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On the Fundamental Importance of the Social Psychology of Research As A Basic Paradigm For the Philosophy of Science: A Philosophical Case Study of the Psychology of the Apollo Moon Scientists

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Abstract: A combined philosophical and social psychological study of over 40 of the Apollo moon scientists reveals that the Orthodox or Received View of Scientific Theories is found wanting in several respects: (1) observations are not theory-free; (2) scientific observations are not "directly observable;" (3) observations are no less problematic than theories. The study also raises some severe criticisms of the distinction between the context of discovery and the context of justification. Not only does this distinction fail to describe the actual practice of science but even more important it has the dangerous effect of excluding some of the strongest lines of evidence which could most effectively challenge the distinction. The distinction is harmful of efforts to found interdisciplinary theories and philosophies of science.

This paper is strongly supportive of Kuhn's efforts to introduce social psychological considerations into the philosophy of science. It is concerned with showing what the social psychology of research distinctively has to contribute to the philosophy of science by drawing the philosophical implications of a specific study of an interesting group of contemporary scientists. While basically in agreement with Kuhn, the paper nevertheless shows that substantial modifications of Kuhn's position are in order. These modifications could not have been arrived at without recourse to social psychological considerations in some form or another. Social psychology has a greater role to play than philosophers of science have traditionally been willing to grant it. It is certainly not limited to uncovering mere matters of empirical fact.

Introduction

This paper represents a partial and greatly abbreviated report of a philosophical case study of the psychology of the Apollo moon scientists. The subjects of the study were over 40 of the scientists who participated in the Apollo lunar missions. The basic purpose of the study was to test certain contemporary and critical propositions in the sociology, psychology and philosophy of science by means of a combined philosophical and behavioral science investigation into the attitudes, beliefs, and practices of a highly interesting yet specific group of scientists. Each scientist in the study was intensively interviewed on four separate occasions over a span of three years. The scientists were generally interviewed in the time period just after the completion of one Apollo mission and before the start of another, thus between Apollo 11 and 12, 12 and 14, 14 and 15, and 15 and 16.

The central question with which the study began and which occupied it throughout was the following: Would it be possible to identify and to study those scientists, if any, who exhibited a high degree of prior commitment (i.e., before Apollo 11) to certain pet hypotheses or theories regarding the nature and/or origin of the moon and who thus showed as a result a high degree of reluctance to give up their pet hypotheses in the face of or in spite of the data returned subsequently from the moon? In terms of the interviews with the 40 scientists, a small number of scientists were overwhelmingly and consistently nominated by their peers as the ones "most likely to hang onto their pet hypotheses 'til death do them part."

The perceptions of these few key scientists by their peers (the 40 scientists) were repeatedly studied over each interview round for their implications for the sociology, psychology, and philosophy of science. In general the issues and subsequent topics that were investigated were too numerous for us just to be able to list them, let alone for us to report the results of them here. However, in terms of the central issue of commitment it was shown that it was possible to measure systematically and precisely the differences in psychology between (1) those scientists who were judged most likely to become committed to pet hypotheses and to take strong stands on scientific issues and (2) those who were judged to avoid taking strong stands or not to develop intense commitments. Not only were the differences between these two "types" of scientists immensely striking, but they were also, in statistical terms, highly significant. Furthermore they emerged continually. No matter what was used to measure them, the same differences were obtained repeatedly. In short the study revealed that there are indeed definitive and very strong systematic differences between different kinds of scientists. Most of all, it is possible to capture and to measure these differences in psychology systematically and precisely. One of the major purposes of this paper is to draw the significance of these results for the philosophy of science.

Another major part of the study was concerned with the precise measurement, in information theoretic terms [18,48], of the change in scientific beliefs of the scientists with respect to certain hypotheses proposed for the origin of the moon and to account for its properties. To this end, repeated measurements were taken with respect to how probable or likely each scientist thought the various scientific hypotheses were at various points in the conduct of the Apollo missions. The beliefs and attitudes of the scientists were also studied and measured with respect to certain basic issues in the philosophy of science. Considerations of space prevent us from reporting here on the information theoretic analysis of the change in beliefs with respect to scientific hypotheses. However we will comment briefly on some of the results having to do with the questions that pertained to philosophical issues. Of necessity, the reader is referred to a forthcoming book, The Subjective Side of Science: A Philosophical Enquiry Into The Psychology Of The Apollo Moon Scientists [33], for a full description of the study, e.g., a detailed description of the sample of scientists, the study's methodology, its results, and most of all, its implications.

One of the major purposes of this paper is to demonstrate what the social psychology of research uniquely has to contribute to the philosophy of science. It is the contention of this paper that there are certain contributions which are beyond the logic of science, at least as it is currently constituted, to make. This paper thus enters centrally into the recent dispute between Kuhn and Popper [25,38] regarding the relative merits of the logic of research and the social psychology of research. In general this paper is supportive of the position of Kuhn, critical of Popper's. However the paper does show that Kuhn's position is in need of some fundamental revisions if it is to meet some of the criticisms that have been legitimated lodged against it [23,45,46,51]. Some of the same revisions will also be shown to apply to Feyerabend's position (s). Above all the entire study mounts a severe critique of the orthodox [13] or received view [47] of scientific theories. It is thus as a result very much in sympathy with Toulmin's latest efforts [51].

<u>The Logic of Research Versus the Social Psychology of Research: A False Dichotomy and A Poor Policy for the Study of Science</u>

If it can be argued that a scientific theory is a policy--in effect, a directive--with respect to how one <u>ought</u> to view and to study Nature [10,49], then I would argue that a theory about the nature of science is also a policy, in this case, a policy with respect to how one ought to study and to characterize science. That is, a particular theory about the nature of science is obviously not a "factually real description of science as 'it is'," but rather it directs those who hold the theory to seek a particular kind of explanation of science. But if so, then a particular theory about science is also at the same time a partial state description of its adherents. As much as it tells us something about the nature of science (which it does), it also tells us something about the nature of its proponents. It certainly is expressive of their commitment(s) to a particular point of view. In this regard, the current tendency to view the logic of research and the social psychology of research as locked in an adversary or "zero-sum game" situation may be more indicative of the current state of our philosophy of science than it may be of either science, logic, and social psychology themselves. Contrary to widespread belief (or at least in certain quarters of the philosophical literature) it does not follow that the only way to view the relationship between logic and social psychology is that of an adversary one or one of a superior/inferior relationship. I would argue that not only are there other ways to view the relationship, but that even more important, the failure to take seriously some of these other ways has actually impeded some interesting new lines of research and developments in the philosophy of science. Even worse, it has had the disastrous side effect of excluding before the fact some of the strongest lines of evidence which could most effectively challenge some of the prevailing points of view.

It would be considerably easier to accept the arguments of those who have been most severe in their condemnation of the social psychology of research and of its potential contributions to the philosophy of science were it not for the following: (1) that in every case it is possible to show [32,33] that their own position rests upon or embodies a particular, largely implicit, social psychology; i.e., their own position rests upon a number of propositions regarding the nature of science and scientists which are sociological and psychological in their import; (2) that for the most part this implicit "homespun" social psychology is inferior to the professional products already available on the market (i.e., in the professional literature); and (3) that the critics of social psychology have for the most part exhibited a gross misunderstanding with respect to the aims of the social psychology of research on the one hand and a gross ignorance of the facts and methods of social psychology on the other. Given this, it is little wonder then that the view of social psychology which has largely been promulgated has tended to be the worst characterization of social psychology possible. For example, not only is it a gross misunderstanding of the aims of social psychology but it is basically demeaning of it to say that its potential contributions are limited to "uncovering mere matters of empirical fact" or that it is perpetually relegated to studying only those aspects of science having to do with the messy illogical aspects of discovery. It is also a misunderstanding to perveive it as having little if anything to contribute to the study of the epistemology of testing scientific ideas. Given these perceptions of the role of social psychology, it is little wonder then why any appeal to the social psychology of research has tended to be met with charges of sociologism or psychologism.

It is unfortunately beyond the scope of this paper to demonstrate the actual psychology that is implicit in the views of those who are critical of the social psychology of research [cf.,32,33]. Instead the focus is on demonstrating how the social psychology of research can be brought to bear on philosophical problems and how social psychology can form an essential part of an integrated program in the philosophy of science. In this regard it should be noted that C. West Churchman has consistently articulated a philosophy of science [5,6,7,8,9] wherein the logic of research and the social psychology of research are taken as vital but yet as only partial components of the program. In other words, neither the logic nor the social psychology of research form the whole or the core of the program. Indeed, no single science forms the core or becomes the single basis for explicating science. All of the sciences now known are conceived of as indispensible in the sense that there is an aspect of the process of science that each of the various sciences is uniquely suited to uncovering and to studying. Viewed from this perspective, the relationship between the logic and the social psychology of research is a complimentary one, not a hostile adversary relationship, although it is vitally necessary that each of these components be free to criticize the other. In terms of this view of science, the Reichenbach distinction [40,41] between the context of discovery and the context of justification is not only naive but it is actually harmful to studies into the nature of science. The distinction is regarded as naive because a Churchmanian view of science does not recognize that scientific activity can be fundamentally partitioned into discovery versus testing or justification phases; i.e., it does not recognize that there is a sharp cleavage between the acts of discovery and of testing, and as a result, it does see where the process of discovery can be explicated independently of the process of testing, and vice versa. As a consequence, it also regards the distinction as harmful because it not only promotes a separatist, piece-meal view of science in theory but also in practice; e.g., it discourages the kind of broad-based interdisciplinary studies which could uncover the necessary evidence that could effectively challenge the basis for the distinction in the first place. The study on which this paper is based is in effect an example of a Churchmanian program in the philosophy of science.

On the Nature and Function of Commitment and Bias in Science: The Case For Scientists As Highly Partisan Advocates

One of the (if not the) most striking things about the contents of the interviews was the tremendous extent to which they documented the intense emotions that permeate the entire process of science. No matter what the topic that was being discussed--for example, the status of a certain physical theory--the scientists continuously moved the discussion toward a consideration of highly personal matters and social concerns which effected their stance toward the topic. It was fundamentally impossible for them to discuss the status of a physical theory and the evidence pro and con with respect to that theory without their reacting to the proponents of that theory and in the most intense and volatile of terms. It would appear that it is only in idealized accounts of science that scientists are able to keep their personal feelings toward the issues (and particularly their feelings toward those of their peers who are associated with the issues) clearly apart from their abstract, impersonal thoughts about the issues. This is not to say that scientists ought not to keep the two clearly apart. It is to say however that the reasons that have been advanced in support of separating the two largely amount to meaningless prescriptions if scientists are psychologically unable to obey them in the heat of practice. If scientists are to keep these two aspects apart then we will have to provide them with far more effective means that will actually allow them to accomplish this. Indeed if it is fundamentally impossible to confine intense emotions in science to a particular area,then the most effect means would be a theory about how emotions actually function in the whole of the scientific process so that we could effectively account for their influence. If we could thus account for them, we would not have to worry about eliminating or confining them. Whatever the outcome, mere prescriptions and abstract discourses on "good scientific method" alone do not seem sufficient to do the job (they are necessary however). Furthermore, it is even debatable whether scientists should always keep the two apart. If there are good reasons for keeping the highly personal (and supposedly subjective) apart (or at the very least,distinguishable) from the impersonal (and supposedly objective), then there are also good reasons for not separating the two. The latter reasons deserve as much serious consideration as the former have received.

From the interviews it is abundantly clear that scientific ideas are not only tested against an elaborate background of prior theories and ideas but they are also tested against an elaborate network of prior social and personal relations. While theories, ideas, and evidence have an abstract and impersonal side, they also have a strong concrete and a personal dimension. In many cases this personal dimension actually overwhelms the impersonal. It certainly always influences it.

The interviews make abundantly clear that theories are associated with particular men who serve and are identified as the personal advocates of those theories. This is particularly the case all the more that the theories are bold, provocative, and all encompassing. To disprove, to falsify a theory is not merely to discredit an abstract entity but it is also to discredit the idea and position of a scientist who is personally associated with that idea and who has a stake in it. It was clearly recognized by the respondents that more often than not scientists were ardent, passionate, partisan advocates for their personal theories and that scientists did everything in their power (excluding cheating) to muster every bit of evidence favorable to the theory that they could find. They were not out to falsify their own theories but to confirm them. If they were out to falsify anything, it was the theories of their opponents.

The foregoing do not merely represent my own conclusions and interpretations of the interview materials but they also represent the perceptions of many of the scientists themselves. Indeed the recognition of the preceding was so strong that time and time again virtually the entire sample of over 40 scientists laughed at, and in the most derisive terms, the notion of the dispassionate, unbiased, objective observer of Nature. Not only was it recognized that in point of actual conduct the scientist was more often than not highly committed to a point of view--at the very least to his pet theories and hypotheses--but even more interesting and important, strong reasons were evinced why this ought to be the case, i.e., that ideally scientists ought not to be without strong prior commitments. Now I am perfectly willing to grant that many of these reasons are nothing more than outright rationalizations. Nevertheless even as rationalizations they are still worth considering, in fact all the more that they are, since they then represent some of the functional norms and operating rules of practicing scientists. Further they give one reason to pause and consider other models of science which are not dependent on the presumption of untainted, unbiased observers in order for scientific knowledge to result. Because of the importance of these points, it is worth our while to present some representative excerpts from the interviews:

Scientist A - "Bias has a role to play in science and it serves it well. Part of the business [of science] is to sift the evidence and to come to the right conclusion, and to do this you must have people who argue for both sides of the evidence. This is the only way in which we can straighten the situation out. I wouldn't like scientists to be without bias since a lot of the sides of the argument would never be presented. We must be emotionally committed to the things we do energetically. No one is able to do anything with liberal energy if there is no emotion connected with it."

Scientist B - "The uninvolved, unemotional scientist is just as much a fiction as the mad scientist who will destroy the world for knowledge. Most of the scientists I know have theories and are looking for data to support them; they're not sorting impersonally through the data looking for a theory to fit the data."

"You've got to make a clear distinction between not being objective and cheating. A good scientist will not be above changing his theory if he gets a preponderance of evidence that doesn't support it, but basically he's looking to defend it."

"Without commitment one wouldn't have the energy, the drive to press forward sometimes against extremely difficult odds. Trying to collect data on the moon is not the easiest thing in the world [this may be the biggest understatement of the entire study]. There are not only physical problems but there are bureaucratic problems as well to fight."

"You don't consciously falsify evidence in science but you put less priority on a piece of data that goes against you. No reputable scientist does this consciously but you do it subconsciously."

"The experimentalist doesn't have to be committed to a theory to do good work, but the theorist does, so the experimentalists can call the theorists metaphysicians. Lunar physics is not typical of science; there have been more theorists than experimentalists. It's a field dominated by theorists; until 2 years ago it was impossible to do experimental work on the moon."

Scientist C - "The disinterested scientist is a myth. Even if there were such a being, he probably wouldn't be worth much as a scientist. I still think you can be objective in spite of having strong interests and bias."

"If you make neutral statements, nobody really listens to you. You have to stick your neck out. The statements you make in public are actually stronger than you believe in. You have to get people to remember that you represent a point of view even if for you it's just a possibility."

"It takes commitment to be a scientist. One thing that spurs a scientist on is competition, warding off attacks against what you've published."

Scientists D - "In order to be heard you have to overcommit yourself. There's so much stuff if you don't speak out you won't get heard but you can't be too outrageous or you'll get labeled as a crackpot; you have to be just outrageous enough. If you have an idea, you have to pursue it as hard as you can. You have to ride a horse to the end of the road."[1]

Scientist E - "The notion of the disinterested scientist is really a myth that deserves to be put to rest. Those scientists who are committed to the myth have an intensity of commitment which belies the myth."

"Those scientists who are the movers are not indifferent. One has to be deeply involved in order to do good work. There <u>is</u> the danger that the bolder the scientist is with regard to the nature of his ideas, the more likely he is to become strongly committed to his ideas."

"I don't think we have good science because we have adversaries but that it is in the attempt to follow the creed and the ritual of scientific method that the scientist finds himself unconsciously thrust in the role of an adversary."

It should be noted that the interview materials do not entirely rule out a falsificationist interpretation of science. After all these scientists like their colleagues elsewhere do test their ideas. However the materials do not support the kind of falsificationism which depends on the necessity of positing a firm distinction between the context of discovery and the context of justification [37]. The contexts of discovery and of justification are at least bound together through the intense emotional commitment that it requires of a scientist first to discover his ideas and then subsequently to pursue their testing. In other words, I think the process of testing or justification is intimately bound to the process of discovery through the intense psychological emotions which are needed to keep the whole process of scientific inquiry alive. As I read the excerpts and the broader materials uncovered by the study I am impressed by the fact that science advances not through the sole efforts of individual men each "dispassionately" and "logically" testing their own ideas but through a heated adversary process wherein one man tests "his" discoveries against the discoveries of "another." The discoveries of one man are passionately thrust against the discoveries of another man in order to "test" both. Psychological energy and commitment infuses the whole process to such a degree that it is silly in my opinion to say that schientific inquiry naturally exhibits a clear-cut dividing line between either individual scientists or the contexts of discovery and of justification. In this sense the distinction between discovery and justification actually embodies a naive social psychology of science. To remove commitment and

and even bias from scientific inquiry may be to remove one of the strongest sustaining forces for both the discovery of scientific ideas and for their subsequent testing.

This is not to deny that this conception of science raises as many problems as it promises to "solve". For example, it is unfortunately beyond the scope of this paper [33] to demonstrate in detail how scientific objectivity could still emerge from such a system. In a number of previous papers [31,35], I have tried to lay the foundation for a combined philosophical/behavioral theory of science by outlining various epistemological and behavioral mechanisms whereby a consensually based notion of scientific objectivity could still result eventhough the starting point was an initial state of strong conflict, i.e., strongly contending adversary positions. The goal (as well as the problem) or such a conception is to show how objectivity is possible not by excluding strong emotions from science (which is a psychological impossibility) but to show how objectivity is possible <u>because</u> of them, not in spite of them. As Churchman and Ackoff have put it:

> Pragmatism does not advocate a scientist who removes all his emotions, sympathies, and the like from his experimental process. This is like asking the scientist to give up being a whole man while he experiments. Perhaps a man's emotion will be the most powerful instrument he has at his disposal in reaching a conclusion. The main task, however, is to enlarge the scope of the scientific model so that we can begin to understand the role of the other types of experience in reaching decisions, and can see how they too can be checked and controlled. The moral, according to the pragmatist, should not be to exclude feeling from scientific method, but to include it in the sense of understanding it better [7, pp. 224].

Toward A More Precise Explication Of the Differences Between the 'Great' and the 'Normal' Scientist

Very early on during the first round of interviews, a number of the scientists, independently of one another and of their own accord, suggested a typology of different kinds of scientists. In their opinion, their experience suggested that there were very sharp differences between a relatively small number of fundamentally differing kinds of scientists. Indeed, on presenting the typology to the rest of the sample for their reaction and evaluation, a remarkable degree of consensus was obtained as to , first, the validity of the typology, and then, second, the names of those specific scientists who "most typified" each of the various "types." The precision of the typology was continually refined on subsequent interview rounds. In addition, the perceptions of those few scientists who were most frequently nominated as best typifying the various types were systematically measured. Each of the 40 scientists in the study reported their perceptions of these few scientists in terms of a variety of attitudinal scales.

Although the differences between the various "types" cannot be reduced to a single or simple underlying dimension, the most fundamental dimension and the one which originally suggested the typology was that of "speculativeness" or "willingness to extrapolate beyond the available data." In any social grouping, there are always those who prefer to stay close to "the facts" and those who prefer to venture out beyond them even at the risk of ignoring them. Type I scientists, as they were referred to, were distinguished by the essential defining quality that they excelled at extrapolating from data. Although they were often fine, detailed experimenters themselves and even at times enjoyed experimental results or numbers for their own sake, theorizing was obviously their most pleasurable and exalted task. Type I scientists were willing and even relished the bold intuitive and theoretical leaps always required in making inferences from incomplete data to a comprehensive and encompassing theory. Type III scientists, as they were referred to, represented the other end of the spectrum. Here, numbers are often relished primarily for their own sake. There is a preoccupation, even an obsession, with data gathering. There is often an extreme disdain of theorists who deal with highly inferrential and abstract concepts. Speculation or extrapolation from data is valued little and only engaged in when the data clearly warrants such extrapolation and then only with extreme cautiousness. Where type I's more readily tended to see the positive advantages of speculation in science and to speak glowingly of it, type III's tended to disparage it. They tended to equate speculation with wild theorizing and refer to it as "finger-painting in the sky."

Type III scientists are often seen as brilliant but extremely narrow and specialized experimenters. In some instances, they are regarded as nothing more than "super-technicians with a Ph.D." (In fairness, it was noted that theorists can often be just as narrow. In general though, it was the consensus opinion of the sample that it was much more difficult for a theorist to be a narrow specialist than it was for an experimenter. Theorizing on something so broad as the origin of the moon requires, by its very nature, that a scientist be familiar with, if not competent in, several diverse scientific fields.) Type II scientists represented something in between. Here were to be found scientists who were equally capable of doing competent experimental work as well as engaging in modest theorizing and extrapolating activities. From time to time, they could even rise to bold feats of theorizing and extrapolation, but in general, they represented the middle ground, running to neither of the extremes represented by Types I and III.

Here, as with every topic investigated throughout the study, it proved impossible to keep the exploration and assessment of a supposedly abstract quality like that of speculation or the notion of different types of scientists apart from a discussion of the personalities of specific individuals. Indeed, the scientists discussed abstract issues in terms of concrete specifics. Their comments were constantly peppered with highly intense and even slanderous jibes at their colleagues. Although as a general rule most of the scientists seemed to appreciate that a wide variety of different types and styles were necessary for the advance of science [24], there were strong exceptions. There were more than just isolated pockets of scientists who would be more than happy to see certain of their colleagues disappear from science altogether, particularly those scientists identified as the most extreme of the type I's.

The original typology of three types was refined in two ways: 1) each of the I, II, and III types was further subdivided; the final typology thus contained six types instead of three; and 2) the number of dimensions underlying the typology was expanded; this allowed the differences between the types to be drawn more sharply and generally. While both of these refinements were important, only the latter need concern us here. Table 1 gives the results of one of the many exercises that were used to measure more precisely the differences between type I, II, and III scientists.

TABLE 1: ADJECTIVE PROFILES FOR THE DIFFERENT TYPES OF SCIENTISTS

Scale #	TYPES S_I^3	S_I^2	S_I^1	S_{II}	Y	S_{III}
Biased-Impartial 1	Markedly BIASED	Markedly BIASED	Moderately BIASED	Neither or Both IMPARTIAL-BIASED	Moderately IMPARTIAL	Neither or Both IMPARTIAL-BIASED
Brilliant-Dull 2	Extremely BRILLIANT	Markedly BRILLIANT	Markedly BRILLIANT	Markedly BRILLIANT	Neither BRILLIANT Nor DULL	Neither BRILLIANT Nor DULL
Theoretical-Practical 3	Markedly THEORETICAL	Markedly THEORETICAL	Moderately THEORETICAL	Both PRACTICAL And THEORETICAL	Both PRACTICAL And THEORETICAL	Moderately PRACTICAL
Generalist-Specialist 4	Markedly GENERALIST	Moderately GENERALIST	Markedly GENERALIST	Moderately GENERALIST	Moderately GENERALIST	Markedly SPECIALIST
Creative-Unimaginative 5	Extremely CREATIVE	Markedly CREATIVE	Markedly CREATIVE	Markedly CREATIVE	Moderately CREATIVE	Neither CREATIVE Nor UNIMAGINATIVE
Agressive-Retiring 6	Extremely AGGRESSIVE	Markedly AGGRESSIVE	Markedly AGGRESSIVE	Markedly AGGRESSIVE	Moderately AGGRESSIVE	Moderately AGGRESSIVE
Vague-Precise 7	Neither or Both VAGUE-PRECISE	Moderately PRECISE	Moderately PRECISE	Markedly PRECISE	Moderately PRECISE	Markedly PRECISE
Rigid-Flexible 8	Moderately RIGID	Moderately RIGID	Neither or Both FLEXIBLE-RIGID	Moderately FLEXIBLE	Markedly FLEXIBLE	Moderately RIGID
Theoretician-Experimentalist 9	Markedly THEORETICIAN	Markedly THEORETICIAN	Moderately THEORETICIAN	Both EXPERIMENTALIST-THEORETICIAN	Both EXPERIMENT-ALIST-THEORETICIAN	Markedly EXPERIMENTALIST
Speculative-Analytical 10	Markedly SPECULATIVE	Moderately SPECULATIVE	Moderately SPECULATIVE	Both SPECULATIVE-ANALYTICAL	Moderately SPECULATIVE	Markedly ANALYTICAL

KEY:
Extremely or Significantly = 1.0 to 1.5 or 6.5 to 7.0 values on SD scales
Markedly = 1.5 to 2.5 or 5.5 to 6.5 values on SD scales
Moderately = 2.5 to 3.5 or 4.5 to 5.5 values on SD scales
Neither or Both = 3.5 to 4.5 values on SD scales

SEE FOOTNOTE 2 FOR AN EXPLANATION

From the first set of interviews, five specific scientists were identified as significant representatives for each of the three types. The perceptions of each of these specific scientists by the 40 scientists were separately measured in terms of a Semantic Differential (SD) consisting of 10 scales[36].[2] The perceptions of three type I scientists, S_I^3, S_I^2, and S_I^1, were scaled; one type II scientist, S_{II}; one type III scientist, S_{III}; and finally, one general category Y which referred to the designation "Yourself;" i.e., in addition to scaling five other scientists, each of the 40 scientists scaled himself so that it could later be determined where the general body of scientists lay in relation to the specific representatives of the various types.

It should be noted that the decision to scale three type I scientists was made for very specific reasons. In response to the question, "What scientists are, in your opinion, most committed to their pet hypotheses or theories and as a result least likely to shift their commitment as a consequence of the Apollo data?," three scientists were consistently mentioned over and over again. These three scientists were also nominated as the most outstanding examples of type I scientists. More than any other group of scientists, these scientists excited the jealous envy and hostility of their peers. They also excited the most positive superlatives.[3] They not only dazzled their peers with their spectacular feats of speculative theorizing but they also offended them with the abrasive, provocative, and often agressive manner with which they presented and defended their theories. These reasons alone warranted comparing these three type I scientists among themselves as well as against the other types. In addition it was felt that it was important enough to get a collective portrait of those scientists who were perceived as most committed to their ideas to warrant the study of three type I's.

Table 1 shows that there is a marked and systematic difference between the perceptions of the types as we move across the table from left to right. That is, if take the means ($\bar{x}$) of the perceptions of each of the 5 specific scientists, including the general category "yourself," Y, and lay them out on 10 straight lines (one line for each of the 10 scales against which the types are being rated as indicated in Table 1) then S_I^3 is generally found at one extreme of the scales, S_{III} at the other. Furthermore, S_I^3, S_I^2, and S_I^1 bunch together; i.e., the differences between S_I^3, S_I^2, and S_I^1 are inconsequential when compared to the differences between themselves as a group and the other types of scientists. Above all it should be noted that the differences between types (S_I, S_{II}, Y, and S_{III}) are highly significant. We are not dealing with borderline or trivial differences. A one-way analysis of variance (ANOVA) reveals that the differences between the mean scores for the types are statistically significant at an α- level of less than 0.001, i.e. $\alpha \ll 0.001$. Furthermore, multidimensional scaling and correlational analyses [50] reveal that the ordering pattern, S_I^3, S_I^2, S_I^1, S_{II}, Y, S_{III}, is itself statistically significant. That is, <u>not only are the differences between the types significant but so is the relative placement (order) of the types with respect to one another</u>.

The significance of these results is as follows: (1) they add needed support to the validity of the original typology, and (2), they give us some interesting insights regarding the psychological differences between the Great and the Normal scientist [ ]. With regard to the first point, if the specific scientists, S_I^3, S_I^2, S_I^1, S_{II}, and S_{III}, are really representative of the various types then it is comforting that they at least fell on certain of the scales according to prior expectations. For example, according to the original definitions of the types one would expect that S_I^3, S_I^2, and S_I^1 would tend to fall toward the biased end of the Biased-Impartial scale, S_{II} in the middle, and S_{III} toward the impartial end. (S_{III} is the only scientist which does not fall as expected on this scale.) The placements follow similarly on scales 3, 9, and 10. Scales 9 and 10 are particularly crucial since they literally feed back the original dimensions on which the typology was initially proposed. It would have been particularly distressing if the placements on these scales turned out to be anything other than as expected. Of course this by itself does not establish either the uniqueness or the ultimate correctness of the typology but it is at least comforting that it has survived the most elemental tests of it. The full report [33] of this study presents an extensive number of validating exercises. The typology survives every one of these critical tests.

With regard to the second point, the differences between the three type I scientists and the single type III scientist are most instructive. In order to appreciate the significance of these differences it is necessary to point out that throughout the study S_{III} was the particular scientist who was judged most representative of the average or typical scientist in the lunar program. (It should be emphasized in this regard that throughout the study the scientists talked extremely freely about one another. There were as a result many opportunities to obtain consistent portraits of the various scientists. That is, the descriptions and inferences with respect to the types are not based on isolated, infrequent observations.) The three type I scientists on the other hand were the ones who were most frequently judged the outstanding, extraordinary scientists of the program, the scientists who most consistently stood at the creative apex of the profession. It is thus both interesting and important to compare the adjective profiles between these two rather distinct types, I and III.

If the type I scientists most nearly correspond to Kuhn's Great scientist (as I believe they do), and if the type III scientist most nearly corresponds to the Normal scientist, then the results imply that the Great scientist is more likely to be more creative in the production of bold and speculative ideas. They are also most likely to be the kinds of scientists who become most rigidly committed to their ideas. That is, the three type I's were perceived as extremely creative in the sense of their being able to produce and having produced many original innovative ideas over a long period of time and in this sense they were regarded as extremely flexible. They possessed the requisite mental agility and nimbleness of mind to see old problems in a new light and to perceive (literally invent) highly imaginative patterns in a complex sets of data; thus, the judgements of "brilliant," "theoretical," "generalist," "speculative," and the tendency towards "vagueness." On the other hand, they were perceived as extremely attached to their ideas once they were produced; thus, the strong judgements of "bias," "agressiveness," and "rigidity." Indeed, independent exercises [33] establish that over a span of three years, there is virtually no perceived shift (according to the 40 scientists) in the positions of each of the three type I scientists with respect to certain scientific hypotheses with which they have long been associated. This point cannot be overemphasized. Although the psychological differences are interesting and important for their own sake, they are even more important for what they imply for the understanding of the growth and change of scientific ideas. While it is beyond the scope of this paper to demonstrate how these psychological differences operate in detail, it can be shown [33] what kinds of rationalizations and rational arguments that different types use to hang onto, preserve, as well as, change their ideas. In other words, under certain circumstances, it is possible to relate explicitly differences in psychology to the growth of a scientist's ideas.

The Bearing of the Social Psychology of Research on the Orthodox View of Scientific Theories, Kuhn, and Feyerabend: Modifications and Revisions

At some point or another every philosophical view of science is critically dependent upon the presumption of an observing scientist. More specifically, every philosophical view of science makes certain assumptions about the nature of scientific observations and/or observables and their relationship to a scientific observer. In the case of the orthodox or received view of scientific theories [13,47], the assumptions take the form of assuming that there exists a class of certain entities distinguished by their capability of being "directly observed" by a human observer through his "direct sensory apparatus." It is also assumed that there exist certain theoretical entities which while they cannot be directly observed can be sharply differentiated from the observational entities. It is further assumed that while the theoretical entities are, because of their hypothetical and inferrential status, problematic, the observational entities are, by virtue of their ability to be "directly observed," nonproblematic.

In a previous paper [34] I have argued that the notion of direct observation or immediate apprehension is contradicted on every count by an overwhelming body of behavioral evidence. With respect to this evidence, the notion of direct observation is nonsensical. All the behavioral evidence I am aware of suggests that all observation on the part of anything resembling a human observer is mediated by the observer's entire past behavioral and physical history, his current emotional state, feelings, and future aspirations. In a word, all observations are observer dependent, dependent upon the complicated and highly partial mental states of some observer. Observations are thus not neutral because observers are not neutral with respect to what they observe. Indeed for the act of observation to even occur, it is necessary that the observer bring with him some initial set of presuppositions towards what he expects and even desires to see. On two counts then, observations are not theory free, and hence, are anything but "direct". First, because in order to assess what any particular observer contributes to what he observes we require a general behavioral and physical theory of observation which relative to the class of all other observers allows us to relate and compare the reports of different observers.[4] Second, in order to take any specific scientific observation we require some theory or model, no matter how crude or unarticulated (and not necessarily unique), of the event or process we wish to observe, particularly all the more that our observations take place through complicated instrumentation [8,47]. The results of this study [33] more than support these assertions. In a word, radically different observers or types of scientists bring radically different types of presuppositions with them to the field of scientific observation. Hopefully the results of this study contribute to the eventual establishment of a behavioral theory of scientific observation [8] that will allow us eventually to compare the reports of different scientific observers. On a more abstract level, the results of this study support a "presupposition-dependent view of scientific theories." [5]

As important as it is to raise severe challenges to the orthodox view of scientific theories, an even more important task is that of pointing out the modifications that the results of this study suggest to the views of Kuhn and of Feyerabend. To recall briefly the views of both: Kuhn sees science marked by long periods of normal development (Normal Science) broken by occasional severe crises, or in his terms, "scientific revolutions." During a period of Normal Science scientists largely work within an accepted paradigm, an accepted largely implicit world-view. The accepted framework or paradigm provides relatively clear-cut guidelines for all the facets of scientific activity: it specifies the problems that will be worked on, the methods that will be used in working on them, the basic vocabulary that will be used for describing (recognizing) scientific problems, and above all a common language, process, or medium for resolving the inevitable disagreements or disputes that arise between scientists, data, theories. For the most part, most scientists spend their life working within Normal Science, i. e., extending and working out the problems within the current paradigms; very few scientists are either prepared to work on or actually do work within "extraordinary science, i.e., contribute to the overthrow of an accepted paradigm. For the most part, scientific training is training for Normal Science. There are times however when the current paradigms of Normal Science are challenged, threatened, and even more rarely, overthrown completely by a new paradigm which in turn becomes the paradigm of the new period of Normal Science. Depending on how complete the revolution is, and apparently for Kuhn they are rather complete, the disjuncture between the old and new paradigms becomes severe. Old and new paradigm proponents are unable to communicate across the gap; theoretical terms no longer possess the same meanings; they take on the meanings of the paradigms with which they are associated. As Toulmin has put it in characterizing Kuhn's views, "a scientific revolution involves a complete change of intellectual clothes [51 , p. 101]."

Feyerabend as well as Kuhn emphasizes the radical disjuncture between competing scientific theories. However they argue it and reach it via very different pathways. Kuhn's pathways are historical and social psychological. He proposes radical disjuncture (or as it has been called by some [23], "radical meaning variance theory") as a way to account for the fact of the growth and change in scientific theories, that science grows--contrary to popular myth--not by the patient and step by step accumulation of facts but by radical leaps and bounds. Feyerabend on the other hand [14, 15, 16, 17] proposes radical disjuncture as a methodological principle, as a way of doing good philosophy of science: scientists should always strive to produce the most radically differing counter-theories to the theories currently in existence. If all data are dependent upon the use or presumption of some theory for their production, and as is so often the case, the only theory we have available is the theory we wish to test, then the data produced through the use of that theory will in general constitute far less than the most powerful test of that theory. The test data will certainly not constitute an independent test of the theory [35]. Faced with this circumstance, the only way we can provide the strongest test of our theories is to test them against data uncovered by some counter-theory. The severest tests will be provided by the data that are derived from the theories that are in sharpest disagreement with one another.

Needless to say both authors have come in for some severe criticism. For example, both Kuhn and Feyerabend have been accused of over-emphasizing, the discontinuities, the disagreements between successive phases of the course of scientific development. They have been accused of placing a greater emphasis on the conflict between competing theories, paradigms, and scientists than actually exists or can be shown to exist. In short they have been accused of viewing the successive phases between competing theories or paradigms as an all or nothing phenomenom, i. e., as either perfect agreement or perfect disagreement. As Shapere has put it:

> ...Kuhn has committed the mistake of thinking that there are only two alternatives: absolute identity or absolute difference. But the data at hand are the similarities and differences; and why should these not be enough to enable us to talk about more, and less, similar views and, for certain purposes, to classify sufficiently similar viewpoints together as, e.g., being in the same tradition? After all, disagreements, proliferation of competing alternatives, debate over fundamentals, both substantive and methodological, are all more or less present throughout the development of science; and there are always guiding elements which are more or less common,

> even among what are classified as different 'traditions.' By hardening the notion of a 'scientific tradition' into a hidden unit, Kuhn is thus forced by a purely conceptual point to ignore many important differences between scientific activities classified as being of the same tradition, as well as important continuities between successive traditions. This is the same type of excess into which Feyerabend forced himself through his conception of 'theory' and 'meaning.' Everything that is of positive value in the viewpoint of these writers, and much that is excluded by the logic of their errors, can be kept if we take account of these points [45, p. 71].

Both Kuhn and Feyerabend have also been criticized on the grounds that their views lead to relativism, that if each theory or paradigm defines its own terms, and that if the disjuncture between strongly competing theories is complete, then how can we compare theories? How can we say that one theory is better than another and on what grounds are we justified in saying that progress has occurred just because one theory has replaced another? Further if each theory supplied its own criteria, then because of radical disjuncture again, how can we even compare criteria let alone theories. And even further, if the terms differ so radically from one theory to another, how can the data unearthed by one theory be used to test another theory? If data only have meaning with reference to their specific background theory, and if two theories differ radically in meaning, how can the data from one theory have meaning with respect to another theory and hence be used to test it?

While I consider myself basically in sympathy with the views of Kuhn and Feyerabend, I recognize the validity of many of the arguments of their critics. Even more the data from my own study allows me to support a number of their critics points with empirical evidence as well as with theoretical arguments. And yet their critics are not all right either. There is a sense in which Kuhn and Feyerabend are right and a sense in which they are wrong, or better yet, incomplete. If, as their critics have pointed out it is not an "all or nothing" choice between competing paradigms or theories, then it is not an "all or nothing" choice between Kuhn and Feyerabend. There is a point in between.

The central issue _is_ that of "agreement." Kuhn is right when he emphasizes the tremendous conflicts and disagreements that exist between competing paradigms, theories, and scientists. The data of this study more than supports him on this point. He is even right when he emphasizes that there are occasions for which there are theories, paradigms, and scientists which, for all practical purposes, may be regarded as in total disagreement. However, Kuhn is wrong when he either fails or neglects to point out that what is true on the _micro_ level is not necessarily true on the _macro_ level. That is, there are _individual_ scientists who for all practical purposes may be regarded as in extreme disagreement with one another, and indeed the results of this study would make it seem that there are issues on which all scientists disagree some and maybe even all of the time. But likewise there are also _individual_ scientists who for all practical purposes may be regarded as in complete agreement with one another. Since both of these forces or tendencies operate in every system, it would seem false to characterize a _system_ (a collectivity of scientists) as existing in either a state of total or near total disagreement or agreement.

There is a more telling way to put this point. It seems to me that the data of this study suggests that in every social system there are those kinds of individuals ("types") who have a compulsive need to make revolutions, to disagree _as strongly as possible_ with established ways of thinking--paradigms if one prefers to call them. These individuals have an almost consuming need to produce radical counter ideas and theories to those currently in existence if not in vogue. They seem to need to go out of their way to produce extremely novel ways of looking at old phenomena. However the data also suggests that there are also those kinds of individuals who have a compulsive (security) need to preserve continuity with old established ways of thinking, to differ as little as possible from the tried and the true.

Rather than complete disagreement or agreement being an actual state of affairs, they are instead "states of Mind," attitudinal Ideals [9,35], or in Toulmin's terms [51] the divergent "disciplinary aims" of radically distinct types of men. There are, in other words, scientists who act to bring about "revolution in perpetuity" but there are also those who act to preserve the status quo, to bring about "continuity in perpetuity." Even more so, there is a large body of scientists in between who are blends of these two extremes and much more besides. To say then that after a scientific revolution, there is complete lack of communication between the two sides is to distort the situation. It is a partial truth at best in the sense that there are at any one point in time--before, during, and after a revolution--those scientists who are unable to communicate across any one of a hundred different gaps--theories, attitudes, issues. But there are also those kinds of individual scientists or forces acting within the system whose Ideal is to discover links between the past and the present. It is too much then to say that after a revolution there is a complete breakdown between the two sides. For one this assumes a far greater degree of cohesiveness and homogenity between "sides", groups, or schools of thought than is warranted. I see as much diversity within the system or Game of Science at any one point in time--even within supposedly tight common factions--as between successive time periods. In other words, as much as there are strong forces ("revolutionaries") acting to produce drastic changes in The Game at any one point in time, there are also just as strong forces acting to keep The Game the same as it has always been ("reactionaries"), and further yet, there are those ("moderates") acting to steer a course in between, to usher in the new while acting to preserve its links with the past.

There is even a stronger way yet to put these points and one that brings Feyerabend into the discussion. Michael Martin [27,28] for one has raised a particularly interesting series of questions and challenges to Feyerabend's position. Although it is not exclusively associated with Feyerabend, Martin subsumes Feyerabend's position under the general heading of Extreme Theoretical Pluralism (ETP) which Martin characterizes as follows:

> ETP: Develop as many alternative research programs [theories] as possible at all stages of development [of a science] whether the prevailing research program is progressive or not [28, p. 5]!

As a strong alternative to ETP, Martin lists another position which he labels Theoretical Monism (TM):

> TM: Do not develop alternative research programs in any science at any stage of development if the prevailing research program is progressive [28, p. 5]!

There are of course as Martin notes an infinite number of other positions in between, e.g., for a particular set of (some) circumstances in the historical development of a science, develop some (not necessarily "as many") alternative research programs whether the prevailing research program is progressive or not. As Martin notes, the interesting question is, which program is "best" for which science at which stage in its development?

There are a number of important implications that follow from Martin's treatment of the issue: One, whether ETP, TM, or some other alternative actually holds or not is not merely a matter of "mere empirical" historical research. It would be extremely difficult for any finite set of observations on so complicated a phenomenon as science to establish a general rule like ETP or TM. Two, thus, although rules like ETP or TM can be empirically researched, e.g., for the grounds of their empirical applicability, they are not solely or merely empirical propositions or hypotheses to be empirically confirmed or rejected. In a more fundamental sense, ETP and TM express the basic metaphysical or intellectual commitments of their proponents about the nature of science. If anything, they are maxims or commands--directives--("develop," "do not develop") to view science in a very particular way prior to the gathering of empirical data on it. Rather than following from empirical observation and confirmation, they are instead prior to it. In this sense it is not a question of Feyerabend's being "right" or "wrong" but that his position merely expresses one stance out of the range of possible stances towards the nature of science (i.e., towards studying it, characterizing it, etc.).

While granting this, to me one of the most interesting implications of the data of this study is that it suggests a novel way of studying various stances towards the nature of science. If one takes this study to its utmost limits then one can assert that ETP, TM, and all the other alternatives in between are not just abstract intellectual possibilities but they are also psychological realities. For every abstract philosophical position that can be formulated with respect to the nature of science, there exists a corresponding "real" scientist whose behavior and attitudes embodies that abstract position. Following this line of thought further, one way then to study the implications of a proposed abstract position is to trace through its effects via a postulated, corresponding human embodiment of that or any other position; i.e., study the attitudes of those who embody that position.

In sum, I think Kuhn was right in his basic appeal to social psychology and history as one way of doing philosophy of science. Where Kuhn went wrong in my opinion was in his detailed application of social psychology. In fact it can be said that Kuhn wasn't social psychological enough to be able to give a good accounting of the workings of scientists. For instance, Kuhn accords far too passive a role to individual scientists in bringing about a revolution [11]. As I have stressed, there are certain very specific kinds of scientists who actually seek to promote revolution.[6]

Likewise it can be said that for one who espouses radical or Extreme Theoretical Pluralism, Feyerabend wasn't radical enough in envisioning alternatives to his alternatives, how his ideas might be tested by other radical alternatives such as those outlined by Martin. In a way however these are merely minor complaints. Kuhn and Feyerabend still deserve much credit for opening up some needed and new pathways in the philosophy of science.

Concluding Remarks

Forty or so scientists is obviously a rather small sample size on which to base such weighty generalizations about the nature of science. If as the cliche goes--and it is a cliche[7]--that "90% of all scientists who have ever lived are alive today," how can one found such strong generalizations on so small a sample? If the only legitimate basis for extrapolation or generalization is the size of one's sample, then the answer quite properly is that "one can't." However I believe that there are other legitimate reasons for extrapolation. If only for no other reason than to encourage and to challenge others to do similar interdisciplinary studies of science, I believe the results of this study should be generalized as far as possible. Philosophers of science do not hesitate for one moment to make all kinds of universal statements about the nature of science based on no empirical data at all, or worse yet, on the empirical data of what in their imagination they construe as the behavior of scientists. If philosophers are justified in doing this based on no data, effectively on a statistical sample size of zero scientists, then I should like to extrapolate and generalize based on a sample size of slightly better than forty.[8]

A word also needs to be said again about the potential charges of psychologism. The study on which this paper is based is admittedly heavily psychological. However it is not in my mind to be labeled psychologistic because I do not believe in psychologism as a philosophy. I have not been out to reduce every matter and concern of science to psychology. I don't believe that this can be done. Even stronger I don't believe this should be done. I do believe however that standing behind <u>every</u> aspect of science there can be shown to be operating exceedingly deep and personal elements. However I also believe that a set of rational rules and public procedures also enfuses the entire structure. In this sense this study is not out to replace the logic of science but rather to question whether we can have an effective understanding of how the logical operates in science independently of the social psychological. In short I have been out to build the strongest case possible for psychology, not to make psychology the sole basis for studying or characterizing science. To replace the "logic of science" tradition entirely by the "social psychology of science" tradition would in my opinion be no more than to replace one reductionism with another. If in spite of this I have still been guilty of psychologism, then all I can say is that much of our philosophy of science has been guilty of "logicism."[51] If so, then this study is a needed counter to that overemphasis.

In sum, it should not be the case that because an enterprise involves the gathering of empirical evidence it is therefore to be regarded as "merely empirical." Rather, it is the intent and the spirit with which the data is gathered that makes the enterprise merely empirical or not. The spirit with which many social psychologists have studied science, their lack of understanding and appreciation for philosophical issues, and finally, their inability to draw the fullest implications of their date--all of these have tended to make the social psychology of research merely empirical in the past. I hope that I have demonstrated that this need not be a necessary and universal characteristic of all applications of the social psychology of research. We need to experiment with even more ways of combining seemingly and traditionally diverse fields of inquiry so that we can break down the silly barriers that so often separate disciplines and lead us into believing that this or that aspect of science is the unique province of this or that discipline to study. As Russell Ackoff has put it, "Nature is not organized in the same way that universities are [1]." By not raising perpetual challenges to the organization of our disciplines, we ensure our ignorance of Nature and of our science.

FOOTNOTES

1. This statement is interesting for the reason that it corresponds almost exactly with Murray Davis's [11] fascinating notions of what makes a theory "interesting." That is, if a theory is to be interesting, it must differ substantially from our ordinary sense expectations but not too much or "you'll get labeled as a crackpot." For an extended discussion, see [11].

2. A Semantic Differential is a social science technique for obtaining a semantic profile of the "meaning" of a concept or set of concepts to a set of respondents. The concept whose meaning is to be determined is placed over a pre-determined number of scales. Each scale is bounded by a contrary pair of adjectives. Thus, for example, the first scale in Table 1 is Biased-Impartial. Each scale has the numbers 1 through 7 on it. If a respondent feels that for the particular concept he is rating that its meaning is best captured by one of the polar adjectives then he checks the numbers "1" or "7" For example, if a respondent felt that a particular specific scientist was at the extreme Biased end of the scale, he would check "1;" if at the extreme Impartial end, he'd check "7;" he'd check "4" if he felt the scientist was neither Biased nor Impartial, if he was undecided, or indifferent to the scale. If a respondent felt a scientist leaned toward one end of the scale or the other but not the extreme, he'd check a number in between, e.g., "3." The entries in Table 1 give which adjective of the adjective pairs the average ($\bar{x}$) of the respondents numerical values for each SD scale most nearly correspond. A Semantic Profile is typically constructed for each concept being rated by connecting the average values for each scale. More sophisticated uses of the technique allow one to compare concepts by means of more complex statistical analyses, e.g., factor analysis [36].

3. Throughout the study, no matter what the topic that was being discussed, equally strong and opposing opinions were expressed on both sides of it. Again it would seem that it is only in idealized accounts, or as Kuhn has so rightly stressed [25], it is only in idealized "after the fact" textbooks that we ever find scientists in complete agreement on any issue, scientific as well as non-scientific. Of all the divergent opinions, the strongest were expressed with respect to the perceptions of the three type I's; for example:

<u>Scientist A on C</u>

"C is an out and out crackpot; I no longer regard him as a scientist. He does more harm to science than he does good. Science would be better off without him."

<u>Scientist B on C</u>

"Of course C is a scientist. It is valuable for science to have people like C. The system would be the worse off without him."

<u>Scientist G on C, D, E</u>

"C, D, and E are examples of the lunatic fringe."

<u>Scientist H on C, D, E</u>

"C, D, and E make people extremely mad but they also spur them on. They are the creative vanguard of the profession."

4. Consider Churchman on this point:

> The third level [of standardization of data] consists of adjusting all or almost all data to standards by means of "laws" that enable one to say: <u>if</u> report R_1 was made at time t_1 in circumstance z_1 by a person having properties w_{11}, w_{12}, and so on, then report R_0 would have been made at time t_0 in circumstance z_0 by a person having properties w_{01}, w_{02}, and so on. The last level most clearly approximates the function of measurement, because it permits the broadest use of the information [8 , p. 121].

5. Without going into the full range and kinds of questions that had to do with the bearing of philosophical and methodological issues on science that were raised with the respondents, it can be said that the scientists were extremely sensitive to the particular issue of the theory-ladenness of observations. They repeatedly stressed the point that it was impossible to make a single observation in science without the presumption of <u>some</u> theory (not necessarily unique). As one might suspect, the sophistication of the respondents with respect to philosophical issues was rather confined. They were most sophisticated with respect to issues that bore most directly on their everyday concerns (e.g., the relation of theory to data) and least sophisticated with respect to issues that were farthest away (e.g., whether it was necessary for science to make metaphysical presuppositions. It should be noted in this regard that the type I scientists tended to be a bit more sophisticated than the rest of their colleagues with respect to the deeper philosophical issues.

6. In another sense it can be said that Kuhn forgot to incorporate one of his earlier papers which contained precisely the psychology he needed. I refer to Kuhn's paper, "Essential Tension: Tradition and Innovation in Scientific Research" [24], in which he discussed that the progress (advancement) of science depended on a tug-of-war between two fundamental types: the innovator and the traditionalist. The one was dedicated to breaking new ground; the other, to preserving the past.

7. It is a cliche because when wasn't there ever a time that this couldn't be said? Surely there were scientists before the term was invented. The official term is only a couple of hundred years old at best anyway. If we define a scientist as one who embodies a certain set of psychological attributes, as one who views Nature in certain prescribed ways, then there is no reason to believe that we have not always had scientists with us at least as far back as the human race began to develop differentiated personality structures.

8. As an aside I find it amusing to note that in the few times I have talked about this study before public groups, there are always those few scientists in the audience who are insistent that if I had studied such and such a group or their particular brand of science (discipline), I wouldn't have found such outrageous behavior. What's so amusing is

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that each group of scientists is so willing to believe that the other group is outrageous, not themselves of course!

Nevertheless I would be the first to admit that the results of this study might not apply in their entirety to every group of scientists, but only in the sense, that not every group might exhibit the effects reported here with the same strength and degree of clarity. Thus, for example, while I admit that the particular group I studied may be more aggressive than the "average," I would still expect every scientific group to exhibit noticeable degrees of aggressiveness because of what's known from the prior literature on the psychology of scientists [12, 30, 52]. Obviously though this study should be replicated across different disciplines, cultures, laboratories, universities, etc. I hope my results encourage others to do this.

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