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NORMS AND COUNTER-NORMS IN A SELECT GROUP OF THE APOLLO MOON SCIENTISTS: A CASE STUDY OF THE AMBIVALENCE OF SCIENTISTS*

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This paper describes a three and a half year study conducted over the course of the Apollo lunar missions with forty-two of the most prestigious scientists who studied the lunar rocks. The paper supports the Merton-E. Barber concept of sociological ambivalence, that social institutions reflect potentially conflicting sets of norms. The paper offers a set of counter-norms for science, arguing that if the norm of universalism is rooted in the impersonal character of science, an opposing counter-norm is rooted in the personal character of science. The paper also argues that not only is sociological ambivalence a characteristic of science, but it seems necessary for the existence and ultimate rationality of science.

INTRODUCTION

T he sociology of science owes a debt to Robert Merton for his many substantive contributions and for his continual suggestion of important, unsolved problems. This paper addresses one of these problems.

This paper considers three aspects of Merton's work: (1) his earliest (1949) attempts to codify the norms of science; (2) his later ideas (1957,1963a,1963b,1969) regarding the norms of science; and (3) his developing ideas about the nature of social or institutional norms and the concept of sociological ambivalence (summarized in Merton and E. Barber, 1963). We begin with the last, the notion of sociological ambivalence. Consider the following:

[We must] consider, first, how potentially contradictory norms develop in every social institution; next, how in the institution of science conflicting norms generate marked ambivalence in the lives of scientists; and finally, how this ambivalence affects the actual, as distinct from the supposed, relations between men of science (Merton, 1963a:80).

From the standpoint of sociological ambivalence..., the structure of [for example] the physician's role [consists] of a dynamic alternation of norms and counter-norms. These norms call for potentially contradictory attitudes and behaviors. For the social definitions of this role [the physician's], as of social roles generally, in terms of dominant attributes alone would not be flexible enough to provide for the endlessly varying contingencies of social relations. Behavior oriented wholly to the dominant norms would defeat the functional objectives of the role. Instead, role behavior is alternatively oriented to dominant norms and to subsidiary counter-norms in the role. This alternation of subroles evolves [italics in original] as a social device for helping men in designated statuses to cope with the contingencies they face in trying to fulfill their functions. This is lost to view when social roles are analyzed only in terms of their major attributes (Merton and E. Barber, 1963:104, major italics added).

Starting in 1942 from a conception of a single dominant set of norms (1949), Merton has come to perceive science as reflecting

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conflicting sets of norms. While Merton and others (B. Barber, 1952; Hagstrom, 1956; Storer, 1966), have tried to codify the "dominant norms of science," these efforts represent only half the total effort. Despite Merton's attempt to explicate the "subsidiary norms" (1961), the second half of the effort has yet to be accomplished. Indeed, the language and manner of the first set of norms (and their Zeitgeist) may have impeded work in explaining the subsidiary norms.¹

This paper focuses on the intense personal character of science. Whereas the impersonal character of science was central in early studies, the reverse is true in later writing. The following from Merton and Barber presents the case for the impersonal character of science:

Universalism finds immediate expression in the cannon that truth claims, whatever their source, are to be subjected to preestablished impersonal criteria: (italics in original) consonant with observation and with previously confirmed knowledge. The acceptance or rejection of claims entering the lists of science is not to depend on the personal or social attributes of their protagonist; his race, nationality, religion, class and personal qualities are as such irrelevant. Objectivity precludes particularism. The circumstance that scientifically verified formulations refer to objective sequences and correlations militates against all efforts to impose particularistic criteria of validity ... The imperative of universalism is rooted deep in the impersonal character of science (Merton, 1949: 607, italics added).

Emotional involvement is recognized to be a good thing even in science-up to a point: it is a necessary component of the moral dedication to the scientific values and methods. But in the application of those techniques of rationality, emotion is so often a subtle deceiver that a strong moral disapproval is placed upon its use.

This is not to say that strong emotions are

entirely absent in the relations among scientists themselves.... In all their specialized fields, scientists have been something more than bloodless automatons. The ideal of emotional neutrality [as an instrumental condition for the achievement of rationality], however, is a powerful brake upon emotion anywhere in the instrumental activities of science and most particularly in the evaluation of the validity of scientific investigation (Barber, 1952:126-7, italics added).²

These earlier passages stand in marked contrast to later views:

No one who systematically examines the disputes over priority can ever again accept as veridical the picture of the scientist as one who is exempt from affective involvement with *his* ideas and *his* discoveries of once unknown fact (Merton, 1963a:80, italics in original).

Michael Polanyi (1958) argues with even more force that the personal character of science infuses its entire structure. The testing and validating of scientific ideas is as governed by the deep personal character of science as the initial discovery of the ideas. In sharp contrast to the views of Popper (1961,1971), Polanyi (1958) argues that not only is this the case, but it ought to be the case. That is, science ought to be personal to its core. Science is not thereby reduced to a state of hopeless subjectivism. Indeed, it is the interplay between personal and impersonal forces that makes for the rationale and ultimate rationality of science. This paper is a case study of sociological ambivalence and the personal nature of science.

¹I am not suggesting that these speculations suffice to explain the lack of widespread investigations by sociologists on the subsidiary norms of science. The entire phenomenon is certainly a fitting topic for investigation. Indeed, it would constitute an appropriate topic in the sociology of sociological knowledge. Such an investigation would undoubtedly shed light on why we have been loath to study science (cf. Merton, 1963a:84).

² Note that the idea of the impersonal character of science, particularly as it relates to validating scientific statements, enters into two distinctly different norms. For Merton (1949), it is deeply rooted in the imperative or norm of universalism; whereas for Barber (1952), it is rooted in the norm of emotional neutrality. The difference may be due to the fact that for Merton the very notion of becoming a scientist implies commitment to the norms of science (Merton, 1949:605). In this sense, it is nonsense to speak of the idea of the emotionally uncommitted scientist. However, since this notion seems so deeply ingrained (Taylor, 1967:3-5), I believe with Barber that it is worthwhile to consider the idea of emotional neutrality as a separate norm (see also Storer, 1966:80). Doing so will allow us to challenge this norm and make the case for the existence of an opposing counter-norm.

This paper seeks (1) to reevaluate the initial set of dominant norms proposed by Merton, and subsequently refined by Barber, Hagstrom, Storer, and West (1960), (2) to propose a tentative set of subsidiary norms based on the results of an empirical and theoretical case study, (3) to examine the relationship between these two sets, and thereby, (4) to raise the question whether a sense exists in which both sets of norms are primary or dominant as, for example, in a dialectic where neither side or position is "superior" or "inferior" but merely "different from" and "maximally opposed" to the other (Churchman, 1971; Mitroff and Betz, 1972; Mitroff, 1973, 1974a).

THE CASE STUDY

Almost three months to the day of Apollo 11's landing (July 20, 1969), a series of extensive interviews were begun with fortytwo of the most eminent scientists who studied the moon rocks. Each scientist was interviewed intensively four times over a span of three and a half years; the interviews were conducted between the completion of one Apollo moon mission and the start of another. The scientists were thus interviewed between Apollo 11 and 12, 12 and 14,³ 14 and 15, and 15 and 16. The interviews ranged from open-ended discussions in the opening round to written questionnaires in the subsequent rounds. The open-ended discussions were designed to explore a range of issues connected with the lunar missions and to establish rapport with the scientists. The written questionnaires, given in person to encourage the scientists to talk about and even criticize the questionnaire items, focused on specific attitudes towards issues raised in the opening discussions.

All interviews were conducted by the author. Each interview was tape-recorded for several reasons: One, tape-recording permitted a detailed analysis of the substantive and affective content of the interviews as manifested in the emotions and vocal inflections displayed by the scientists. Two, the taped interviews are an oral-history record of some of the most important scientists in the Apollo

program, worth preserving for the archives.⁴ All in all, approximately 260 hours of interviews were recorded.⁵ In addition to these interviews conducted in private, the public behavior of the scientists was also monitored (for comparison with their private responses) at such scientific conferences during the interim period of the study as the first three Apollo Lunar Science Conferences held in Houston, Texas, plus various national meetings of the American Geophysical Union, the American Meteoretical Society, and the American Association for the Advancement of Science. The results and conclusions which follow are thus based on observations and inferences from behavior and expressions of attitude made on repeated occasions and cross-checked over a period of almost four years. The stability and consistency of the responses over time and over different methods of measurement gives added credence and significance to the results.

Rationale

The Apollo moon scientists were chosen for study for various reasons. A major initial premise (later confirmed) was that the Apollo program would be an excellent contemporary setting in which to study the nature and function of the commitment of scientists to their pet hypotheses in the face of possibly disconfirming evidence. A review of the scientific and popular literature before the landing of Apollo 11 found that various scientists had strongly committed themselves in print as to what they thought the moon would be like, and in a few cases, what they ardently hoped the moon would be like.⁶

⁶ As desirable as it would be to give examples (for one thing to show that such expressions were not isolated and infrequent), to preserve the anonymity of the respondents, some of whom were members of this group, I cannot cite this literature. The reasons for such stringent constraints will become apparent.

³Apollo 13 is not listed since it did not influence the scientific beliefs of the scientists during the period. It was the ill-fated mission that failed to land on the moon.

⁴Preliminary arrangements have been made to deposit the materials in the library of the American Institute of Physics, New York City, to make them available to interested and qualified scholars.

⁵The average recorded length of the first set of interviews was 2 1/4 hours; the second, 1 1/2 hours; the third, 1 1/3 hours; and the last, 1 hour. Thus, for 3 1/2 years an average of 6.1 recorded hours was spent with each scientist. This was supplemented by an average of another four hours of unrecorded time. This does not include the time spent in informal conversation at various conferences.

Furthermore, in some cases, these scientists' views were in direct conflict. Coupling this with the drama surrounding the landing of Apollo 11 and the competitiveness in the community of Lunar scientists, it appeared that the Apollo program had many of the same ingredients aptly described by Watson (1968) in the race for the discovery of DNA. In short, the Apollo program offered an interesting setting⁷ in which to study the "Resistance by Scientists to the Scientific Discoveries of Other Scientists" (cf, B. Barber, 1961) plus "the Commitment of Scientists to their Pet Hypotheses."8 Most important of all was the chance to study the possible normative value of such behavior instead of assuming it to be dysfunctional, unscientific, or irrational.9

The notion of commitment was central to the study in other ways. For example, much of it concerns what the body of scientists thought about their fellow scientists. Who were perceived as most committed to their pet hypotheses? What did they think of such behavior? What did the scientists think of the abstract idea of commitment itself? While they might differ in the degree and nature of their commitments, did they believe that every scientist had to have a certain amount of commitment to do good science? If so, how did the scientists then react to the notion of the disinterested observer? Is that notion as

⁸ The fact that some scientists had publicly and repeatedly declared their scientific positions in print was important. Kiesler's work (1971) suggests that "the explicit and forceful declaring of one's commitment has the effect of increasing the degree of commitment to one's position." The Apollo program thus presented the rare opportunity to study the commitment of scientists to their ideas and the change in their ideas, or lack of it, in the face of strong prior beliefs.

⁹Such behavior is not "irrational" from every standpoint. Certain theories or philosophies of science (Churchman and Ackoff, 1950; Laudan, 1965) maintain that it is rational for scientists to act in accord with the principle of tenacity; i.e., a scientist ought to do everything "legitimately" (excluding cheating, falsifying evidence) in his power to present his hypotheses in the best possible light and to defend them. This point will be discussed later when we deal with an alternative normative structure for science. deeply ingrained in the beliefs of scientists as the conventional portraits of science would have us believe?

The study also asked how did the scientists' ideas about the moon change from mission to mission? What were the significant results from Apollo? Which scientists were credited with producing these results? Were serious errors committed in selecting lunar landing sites? After Apollo 11, did they continue to think the moon trip was worthwhile? Why? Why not? Are there lessons to be learned from Apollo for planning future missions?

As for methodology, what did the moon scientists believe about the relationship between theory and data? Did they believe that observations were independent of theory, or as increasing numbers of philosophers of science (Churchman, 1961,1971; Feyerabend, 1965, 1970a, 1970b) were asserting, that observations were theory-laden? Further, what did the scientists believe about the notion of the hypothetico-deductive method as an accurate and fruitful representation of scientific method? Countless philosophers and methodologists of science have examined such issues, but there are few (cf. Hagstrom, 1965) systematic studies of what scientists themselves think about such issues. No matter how idealized the concept of scientific method and however far removed from real concerns, it should be compared with the reality of everyday practice and the beliefs of practicing scientists (cf., Maxwell, 1972:133). One way to do this is to submit methodological statements for the scrutiny and responses of scientists.

The Sample

Table 1 gives the breakdown by institutional affiliation of the scientists interviewed in round I (the time period of the Apollo 11 mission) who were either (1) principal investigators (PI's) or (2) co-investigators (Co-I's), (3) those who were neither but who had access to or contact with the lunar samples (Access), and finally, (4) those scientists who had no contact at all with the lunar samples (No-Access). The term PI is the official designation that NASA (like other granting agencies) uses to denote the officially designated principal researcher (or proposer) of a project or experiment. Every experiment

⁷In Merton's terms, the Apollo program constituted a "strategic research site" for observing counter-norms. See Merton (1963b:239) on the importance of "strategic research sites" in the sociology of science.

SCIENTIFIC NORMS

Table 1.	Form of Involvement in Lunar	Program and Institutional
	Affiliation of Scientists	5

Institutional Affiliation		Form of Involvement in the Lunar Program				
		Co-I's	Access	No-Access]	
University or university affiliated research labs	15	2	4	5	26	
NASA** installations	0	2	4	3	9	
USGS*** plus related Govt. agencies	1	0	1	1	3	
Govt. research**** labs, institu- tions plus foreign counterparts	1	0	0	0	1	
Private industry		0	1	1	3	
Total		4	10	10	4 2	

**	=	National Aeronautics and Space Administration.
***	=	United States Geological Survey.
****	=	For example, like the Brookhaven National Lab.
PI's	=	Principal Investigators.
Co'I's	=	Co-Investigators.
Access	=	Neither a Principal nor Co-investigator, but had access to Lunar materials.
No-Access	=	Had no form of access to Lunar materials, but interviewed because of historical importance.

whether it had a co-experimenter or Co-I had a principal investigator or PI. Access refers to those scientists who were neither PI's nor Co-I's but who had legitimate access to the lunar materials as members of a PI's research team. The Sample Access scientists contributed directly to the study of the lunar samples experimentally or theoretically, as the remaining category of scientists did not. Although many were indirectly involved, the No-Access scientists were not directly involved in analyzing lunar materials. They were included because they had played an important historical role in our understanding of the moon, or had significant insight into the lunar missions or their fellow scientists, or finally, because they had been recommended for inclusion by their fellow scientists.

Table 1 shows that the majority of the scientists interviewed were based in universities (26) and that an even larger number of PI's were university scientists (15). To

appreciate the full significance of these percentages plus the remaining percentages, it is necessary to discuss how the sample was formed.

Like the total population of scientists selected by NASA to be PI's and Co-I's for Apollo 11 (see Table 2), almost two-thirds of our sample were university scientists. The sample is a snowball sample (Sjoberg and Nett, 1968). It began with a few key scientists willing to lend their names to draw others into the sample. Two questions were asked of everyone interviewed, Which scientists would you recommend that I ask these same (interview) questions of next?, and, For which reasons do you recommend that I see these persons?.

The sample was formed this way for the following reasons: (1) Not only were the scientists inordinately busy, but they had been besieged by reporters for interviews. They had to be induced to give time and

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Table 2	2.	Form of Involve	ement in th	he Lunar 1	Program of	Total Pe	opulation	of	
		Scientists Sele	ected by NA	ASA to be	PI's and	Co-I's fo	or Apollo	11	and
		Institutional A	ffiliation	ı					

	Form of		
Institution Affiliation	PI's	Co-I's	Totals
University or university affiliated research labs plus foreign counterparts	96	101	197
NASA installations	9	23	32
USGS plus foreign counterparts	20	23	43
Govt. research labs, institutions plus foreign counterparts	7	12	19
Private industry	10	13	23
Totals	142	172	314

NOTE: The numbers in this table were compiled from several sources, published PI lists in technical magazines and internal NASA documents. Since the various lists are not always in complete accord, the numbers reported above can only be taken as approximate. However, they would seem to be in the right range since the number that was commonly bantered about to denote the number of Apollo 11 PI's was of the order of 140.

thought to the study. (2) Some of the interview questions were sensitive in that they asked the scientists to express their feelings about their colleagues. However, the investigation was given legitimacy by the support of their peers. (3) Some scientists in the system were so important that no study of the Apollo missions could ignore them, whether or not they appeared in a random sample. (4) The social organization of the system was of basic interest. Asking the respondents who should be interviewed not only formed the sample but also generated vital information about this organization. (5) Forming the sample thus tends to offset some of the objections (Lakatos and Musgrave, 1970) raised against studying the "average" scientist, or in Kuhn's (1962) terms, the "normal" scientist. The argument is that a sample composed entirely of "average" scientists is a poor one on which to base conclusions about the nature of science. It can be an even worse basis for concluding about the ideal practice of science. Why base ideas for the superior or improved practice of science on the behavior of average or mediocre scientists? Selecting a sample in the above manner tends to counter this objection, since such a sample will tend to

contain the elite scientists in the system under study.

If the general population of Apollo 11 scientists represents an elite to begin with, then the sample is best described as an elite of elites (cf., Zuckerman, 1972). It contains some of the most distinguished geologists and scientific analysts of the Apollo missions. Two of the forty-two have the Nobel prize; six are members of the National Academy of Scientists; thirty-eight have their Ph.D.; thirteen are major editors of key scientific journals in the field. Nearly all are at prestigious universities or a top-ranked government research lab.

The sample was not expressly generated by asking for elites. The scientists based their recommendations of others on one or more of three criteria: (1) that they should be important or eminent; (2) that they should be included if the study were to represent many scientific points of view (most recognized that they represented only one viewpoint of the scientists and hence recommended that I see at least one whose views were opposed to theirs); and (3) that the study should include several "typical" or "average" scientists. Many asserted: "You should see some of the average

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stiffs, not just the stars," or, "If you see only those guys, you've got a sample of all chiefs and no Indians." Despite their warning, the snowball sample contains "many more chiefs than Indians."

The sample also includes data on age and scientific discipline. As of July, 1969 (the time of Apollo 11), the mean age was 47.0 years with a standard deviation of 9.3 years. This is indicative that these are largely established scientists. Only six in the first round of interviews were under forty and only three in the entire study were under thirty-five. In this regard, the sample differs markedly from the general population of Apollo scientists. In their summary of the Apollo missions, Levinson and Taylor (1971) note that "a surprisingly large number of the scientists are in their 30's; only a small percentage are over 50 years (Levinson and Taylor, 1971:2)." The sample almost exactly reverses the trend in the larger population of Apollo scientists. Finally, nearly all the scientists are located in an academic department or institutional setting that corresponds closely to the academic discipline in which they received their degree (see Table 3).

CONDUCT OF THE INTERVIEWS

The opening interviews explored the scientific issues connected with Apollo and assessed the scientists' positions on these issues before and just after Apollo 11. They were asked: What theories for the origin of

Table 3. Scientific Disciplines Represented in the Sample*

Scientific Discipline	Number of Scientists
Geology (general)	7
Geophysics	4
Geochemistry	16
Chemistry	5
Physics	4
Astronomy	4
Engineering	2

*Based on discipline in which received highest degree.

the moon are you familiar with? Can you rate how plausible you felt each theory was before the Apollo 11 data? Can you rate how plausible you feel each theory was after the Apollo 11 data? Other technical issues, such as the temperature of the moon and the origin of mascons (Muller and Sjogren, 1968) were also explored in this way.

These questions elicited needed information and were natural interview openers. They were designed to get the scientists talking about an area of interest to them in which they were the experts. They avoided personal issues and focused on supposedly neutral technical issues. They did not ask which scientists were most committed to their pet hypothesis. I assumed that such an issue, especially reference to particular scientists by name, would be far too sensitive to approach directly. These fears turned out to be entirely unfounded. The scientists themselves raised the question of commitment.

SOME GENERAL FINDINGS

All the interviews exhibit high affective content. They document the often fierce, sometimes bitter, competitive races for discovery and the intense emotions which permeate the doing of science.

No matter what the topic-for example, the status of some technical physical theorythe scientists moved the discussion toward intensely personal matters. They could not discuss the status of a physical theory and the scientific evidence bearing on it in purely impersonal or "objective" terms (see footnote 13). Some scientists or group of scientists were clearly associated in the minds of the sample with each theory, serving as its personal advocates and defenders. Hence, the scientists could not react to a theory without reacting simultaneously to its proponents.

After The Double Helix (Watson, 1968), these observations are no longer novel. As Merton (1969) pointed out, only our naïveté about science and our lack of historical awareness of past priority disputes caused this aspect of The Double Helix to make news. Bitter competition and acrimonious disputes have been more nearly the rule in science than the exception.

What was surprising in my interviews was the ease with which the scientists recognized the commitment of their peers to certain doctrines, their willingness to talk about it openly and to name names. Even more surprising was the extent to which most of them had considered the effect of commitment. The term "commitment" was used in three distinct (but related) senses. The first expressed the notion of intellectual commitment, that is, that scientific observations were theory-laden. In order to test a scientific hypothesis, one had to adopt or commit oneself, if only provisionally, to some theory so that the phenomenon could be observed. The second sense expressed the notion of affective commitment. More often than not scientists were affectively involved with their ideas, were reluctant to part with them, and did everything in their power to confirm them. The third sense expressed the notion that the entire process of science demanded deep personal commitment. Deep personal emotions were not merely confined to the discovery phases of scientific ideas but to their testing as well. In the words of the respondents, it took "as much personal commitment to test an idea as it did to discover it." The context of the interviews generally made clear which notion was being invoked.

In response to the opening interview questions on the relative plausibility of various scientific hypotheses associated with the moon, three scientists were overwhelmingly nominated as most attached to their own ideas.¹⁰ The comments referring to these scientists were peppered with emotion. The following is typical:

X is so committed to the idea that the moon is Q that you could literally take the moon apart piece by piece, ship it back to Earth, reassemble it in X's backyard and shove the whole thing. . .and X would still continue to believe that the moon is Q. X's belief in Q is unshakeable. He refuses to listen to reason or to evidence. I no longer regard him as a scientist. He's so hopped up on the idea of Q that I think he's unbalanced.

The three scientists most often perceived by their peers as most committed to their hypotheses and the object of such strong reaction were also judged to be among the most outstanding scientists in the program. They were simultaneously judged to be the most creative and the most resistant to change. The aggregate judgement was that they were "the most creative" for their continual creation of "bold, provocative, stimulating, suggestive, speculative hypotheses," and "the most resistant to change" for "their pronounced ability to hang onto their ideas and defend them with all their might to theirs and everyone else's death." Because of the centrality of these scientists, the perception of them by their peers was studied over the course of the Apollo missions. The perceived intensity of commitment of these scientists to their pet ideas was systematically measured in terms of various attitude scales. Every scientist in the sample was asked to locate the scientific position of each of the three scientists with respect to a number of possible positions and to rate the intensity of their commitment to their position. There was virtually no change in the perceived positions and the perceived intensity of their commitment to their ideas over the three and a half vear period.¹¹

¹⁰ These same three scientists were nominated in open-ended conversation and also in response to the direct questions: "Which scientists are in your opinion most committed to their pet hypotheses?" and "Which scientists do you think will experience the most difficulty in parting with their ideas?" These questions were asked at each interview in the 3 1/2 year period. No matter how they were asked or when, the responses are the same.

¹¹It is beyond the scope of this paper (cf. Mitroff, 1974b) to report on this aspect of the study in detail. Measuring and assessing the differences in psychology between the scientists in the sample was a major focus of the study. Various typologies of different kinds of scientists were constructed from their comments. At the one extreme, are the three highly committed scientists who "wouldn't hesitate to build a whole theory of the solar system based on no tangible data at all; they're extreme speculative thinkers." At the other is the data-bound experimentalist who "wouldn't risk an extrapolation, a leap beyond the data if his life depended on it." Whereas the three highly committed scientists are perceived as biased, brilliant, theoretical, as extreme generalists, creative yet rigid, aggressive, vague, as theoreticians, and finally as extremely speculative in their thinking, the opposite extremes are seen as impartial, dull, practical, as specialists, unimaginative yet flexible, retiring, precise, as experimentalists, and extremely analytical in their thinking. It is also beyond the scope of this paper to show (cf. Mitroff, 1974b) that these psychological differences can be used, contrary to Merton (1957:63840), to argue for a psychological explanation for the contentious behavior of scientists involved in priority disputes. This is not to say that such behavior must be explained purely psychologically or sociologically. Indeed, it is due to the interaction of both factors

The Emergence of Counter-Norms

The concept needed to make the transition between the interview material and the first counter-norm is provided by the following criterion for recognizing the existence of a norm:

...as we know from the sociological theory of institutions, the expression of disinterested moral indignation is a signpost announcing the violation of a social norm (Merton, 1957:639; see also Parsons, 1948:368-70, and Merton, 1949:390-4).

If moral indignation towards a certain kind of behavior₁ announces that a social norm₁ has been violated, then moral indignation towards a class of opposing behavior₂ announces the violation of an opposing norm₂ (cf., Mitroff, 1973,1974a).

The intense reactions of the scientists towards the behavior of the three scientists perceived as most committed to their ideas suggests strong support for the notion of the impersonal character of science. The behavioral characteristics which produced the most intense reactions were those most in conflict with the impersonal character of science. In this regard, the scientists affirmed precisely the norms of universalism and emotional neutrality in science (Merton, 1949:607-10; Barber, 1952:126-7); hence, one can infer that these particular norms of science are accepted. Further, although this conclusion is based on an inference, it is strengthened repeatedly since it arises directly out of the scientists' open-ended responses. Early in the study, I deemed it important to avoid references to the norms of science that might put socially desirable responses in the mouths of the scientists. I quote from some of the scientists regarding the three scientists perceived as most committed to their pet hypotheses: "They have no humility;" "their papers are public relations jobs;" "X relishes the spectacular and has a craving for power;" "Y is a good salesman: that's why he gets attention;" "Z tried to put words in the astronauts' mouths; he tried to get them to

in that individual scientists react differently to the social institution of science. In other words, science probably does not attract contentious personalities more than other institutions. However, some kinds of scientists are more contentious than others and thus quicker to initiate and press their claims for priority. see what he wanted them to find;" "X has a curious if not perverted pattern of reasoning that goes something as follows: Hypothesis-If the moon were P, then Q would be true; Premise-I WANT Q to be true; Conclusiontherefore, P IS true;" "X and Y don't do science, they build personal monuments to themselves; I no longer regard them as scientists."

On the other hand, if the preceding can be interpreted as moral indignation indicating support of the dominant norms of emotional neutrality and universalism, then some equally strong responses from the scientists suggested the existence of two equally strong counternorms. Immediately after the responses to the opening questions, two follow-up questions were raised: "Given your strong reaction to the behavior of the particular scientists you've mentioned as being most committed to their ideas, is there any positive role that you see that commitment has to play in science? If so, what is your opinion of the concept of the disinterested observer?" Again, in many cases I need not have raised these questions since the scientists raised them in the course of their comments. Also, the context made clear that the scientists used the term "emotional commitment" in two of the three senses referred to earlier: in the sense of an individual scientist's deep affective involvement with his ideas and in the sense that science was a personal enterprise from beginning to end.¹²

Every one of the scientists interviewed on the first round of interviews indicated that they thought the notion of the objective, emotionally disinterested scientist naïve.¹³

¹² The norm of "disinterestedness" was not raised in the sense originally formulated by Merton (1949:612-14) and Barber (1952:131-3), i.e., as the idea that a scientist is expected to achieve his self-interest in work-satisfaction and prestige through serving the community.

¹³The notion of objectivity was not defined for the scientists because the context of the interviews and their comments made clear that it was most typically taken to mean facts "uncoloured by, [or] independent of, the feelings or opinions of the person making them (Graham, 1965:287)." As Popper (1972) put it:

Knowledge in this objective sense is totally independent of anybody's belief, or disposition to assent; or to assert, or to act. Knowledge in the objective sense is knowledge without a knower: it is knowledge without a knowing subject (Popper, 1972:109).

The vocal and facial expressions that accompanied the verbal responses were the most revealing of all. They ranged from mild humor and guffaws to extreme annoyance and anger. They indicated that the only people who took the idea of the purely objective, emotionally disinterested scientist literally and seriously were the general public or beginning science students. Certainly no working scientist, in the words of the overwhelming majority, "believed in that simple-minded nonsense." Because they actually did science and because they had to live with the day-to-day behavior of some of their more extreme colleagues, they knew better.

What was even more surprising was that the scientists rejected the notion of the "emotionally disinterested scientist" as a prescriptive ideal or standard. Strong reasons were evinced why a good scientist *ought* to be highly committed to a point of view. Ideally, they argued, scientists ought not to be without strong, prior commitments. Even though the general behavior and personality of their more extremely committed colleagues infuriated them, as a rule they still came out in favor of scientists having strong commitments. The following comments are typical:

- Scientist A Commitment, even extreme commitment such as bias, has a role to play in science and it can serve science well. Part of the business [of science] is to sift the evidence and to come to the right conclusions, 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 [emotional] commitment one wouldn't have the energy, the drive to press forward sometimes against extremely difficult odds.

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.

- Scientist C The [emotionally] disinterested scientist is a myth.¹⁴ 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 biases.
- Scientist D 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.

Scientist E - In order to be heard you have

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The scientists generally rejected this notion of objective knowledge.

¹⁴See Imagination and the Growth of Science (Taylor, 1967:3-5) for a forceful presentation of the myth of science: "Scientists must be immediately prepared to drop a theory the moment an observation turns up to conflict with it. Scientists must have an absolute respect for observations, they must hold scientific theories in judicial detachment. Scientists must be passionless observers, unbiased by emotion, intellectually cold" (Taylor, 1967:4). Also see Taylor (1967) for an argument as to why the preceding view of science though false is not a straw-man (see also Merton [1969:2-3]and Mitroff [1972]).

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.

Scientist F - 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 *is* 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.

And finally,

Scientist G - You can't understand science in terms of the simple-minded articles that appear in the journals. Science is an intensely personal enterprise. Every scientific idea needs a personal representative who will defend and nourish that idea so that it doesn't suffer a premature death. Most people don't think of science in this way but that's because the image they have of science only applies to the simplest, and for that reason, almost non-existent, ideal cases where the evidence is clear-cut and it's not a matter of scientists with different shades of opinion. In every real scientific problem I've ever seen, the evidence by itself never settled anything because two scientists of different outlook could both take the same evidence, and reach entirely different conclusions. You eventually settle the differences, but not because of the evidence itself but because you develop a preference for one set of assumptions over the other. How you do this is not clear since there's not always a good set of reasons for adopting one rather than the other.

Note that in this part of the discussion the scientists partly reversed themselves and praised their more committed colleagues precisely for their extreme commitments:

The commitment of these guys to their ideas while absolutely infuriating at times can be a very good thing too. One should never give up an idea too soon in science-any idea, no matter how outrageous it may be and no matter how beaten down it seems by all the best evidence at the time. I've seen too many totally disproven ideas come back to haunt us. I've learned by now that you never completely prove or disprove anything; you just make it more or less probable with the best of what means you've got at the time. It's true that these guys are a perpetual thorn in the side of the profession and for that reason a perpetual challenge to it too. Their value probably outweighs their disadvantages although I've wondered many times if we might not be better off without them. Each time I reluctantly conclude no. We need them around. They perpetually shake things up with their wild ideas although they drive you mad with the stick-to-itiveness that they have for their ideas.

The comments illustrate clearly the variety of reasons for the belief that scientists should be emotionally committed to their ideas. Above all, they reveal the psychological and sociological elements that permeate the structure of science. Psychologically, the comments indicate that commitment is a characteristic of scientists. The comments strongly support Merton's ideas on scientists' affective involvement with their ideas (1963a:80). Sociologically, the comments reveal the social nature of science. Scientist E, for example, says there's so much "stuff in the system" that if one wants to be heard over the crowd, one must adopt a position more extreme than one believes in.¹⁵ Scientist F

¹⁵Scientist E's statement is interesting for a

continues that this inevitably thrusts scientists into the midst of adversary proceedings, a highly significant observation.

In recent years, considerable work in the philosophy of science, for example, the work of Churchman and Feyerabend, has explained science as resulting as much from conflicts between scientists as from agreements. They argued that disagreement between scientists is as natural as agreement between them, and that such disagreements are as necessary for the growth of science as their agreements (see also Kuhn, 1962). Feyerabend has consistently expressed the view that science depends on intense opposition between at least two theorists who disagree strongly about the same phenomenon. In Feyerabend's theory, the proliferation of contesting views on any subject is fundamental to the progress of science. The implications of Feyerabend's thesis for the present discussion are as follows (Mitroff, 1974a, 1974b): If every scientist were committed to the same idea to the same degree as every other scientist, there would be nothing positive in commitment per se. Indeed, if all men shared the same commitments, the terms "commitment" and "bias" would have no meaning since they would be undetectable. The fact that men differ greatly in the make-up and degree of their commitments and biases enables scientific objectivity to emerge from conflict and passion.¹⁶ Furthermore, science can always afford a few men of deep commitments. Although they run the risk of being labeled crackpots and being ignored (Davis, 1971), the comments of the respondents suggest that they serve a positive function in science. Finally, Scientist G's comments indicate that the personal character of science pervades its entire structure (Merton, 1957.1963a; Polanyi, 1958).

¹⁶ This of course requires a different concept of scientific objectivity than the one stated in footnote 13. Churchman (1971) has developed a dialectical notion of objectivity which does not depend for its existence and operation on the presumption, as Popper's (1972) does, of knowledge without an opinionated knower.

In the second round of the study a semantic difference related to the concept of the ideal scientist was administered to check on the strength and consistency of the beliefs expressed in the first round. A full discussion of the results would take us too far afield (cf., Mitroff,1974b). Therefore, we will report the results of the one scale (impartial-biased) of twenty-seven scales most relevant to our concerns.

The semantic differentials were administered in person to encourage the scientists to state freely what the scales' and their end adjective pairs meant to them. Each of the twenty-seven scales on the semantic differentials were used to gather quantitative scale responses and verbal protocols. I adopted this technique to maximize the information gained by collecting both qualitative and quantitative responses and to balance the weaknesses of the structured instrument with the strengths of the open-ended or projective interview and vice versa (Sjoberg and Nett, 1968). A t-test performed on the quantitative responses to the scale impartial-biased shows that the scientists reject the notion that their ideal scientist is completely impartial at a high level of significance (p < 0.001).¹⁷

The verbal responses are even more instructive. They parallel those of the first round of interviews and exhibit new aspects as well. The comments indicate that the scientists know that "impartiality is the commonly accepted norm or ideal of scientific life," and that they deliberately reject it as a fact of scientific life and as an ideal. Even more important, their responses indicate a deep ambivalence. They reflect not a simple either/or choice between complete impartiality or complete bias but a complex tug-of-war between two opposing norms operating simultaneously. The following are representative responses:

Scientist A - The concept of the com-

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variety of reasons. For one, it corresponds almost exactly with Murray Davis's provocative notions of what makes a theory in social science interesting. According to Davis (1971), if a theory is to be interesting, then it must differ substantially from our ordinary common sense expectation, but not too much or "you'll get labeled as a crackpot."

¹⁷ Again see Taylor (1967) for why the notion of the completely impartial observer (Mitroff, 1971) is not a straw-man argument. The persistency with which this notion appears in accounts of science destroys the contention that it is a straw-man. If anything, the concept deserves analysis not dismissal. Indeed, labeling such an image a straw-man seems more defensive than analytical. As Merton put it: "The practice of seeking to trivialize what can be shown to be significant is a well-known manifestation of resistance" (Merton, 1963b:251).

pletely impartial observer is as much an absurdity as the completely disinterested scientist. I can't recall any scientist I've ever known who has made a fundamental contribution that was impartial to his discoveries or to his ideas. You not only don't discover anything by being impartial but you don't even test it by being impartial. The severest test of an idea occurs when you've done everything in your power to make the best possible case for it and it still doesn't hold water. Nowhere in all of this are you impartial. This doesn't mean that you ultimately don't discard your ideas. You do, but with reluctance.

Scientist B - It's all right for a scientist to be rather strongly biased while he's pursuing an idea; he should not be indifferent to the various alternatives he's trying to decide between, but he has to be objective enough to discard an alternative that runs into difficulty. This means he has to be able to switch back and forth between being biased and being impartial. Within the constraints of this questionnaire, I'd check a 3. However, scientists should be around 6 part of the time and then be able to switch back to 1. Even better, I'd like to check near both ends of the scale, say 2 and 6, at the same time because in reality you have to have both of these things going on in you simultaneously. It's not as black and white as this questionnaire makes it to be.

In short, if scientific knowledge were the product of uncommitted or weakly committed observers, its understanding would be trivial. Given the presumption of untainted, unbiased observers, it is a trivial matter to explain how objectivity results. It is also a trivial matter to justify the concept of objectivity as knowledge "uncoloured by, or independent of, the feelings or opinions of the person making them" (Graham, 1965:287). The problem is how objective knowledge results in science not despite bias and commitment, but because of them. As Churchman and Ackoff 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 (Churchman better and Ackoff, 1950:224).

In summary, this section has offered theoretical and empirical support for the following proposition: if there exist serious reasons why the concepts of emotional neutrality and universalism ought to be considered as norms of science, then serious reasons also exist for positing emotional commitment and "particularism" as opposing counter-norms of science (see Table 4).

Some Additional Counter-Norms

Similarly, for every norm proposed by Merton (1949) and Barber (1952) one could seriously consider an opposing counter-norm. Table 4 represents the outcome of such an effort.

As important as it would be to go through Table 4 in detail, space requires that we treat only one additional norm.

Consider the conventional norm of communism:

"Communism," in the non-technical and extended sense of common ownership of goods, is a second integral element of the scientific ethos. The substantive findings of science are a product of social collaboration and are assigned to the community. They constitute a common heritage in which the equity of the individual producer is severely limited (Merton, 1949:610).

The institutional conception of science as part of the public domain is linked with the imperative for communication of findings. Secrecy is the antithesis of this

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Table 4. A Tentative List of Norms and Counternorms

	Norms		Counternorms
1.	Faith in the moral virtue of rationality (Barber, 1952).	1.	Faith in the moral virtue of rationality and nonrationality (cf., Tart, 1972).
2.	Emotional neutrality as an instru- mental condition for the achieve- ment of rationality (Barber, 1952).	2.	Emotional commitment as an instrumental condition for the achievement of rationality (cf., Merton, 1963a; Mitroff, 1974b).
3.	Universalism: "The acceptance or rejection of claims entering the list of science is not to depend on the personal or social attri- butes of their protagonist; his race, nationality, religion, class and personal qualities are as such irrelevant. Objectivity precludes particularism The imperative of universalism is rooted deep in the impersonal character of science" (Merton, 1949:607).	3.	Particularism: "The acceptance or rejection of claims entering the list of science is to a large extent a function of who makes the claim" (Boguslaw, 1968:59). The social and psychological characteristics of the scientist are important factors influencing how his work will be judged. The work of certain scientists will be given priority over that of others (Mitroff, 1974b). The im- perative of particularism is rooted deep in the personal char- acter of science (Merton, 1963a; Polanyi, 1958).
4.	Communism: "Property rights are re- duced to the absolute minimum of credit for priority of discovery" (Barber, 1952:130). "Secrecy is the antithesis' of this norm; full and open communication [of scientific results] its enactment"(Merton, 1949:611).	4.	Solitariness (or, "Miserism" [Boguslaw, 1968:59]): Property rights are expanded to include protective control over the dis- position of one's discoveries; secrecy thus becomes a necessary moral act (Mitroff, 1974b).
5.	Disinterestedness: "Scientists are expected by their peers to achieve the self-interest they have in worksatisfaction and in prestige through serving the [scientific] community interest directly" (Barber, 1952:132).	5.	Interestedness: Scientists are expected by their close col- leagues to achieve the self- interest they have in work-satis- faction and in prestige through serving their special communities of interest, e.g., their invis- ible college (Boguslaw, 1968:59; Mitroff, 1974b)
6.	Organized scepticism: "The scien- tist is obliged to make pub- lic his criticisms of the work of others when he believes it to be in error no scientist's con- tribution to knowledge can be accepted without careful scrutiny, and that the scientist must doubt his own findings as well as those of others" (Storer, 1966:79).	б.	Organized dogmatism: "Each sci- entist should make certain that previous work by others on which he bases his work is sufficiently identified so that others can be held responsible for inadequacies while any possible credit accrues to oneself" (Boguslaw, 1968:59). The scientist must believe in his own findings with utter convic- tion while doubting those of others with all his worth (Mitroff, 1974b).
Or	norm; full and open communication its enactment (Merton, 1949:611, italics added). In the face of it, it would seem absurd to ntend that there could be an opposing norm	having the id out du means approi	g some positive function in science. Still, ea that such a norm might exist came uring the interviews. While it was by no universally acknowledged as a problem, ximately a fifth of the sample, of their ccord, brought up the fact that stealing

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ideas was a minor, and sometimes a major problem in science (cf., Gaston, 1971). By stealing, the respondents did not mean conscious stealing. Such stealing was felt to be so rare as not to constitute a problem. The problem was the unconscious, unintended appropriation of another's ideas-the fact that one often could not trace the origin of one's ideas and hence properly credit one's peers (cf., Merton, 1963a:91). If only as a protective device, it makes sense to consider secrecy as a working "norm-in-use" (Sjoberg and Nett, 1968). However, the more interesting question is whether secrecy is a rational standard or ideal norm of science and not merely a crude protective device.

As a norm opposed to communism, secrecy (or "particularism") can serve various positive functions in science: (1) Rather than detracting from its stability and progress, under certain circumstances secrecy can be seen to serve the ends of science. With no protective counter measures at its disposal, the social system of science would be continually racked by the kinds of open internal disputes for priority so aptly described by Merton. Without secrecy, science would degenerate into a state of continual warfare. A certain amount of secrecy is rational since scientists are not always able to acknowledge the source of their ideas. Until we can develop better social safeguards, we may have to learn to live with some secrecy. (2) Perhaps its most interesting and important function is as a before-the-fact acknowledgment to oneself and others that one has something in the works worth protecting. A certain amount of stealing or appropriation may be both tolerable and beneficial as long as it doesn't reach epidemic proportions. While stealing may be more difficult than secrecy to make into a counter-norm, even it can serve some positive function. As perverse and potentially dangerous as they are, stealing and appropriating may be important ways of informing a scientist and his peers that his work is significant. As one respondent put it:

It was only when I began to do something significant and important that people began to *steal* [italics added] from me. When I began to manage a big research program and all the big, important people began to visit me, they would rush home and try to outdo our results. You know you're doing something significant when people want to steal it [italics added].

Science typically measures the significance of a piece of work by its statistical significance. Perhaps the social test of the real significance of a scientist's work is whether it is worth stealing or not. Whatever the ultimate implications of the study, it has long been an unwritten rule of science that you don't divulge what you're up to until you're 99% sure that you've got the competition beat in the race to print (cf. Merton, 1957).

I would not make secrecy an unrestricted ideal of scientific life. If science were to follow the norms of commitment and secrecy exclusively, commitment could cause it to degenerate into complete subjectivity and secrecy could breed solipsism. If science were exclusively founded on secrecy, I doubt it could exist as we know it. The public communication, sharing, and testing of ideas would all but vanish (Ziman, 1968). But the key word is exclusively. For if science were also exclusively founded on the norms of disinterestedness, universalism, and community, I doubt science could have arisen as we know it. The point is that each norm is restrained and if any were unrestrained, science would probably collapse.

CONCLUDING REMARKS

This paper has argued that science contains norms and counter-norms. Both, however, do not operate equally in every situation. Indeed, the concept of sociological ambivalence supposes that one set of norms is dominant and the other subsidiary. However, as this study reveals, the actual situation is more complicated. Norms dominant in one situation can be subsidiary in another. Dominancy is not an invariant property of a set of norms. The dependence of dominancy on situations undoubtedly derives from a host of factors (cf. Mitroff, 1973) such as the paradigmatic structure of a science (Kuhn, 1962). Understanding such dominance is a problem for future research in the sociology of science.

A previous paper (Mitroff and Mason, 1974) examined one of the factors on which dominance depends. The class of scientific problems can be arrayed along a continuum whose underlying dimension is "ease of definition" (Churchman, 1971; Mitroff,

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1973,1974a). At the one extreme are "well-defined" problems, at the other are "ill-defined" problems. Well-defined problems (like the chemical composition of the lunar samples) are amenable to solution in that they can be clearly posed and hence solved by relatively clear-cut, standard, analytic techniques; they are "consensible" (Ziman, 1968) in that a relatively wide degree of consensus can be obtained regarding the "nature of the problem;" in short, they are easily formulated. Ill-defined problems (like the origin of the moon) are almost defiantly elusive; they seem to defy a common "consensible" formulation (Mitroff and Betz, 1972). Because of their widespread consensible nature. well-defined problems seem independent of the personality of their formulators; they appear to be impersonal. Ill-defined problems, on the other hand, appear to be the intensely personal creations of their creators. Whereas the conventional norms of science are dominant for well-structured problems, the counter-norms proposed here appear to be dominant for ill-structured problems. An information theoretic analysis of the shift in the beliefs of the scientists over the course of the Apollo missions with respect to key scientific hypotheses reveals that the more well-structured the problem or hypothesis, the more it was felt to be settled. Conversely, the more ill-structured the hypothesis, the less it was felt to be settled by Apollo (Mitroff and Mason, 1974), and hence, the more it was felt to be subject to the counter-norms described in this paper.

The study of the ambivalence of scientists remains one of the important, unsolved problems in the history, philosophy, psychology, and sociology of science (Holton, 1973). It deserves much more systematic study. The results of this paper, while tentative, are hopefully a step in this direction.

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