increasing discrepancies between theory and observation, an assumption that gives too much weight to *objective* factors. If that is the case (although it has by no means been demonstrated as yet) then part of Siegel's justification for the use of history in science teaching—demonstrating the effectiveness of the new paradigm by explaining why the old one had to be replaced—would become even more vulnerable. On the other hand, if one accepts Kuhn's attitude that a cohesive community is important to a mature science, a science educator cannot really separate the function of preparing students to adopt the social role and behavior-pattern of the scientist from indoctrination in the prevailing paradigm. Hence, contrary to Siegel's statement, he and I are not so far apart in our reasons for advocating the use of accurate history in science education.

References

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Gingerich, O. "Crisis" versus aesthetic in the Copernican revolution. *Vistas in Astronomy* (1975) *17*, 85–95.

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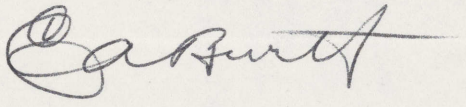
December 18, 1965

Dear Professor Kuhn,

Your reply to my note is very kind and very encouraging. I shall hope that when you have time to do some reading in the book you may be moved to let me have some comment on it. I shall prize any such comment very much.

I am writing you now because I expect that the book will have been sent to the address I had for you in Berkeley. Perhaps you will not mind having whoever takes care of such matters at the University of California to make sure that the book is forwarded to you. Academic people are moving around more rapidly than ever these days.

Very sincerely yours,



THE SOCIETY FOR THE HISTORY OF DISCOVERIES

ANNUAL MEETING

PEABODY MUSEUM SALEM, MASSACHUSETTS

NOVEMBER 13 - 14

Friday, November 13

Morning: 9:30 Registration, Peabody Museum

10:30 Welcome to Peabody Museum
Ernest S. Dodge, Director

Session I: Pacific Exploration

J. H. Boykin, Raleigh, North Carolina
"American Pacific Discoveries."

Roy A. Rauschenberg, Ohio University
"Daniel Carl Solander, the Naturalist of the 'Endeavor' Voyage."

Discussion

12:00 Lunch on the town.

Afternoon: 2:00 "Nineteenth Century Pacific Exploration," A special exhibit of the Peabody Museum and talk by Ernest S. Dodge, Director.

3:30 Business meeting of the Society

5:00 Cocktails

7:00 Dinner Meeting, Welcoming Address by Wilcomb E. Washburn, President of the Society.

Saturday, November 14

10:00 Session II: History of Geographic Thought

Edward Towle, University of Wisconsin
"Paradigms, Perceptions and Metaphorical Models in the History of Geographical Thought: A Speculative View."

Vincent H. De P. Cassidy, University of Southwestern Louisiana
"The Geographic Concept in the Thirteenth Century."

Thomas E. Goldstein, Bard College
"Geographic Thought in the Fifteenth Century Against the Background of Medieval Science."

Discussion

Please send reservations for the dinner on November 13 directly to Mr. Ernest M. Dodge, Peabody Museum, Salem, Mass. Payment for the dinner can be made at time of registration. Cost of the dinner will be about $5.00.

The hotel nearest the Peabody Museum is the Hawthorne Hotel, and a conveniently located motel is the Pilgrim Motel at 40 Bridge Street.

BRANDEIS UNIVERSITY
WALTHAM 54, MASSACHUSETTS

Department of History

27 November 1964

Professor Thomas S. Kuhn
Department of History
Princeton University
Princeton, New Jersey

Dear Tom:

I enjoyed our recent talk and am grateful for the information about Santa Barbara. Your reaction to my ideas about Foucault's pendulum experiment has encouraged me to try and get a paper ready on this subject for the 11th International Congress next summer. In case you're further interested in the copious 19th-century literature on the subject of proving the earth's rotation, there is a good account with critical discussion in J. G. Hagen, La rotation de la terre (Specola astronomica Vaticana, 1) (Rome, 1911). To indicate that the subject was not only for lesser physicists, let me point out that H. Kammerlingh Onnes, the founder of cryogenics, did his thesis in 1879 on proving the earth's motion by pendulum experiments.

In case you're keeping track of the influence of your latest book, I enclose a copy of the program for the meeting I mentioned. Towle tells me that he has a book entitled The Seafaring Frontier coming out soon with Wisconsin. Apparently he is fascinated by the way in which your new mode of discourse gives him a handle on the mid-19th century notion of an open polar sea.

I look forward to seeing you before too long and perhaps to hearing from you if anything turns up in which I might be interested (is anything doing at Rutgers? They were interested in 1961 but Bill Coleman, Larry Holmes, and I talked them out of it.). Please give my best to Arno Mayer.

Sincerely,

Harold

Harold L. Burstyn

hb: h
enc

HARVARD UNIVERSITY
PSYCHOLOGICAL LABORATORIES

Memorial Hall
Cambridge 38, Massachusetts

9 November 1962

Dear Dr. Kuhn:

Now I have read your book on The Structure of Scientific Revolutions and I have come away from it with great excitement that you could have put a new paradigm on the account of scientific progress that is as effective and convincing as this one is. What more is there left for me to say? I am working hard at the moment on my Presidential Address for the XVIIth International Congress of Psychology next summer and changing the title for it every few days. It deals with the sense in which Merton's multiples are consistent with the Great Man theory of progress and the sense in which the two are incompatible, and I find myself quoting you or using your words repeatedly. After all, this is a necessary outcome of your thought, because the paradigms are carried in the stream of the Zeitgeist or the "current of credence," as I think it might be called.

Your book is brilliant and deserves to have an enormous effect. The gracious bit of modesty that you wrote on the reprint that you sent me is not appropriate at all, even though you are young and I am old. You say very much that seems to me to be exceptionally wise, and you say it with clarity and charm. Whatever little bit I can do to get people to read you shall be done.

Sincerely yours,

Edwin G. Boring

Edwin G. Boring
Edgar Pierce Professor
of Psychology Emeritus

Dr. T. S. Kuhn
Department of the History of Science
University of California
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