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# RESISTANCE TO THE SYSTEMATIC STUDY OF MULTIPLE DISCOVERIES IN SCIENCE

By
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# *Resistance to the Systematic Study of Multiple Discoveries in Science*

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by

ROBERT K. MERTON

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# *Resistance to the Systematic Study of Multiple Discoveries in Science*

THE pages of the history of science record thousands of instances of similar discoveries having been made by scientists working independently of one another. Sometimes the discoveries are simultaneous or almost so; sometimes a scientist will make anew a discovery which, unknown to him, somebody else had made years before. Such occurrences suggest that discoveries become virtually inevitable when prerequisite kinds of knowledge and tools accumulate in man's cultural store and when the attention of an appreciable number of investigators becomes focussed on a problem, by emerging social needs, by developments internal to the science, or by both. Since at least 1917, when the anthropologist A. L. Kroeber published his influential paper dealing in part with the subject (1) and especially since 1922, when the sociologists William F. Ogburn and Dorothy S. Thomas compiled a list of some 150 cases of multiple independent discoveries and inventions (2), this hypothesis has become firmly established in sociological thought.

Appropriately enough, this is an hypothesis confirmed by its own history. For as I have recently shown in more detail (3), this idea of the sociological import of independent multiple discoveries—for brevity's sake, I shall refer to them hereafter as 'multiples'—has itself been periodically rediscovered over a span of centuries. During only the last century-and-a-half, working scientists, historians, sociologists, biographers, inventors, lawyers, engineers, anthropologists, Marxists and anti-Marxists, Comteans and anti-Comteans have all called attention, time and again,

(1) Alfred L. KROEBER, "The superorganic", *American Anthropologist*, XIX (1917), 163-213.

(2) W. F. OGBURN and D. S. THOMAS, "Are inventions inevitable?", *Political Science Quarterly*, XXXVII (1922), 83-100; W. F. OGBURN, *Social Change* (New York, B. W. Huebsch, 1922), 80-102.

(3) R. K. MERTON, "Singletons and Multiples in Scientific Discovery: A Chapter in the Sociology of Science", *Proceedings of the American Philosophical Society*, CV (1961), 471-486.



*Archiv. europ. sociol.*, IV (1963), 237-282. 

both to the fact that multiples occur and to some of the implications of this fact (4).

The point of all this is not, of course, to ask who said it first. The point is, rather, that this repeated rediscovery of the same facts and associated hypothesis has remained all these years in a static condition, as though it were permanently condemned to repetition without extension. After all, forty years have elapsed since Ogburn and Thomas compiled their list of independent discoveries. It has been at least a century and a half since observers began taking formal note of the fact of multiples—even to the extent of compiling short lists of cases in point—and began to draw out the implications of the fact. And it has been at least 350 years since Francis Bacon set down some of the principal ingredients of the hypothesis in a set of luminous aphorisms. Why, then, has the idea remained static all this while ?

It may be, of course, that this is so because the last word has been said about the implications of multiples for a theory of how science develops. Or again, the idea may have remained undeveloped because the familiar fact of multiples is quite incidental and lacks significant import ; that it is seemingly as trivial and insignificant, say, as the equally familiar fact that people occasionally make slips of the tongue or pen. All this is possible. But I want now to examine the position that although it is possible, it is not so. Instead, I suggest, first, that the facts of multiples and priorities in scientific discovery provide a research site that is more strategic for advancing the sociology and psychology of science than appears to be generally recognized and, second, that the failure to build on this research site results largely from non-rational resistance to the systematic scrutiny of these facts. The first part of the paper, then, deals with the intellectual uses of the methodical investigation of multiple discoveries ; the second part with the hypothesis that the neglect stems from identifiable forms of resistance. In short, I try first to answer the question : why bother with *systematic* study of the subject ? and then try to answer the next question : in view of its theoretical significance, why don't social scientists bother with it ?

(4) A partial list of those who have come upon the fact and have stated the hypothesis, during the last century or so, would include Macaulay, Comte, Augustus de Morgan, Sir David Brewster, editorial writers for the London *Times* in mid-19th-century, Samuel Smiles, François Arago, Francis Galton, Friedrich Engels, François Mentré, A. V. Dicey, Pierre Duhem, Émile Du Bois-Reymond, George Sarton, A. L. Kroeber, Albert Einstein, Abel Rey, Nicolai Bukharin, Viscount Morley and, of course, Ogburn and Thomas.

Something is known about the social, cultural and economic sources of "resistance" (in the colloquial sense of "opposition") to new ideas and findings in science, both in the large community of laymen (5) and in the smaller community of scientists themselves (6). In examining the resistance of scientists to the detailed study of multiples, I shall be considering "resistance" in the more technical, psychosocial sense of motivated neglect or denial of an accessible but painful reality (7), in this case, the reality of multiples and of the frequent conflicts over priority of scientific discovery.

## 1. *Multiples as a strategic research site.*

In describing multiple discoveries as affording a strategic research site, I mean only that the data they provide can be investigated to good advantage in order to clarify the workings of social and cultural processes in the advancement of science (8). We can identify at least eight connected respects in which this is so.

*First*, the methodical study of multiples supplements the current emphasis of research in the psychology and sociology of science on "creativity" which is largely focussed on (*a*) the psychological traits

(5) Notably, in a series of papers and monographs by Bernhard J. STERN : *e.g.*, *Social Factors in Medical Progress* (New York, Columbia University Press, 1927); *Should We Be Vaccinated ?* A Survey of the Controversy in its Historical and Scientific Aspects (New York, Harper and Brothers, 1927); "Resistance to the Adoption of Technological Innovations", in *Technological Trends and National Policy* (Washington, Government Printing Office, 1937), 39-66. See also, R. K. MERTON, "Science and the social order", *Philosophy of Science*, V (1938), 321-337; "Science and technology in a democratic order", *Journal of Legal and Political Sociology* I (1942), 115-126; "The machine, the worker, and the engineer", *Science* (1947), 105, 79-84 (for resistance of laymen to social research).

(6) Again, Bernhard J. STERN, *Society and Medical Progress* (Princeton, Princeton University Press, 1941), esp. chap. IX ("Resistances to medical change"); Bernard BARBER, "Resistance by scientists to scientific discovery", *Science*, Sept. 1, 1961, 134, 596-602; on the reluctance to develop the sociology of science, see R. K. MERTON, "Foreword" to Bernard Barber, *Science and the Social Order* (Glencoe, The Free Press, 1952), xi-xxiii; *Social Theory and Social Structure* (Glencoe, The Free Press, 1957), 531-533. See also Philipp FRANK, "The variety of reasons for the acceptance of scientific theories", *Scientific Monthly*, 1954, 79, 139-145; Alexandre KOYRÉ, "Influence of philosophic trends on the formulation of scientific theories", *Scientific Monthly*, 1955, 80, 107-111.

(7) Sigmund FREUD, "The resistances to psycho-analysis", *Imago*, XI (1925), 222-233, reprinted in Freud, *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, edited by James Strachey (London, Hogarth Press, 1961), XIX [1923-1925], 213-222.

(8) For the notion of "strategic research site", see R. K. MERTON, "Problem-finding in sociology", *Sociology Today*, edited by R. K. Merton, L. Broom and L. S. Cottrell, Jr. (New York, Basic Books, 1959), xxvi-xxix.

that appear to be distinctive of creative talents in science; (*b*) the psychological processes of scientific thought, adapting in one form or another, Poincaré's, and then, Graham Wallas' four-step process of preparation, incubation, illumination and verification; and (*c*) the social statuses of creative scientists (9). Now, this array of inquiries into the endopsychic and social attributes of individual scientists of course has its place. But we also know that it is only one, and not necessarily an exclusively apt, type of inquiry. Indeed, much of the recent work on "creativity in science" is a little reminiscent of the early work on "leadership" which, for all its suggestive leads, resulted in palpably few definitive findings about the traits and qualities of "leaders" in human affairs.

*Second*, the study of multiples supplements, in ways that will soon become evident, the current research focus on the interpersonal relations in which scientists are engaged while they are at work; a focus on the "milieu" (10) of the scientist. This emphasis has been reinforced by the tradition of small-group research, with its established theory and research instruments, which are, understandably enough, being applied to study of groups of research-scientists.

*Third*, the study of multiples can supplement the pattern of fitting new research on the behavior of scientists into another established tradition of social science investigation, this time, the study of the formal organization of research establishments and the bearing of this organization on the productive work of scientists.

I do not propose to question the uses of these three major types of research on the behavior and productivity of scientists: their

(9) For an extensive review of these inquiries, see Morris I. STEIN and Shirley J. HEINZE, *Creativity and the Individual: Summaries of Selected Literature in Psychology and Psychiatry* (Glencoe, The Free Press, 1960).

(10) For the notion of the "milieu", as the network of personal relations which intervenes between the individual and the larger social structure, see H. H. GERTH and C. W. MILLS, *Character and Social Structure* (New York, Harcourt, Brace, 1953), and on the tendency of some social scientists to focus on the milieu, as contrasted with the larger social structure, in dealing with social environments, see R. K. MERTON, "The social-cultural environment and *anomie*", *in* H. L. Witmer and R. Kotinsky, eds., *New Perspectives for Research on Juvenile Delinquency* (Washington, Government Printing Office, 1955), 25-26, 42. As I note there, current over-emphasis on the milieu, in contrast to larger social structures, is a "little like the prevailing resistance among physical scientists in the 17th century to the notion of action at a distance". The milieu is not the same as what has been called "informal groups", since it includes formal personal relations as well. It overlaps, but is not identical, with what has been called the "ambiance": the collection of *all* people, and not only those in the immediate social environment, with whom a person interacts. See Theodore CAPLOW, "The definition and measurement of ambiances", *Social Forces*, XXXIV (1955), 28-33.

traits and psychological processes of creative work, the effects upon them of local interpersonal relations and of the formal organization of their work places. But to recognize these uses need not obscure the fact that they all deal either with the endopsychic and social traits and processes of individual scientists or with the immediate social environments in which scientists find themselves. Yet we know, as a decisive fact, that scientists live and work in larger social and cultural environments than those comprised by their local *milieux*. And this seems to be true particularly of the most creative among them. Outstanding scientists tend to be "cosmopolitans", oriented to the wider national and trans-national environments, rather than "locals", oriented primarily to their immediate band of associates (11).

K. E. Clark's study of America's psychologists, for example, found that especially productive psychologists were more apt than a control group to report that their significant reference groups and reference individuals—the people "whose opinions of their work they care about"—were composed by other outstanding psychologists in the United States and in other countries, rather than by their local colleagues (12).

The history of science attests that this has typically been the case for outstanding investigators in every science through the last three centuries. The theoretic import of this should not be overlooked. It would be an egregious blunder to allow the otherwise useful emphasis on trait-analysis or on small-group research to deflect attention from the presumably great part played, in scientific work, of social interaction with others who are *not* in the local milieu. To do otherwise, would be to impose convenient existing tools of investigation upon a problem for which they may not be the most appropriate and, surely, not the exclusively appropriate ones. The data, instruments and theory dealing with larger

(11) R. K. MERTON, *Social Theory and Social Structure*, chap. x : "Patterns of influence : local and cosmopolitan influentials". For use of these concepts in studying the behavior of scientists, academicians and other professionals, see A. W. GOULDNER, "Cosmopolitans and locals : toward an analysis of latent social roles", *Administrative Science Quarterly* II (1957), 281-306, II (1958), 444-480; W. G. BENNIS *et al.*, *ibid.*, II (1958), 481-500; Herbert A. SHEPARD, "Nine dilemmas in industrial research", *ibid.*, I (1956), 295-309; Armond FIELDS, "Eine Untersuchung über administrative Rollen", *Kölner Zeitschrift für Soziologie und Sozialpsychologie*, VIII (1956), 113-123. For the relation of these types to "effective scope", see P. F. LAZARSFELD and W. THIELENS, Jr., *The Academic Mind* (Glencoe, The Free Press, 1958), 263-265.

(12) K. E. CLARK, *America's Psychologists : A Survey of a Growing Profession* (Washington, D. C. American Psychological Association, 1957), 85-86.

aggregates of interacting scientists and of spatially distant reference groups and individuals would seem particularly in point for studying the behavior of scientists for whom patterns of social interaction at a distance seem empirically central. This is only a special case of a general hypothesis about "effective scope" (13) : people in various social statuses differ in the radius of their significant social environments : some, the locals, being primarily oriented toward their local *milieux*, others, the cosmopolitans, being primarily oriented toward the larger society, and responsive to it. The systematic study of multiples and priorities in scientific discovery —which of course typically engage scientists with others outside their local environment—thus provides one basis for investigating extended social relations between scientists and the effects of these upon their work.

It may be useful to put much the same point in a slightly different context. Historians of science and other scholars have long used the phrase, "the community of scientists". For the most part, this has remained an apt metaphor rather than becoming a productive concept. Yet it need not remain a literary figure of speech : apt and chaste, untarnished by actual use. For we find that *the* community of scientists is a dispersed rather than a geographically compact collectivity. The structure of this community cannot, therefore, be adequately understood by focussing only on the small local groups of which scientists are a part. The sheer fact that multiple discoveries are made by men of science working independently of one another testifies to the further crucial fact that, though remote in space, they are responding to much the same social and intellectual forces that impinge upon them all. In a word, the Robinson Crusoe of science is just as much a figment as the Robinson Crusoe of old-fashioned economics. He is an illusion, created by a scheme of thought that requires us to look only inward at thought processes and so to abstract entirely from the wider social and cultural contexts of that thought. Occasional scientists may suppose that they really work alone, meaning by this not the evident fact that only individual men and women, not "the group", think and develop imaginative ideas but that they do so, all apart from environing structures of values, social relations and socially as well as intellectually induced foci of attention. But, as multiple discoveries testify, this image of the man of science is just as much a case of the fallacy of misplaced

(13) LAZARSFELD and THIELENS, *op. cit.* 262-265.

concreteness as is the equivalent image of the man of business "who ascribes his achievements to his own unaided efforts, in bland unconsciousness of a social order without whose continuous support and vigilant protection he would be as a lamb bleating in the desert" (14). For scientists, even the most lonely of lone wolves among them, are all "members of one another". The study of multiples shows how scientists are bound to the past by building upon a deposit of accumulated knowledge, how they are bound to the present, by interacting with others in the course of their work and having their attention drawn to particular problems and ideas by socially and intellectually accentuated interests and how they are bound to the future by the obligation inherent in their social role to pass on an augmented knowledge and a more fully specified ignorance. The community of scientists extends both in time and in space.

These three respects in which the study of multiples provides a strategic research site are simply different facets of the same guiding conception : they supplement current emphases in research on the behavior of scientists by conceiving that behavior as a resultant not only of the idiosyncratic characteristics and the local ambiance of scientists, but also of their place within the wider social structure and culture. Beyond these, are quite other uses of the study of multiples.

A *fourth* use is to help us identify certain significant similarities and differences between the various branches of science. To the extent that the rate of multiples and the types of rediscoveries are much the same in the social and psychological sciences as in the physical and life sciences, we are to led to similarities between them, just as differences in such rates and types alert us to differences between them. In short, the study of multiples can supplement the traditional notion of the unity of all science, a notion usually formulated in terms of the logic of method. It can lead us to reexamine this unity from the standpoint of the actual behavior of scientists in each of the major divisions of science and so to identify their distinctive relations to their respective social and cultural environments. This type of behavioral inquiry does not, of course, replace inquiries into the philosophy of science or the logical foundations of scientific method. It supplements them, by attending to what men in the various sciences actually do, rather than

(14) R. H. TAWNEY, *Religion and the Rise of Capitalism* (New York, Harcourt, Brace, 1926).

by limiting us to what textbooks of scientific method tell us they should do, as they go about their work.

This brings us to a *fifth* use of studying multiples. As we shall see, men of science typically experience multiples as one of their occupational hazards. They are occasions for acute stress. Few scientists indeed react with equanimity when they learn that one of their own best contributions to science—what they know to have been the result of long hard work—is "only" (as the telling phrase has it) a rediscovery of what was found some time before or "just" another discovery of what others have found at about the same time. 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, by his social role and his socially patterned personality, from affective involvement with *his* ideas and *his* discoveries of once unknown fact. The value of examining the behavior of men under stress in order to understand them better in all manner of other situations need not be recapitulated here (15). By observing the behavior of scientists under what they experience as the stress of being forestalled in a discovery, we gain clues to ways in which the social institution of science shapes the motives, social relations and affect of men of science. I have tried to show elsewhere (16), for example, how the values and reward-system of science, with their pathogenic emphasis upon originality, help account for certain deviant behaviors of scientists : secretiveness during the early stages of inquiry lest they be forestalled, violent conflicts over priority, an enending flow of premature publications designed to establish later claims to having been first. These, I suggest, are normal responses to a badly integrated institution of science, such that we can better understand the fact that a sample of American "starred men of science" report that, next to what they describe as "personal curiosity", "rivalry" is most often the spur to their work (17).

To the *sixth* use of the study of multiples, I should like to devote some little time, for it has implications both for a sociological theory of scientific discovery and for social policy governing the support of scientific work. With the vast increases in public and private

(15) See, for example, Hans SELYE, *The Stress of Life* (New York, McGraw-Hill, 1956).

(16) R. K. MERTON, "Priorities in scientific discovery : a chapter in the sociology of science", *American Sociological Review*, XXII (1957), 635-659.

(17) S. S. VISHER, *Scientists Starred 1903-1943 in "American Men of Science"*, (Baltimore, Johns Hopkins University Press, 1947), 531-532.

funds for the support of scientific research, there has emerged a great concern to avoid what is called "wasteful duplication" (18) in allocating these funds. This is a widespread concern: recently expressed in the planned society of the U.S.S.R. as it has been expressed, for some time, in unplanned societies of the West. It has given rise to new organizations for improving communication among scientists; in the United States, for example, the Bio-Sciences Information Exchange. One of the explicit functions of this Exchange is to protect the individual scientist from the "distress" that comes from being "just about to ship off a manuscript only to discover that someone else had done his work for him" (19). This function requires no comment here: it is designed to improve the system of scientific communication and so to prevent the unintended repetition of already *completed* scientific investigations. But the Exchange is also thought of as having the function of guiding those who allocate funds for research so that they may reduce (or ideally, eliminate) what is usually described as "the wasteful duplication of scientific effort".

Often enough, this notion of duplication conceals a premise that should be further examined in the light of research on multiples in science before it is adopted at face value as a guide to policy. For it is not at all apparent that it is "wasteful" for several individual scientists, or teams of scientists, to work toward and to arrive at solutions of the same problem. Consider only four items of relevance which must be tucked out of sight and out of mind in order to arrive at the deceptively cogent conclusion that multiple

(18) See, for example, the extensive *Proceedings* of the International Conference on Scientific Information, Washington, D. C. Nov. 16-21, 1958 (Washington, National Academy of Sciences-National Research Council, 1959). Not, of course, that this problem is now being recognized for the first time. So-called "universal catalogues" of scientific papers and books have a long history. Even by 1828, the followers of Saint-Simon were complaining: "In the absence of any official inventory of ascertained discoveries, the isolated men of science daily run the risk that they may be repeating experiments already made by others. If they were acquainted with other experiments, they would be spared efforts often as laborious as they are useless, and it would be easier for them to obtain means for forging ahead." Nor is this all. The complaint about wasteful duplication is coupled with an observation on the quest for priority in science: "Let us add here", say the early Saint-Simonians, "that the security of men of science is not complete. They are haunted by the work of a competitor. Possibly someone else is gleaning the same field and may, as the saying goes, 'get there first'. The man of science has to hide himself and conduct in haste and isolation work requiring deliberation and demanding aid from association with others." *The Doctrine of Saint-Simon: An Exposition, First Year*, 1828-1829, trans. by Georg G. Iggers (Boston, Beacon Press, 1958), 9.

(19) "Who? What? Where?: An Editiorial", *Science*, 8 August 1958, 128, 277.

discoveries necessarily signify "waste" of duplicative (or unknowingly replicative) scientific effort.

*Item :* True, the theory of multiples in science leads us to conclude that these repeated discoveries were "inevitable", since if one scientist involved in the multiple had not made the discovery, another would have (as we know from the fact that he did). But, this "inevitability" holds only under certain, still poorly identified, conditions. In reviewing the facts of multiples, we ordinarily know only of the several scientists who actually *did* make the same discovery ; we usually do not know how many others were at work on the same problem without having solved it. In short, we really do not know how many scientists of what degrees of competence are required to focus on a particular kind of problem in order to ensure a high probability that it will be solved in a given span of time. If that number is progressively reduced, through what may at first seem to be a rational policy of allocating only one grant or very few grants for research on the problem, the discovery may become anything but inevitable, at least during a given interval.

*Item :* That duplication of scientific effort is wasteful may be true when the problems in hand are fairly routine, and bound to yield to a solution, once a scientist elects to work seriously on them. But these, of course, are the small change of science. When it comes to basic problems which are far from routine and, once solved, will have far reaching implications for further inquiry, duplication, triplication or a higher multiplication of effort may be anything but wasteful.

*Item :* It would be ironic if current planned efforts to achieve efficiency in creative scientific work were to prove self-defeating. In the past, when the support of science was slight and thinly dispersed, the efficiency-of-the-seemingly-inefficient pattern resulted in many multiples partly because many scientists, often unknowing that this was so, elected to work on the same problems. A superficial notion of "wasteful duplication" might result in substituting a policy of the inefficiency-of-the-seemingly-efficient, by so allocating funds for research as drastically to restrict the range of scientists at work on the same problem, thus reducing the probability not only of multiple independent solutions but of any solution altogether at the time. The theory of multiples provides one basis for re-examining policies governing the allocation of funds in support of science.

*Item :* The fallacy of wasteful duplication is much like the fallacy that has long afflicted the interpretation of multiples in science. This fallacy made use of an old-fashioned concept of redundancy—strictly old-fashioned, for it has been going the rounds of philosophers, historians and sociologists for a couple of hundred years. The argument went as follows : The occurrence of a multiple discovery is proof in itself that all but one of the actual discoverers were redundant (*i.e.* superfluous). For if all the other co-discoverers had not made the discovery, it would have been made in any case. Ergo, the fact attests their superfluity.

I shall not bother with those knotty points of method which might protect the unwary against drawing such false conclusions from sound evidence. I note only that this old notion of redundancy ordinarily merged two distinct meanings. For one thing, it meant abundant, copious, plentiful, more than is abstractly needed to achieve a purpose. For another, it meant superfluous, that which can be *safely* done away with. The merger of these two meanings smuggled in a fallacy. This was the absolutistic fallacy of assuming that something was either redundant or not, once and for all, and irrespective of the situations in which it is found. The newer, more differentiated concept of redundancy is relative and statistical. It recognizes that efficiency increases the prospect of error ; that redundancy (or reduced efficiency) makes for safety from error. It leads us to think of and then, in certain cases, to measure a functionally optimum amount of redundancy under specified conditions : that amount which will approximate a maximum probability of achieving the wanted outcome but not so great an amount that the last increment will fail appreciably to enlarge that probability. Multiples in science comprise a particular kind of redundancy that can be thought of in terms of the newer, fruitful concept which opens our eyes to what was presumably there all along, but which went unnoticed. There is safety as well as truth in numbers of similar independent discoveries.

Once we use this concept we see the fallacy of the apparently cogent thesis that in multiples, all discoveries but one are super fluous. This is seen to be logically air-tight and sociologically false. For it assumes what remains to be demonstrated. It assumes that a discovery has only to be made in order for it to enter the public domain of science. But the history of science is checkered with cases that show this is not so. Often, a new idea or a new empirical finding has been achieved and published, only to go unnoticed by others, until it is later uncovered or independently rediscovered and only then incorporated into the science.

After all, that is what we mean by rediscovery : the signals provided by a discovery are lost in the noise of the great information system that constitutes science, and so must be issued anew. Multiples—that is, redundant discoveries—have a greater chance of being heard by others in the social system of science and so, then and there, to affect its further development. From this standpoint, multiples are redundant, but not necessarily superfluous (or wasteful). When the all-but-one versions of the same discovery are described as superfluous, this refers only to the discoverer's psychological experience : he has indeed made the discovery. But this account neglects the sociological components of the process of discovery which deal with the probability of the discovery being made in the first place and, once made, of its being assimilated as a functional part of the science.

Multiple discoveries can thus be seen to have several and varied social functions for the system of science. They heighten the likelihood that the discovery will be promptly incorporated in current scientific knowledge and will so facilitate the further advancement of knowledge. They confirm the truth of the discovery (although on occasion errors *have* been independently arrived at). They help us detect a problem which I have barely and far from rigorously formulated, to say nothing of having solved it : how to calculate the functionally optimum amount of redundancy in independent efforts to solve scientific problems of designated kinds, such that the probability of the solution is approximately maximized without entailing so much replication of effort that the last increments will not appreciably increase that probability. They help us distinguish between the psychological experience of individual scientists who originate a new and fruitful idea or make a new and fruitful observation from the independent sociological process through which this discovery succeeds or fails to become incorporated in the then-current body of scientific knowledge. So much, then, for this sixth set of uses that make up the rationale for the systematic study of multiples in science.

A *seventh* use I have examined at some length elsewhere (20), and will therefore only summarize here. The methodical investigation of multiples enables us to develop a sociological theory of the role of scientific genius in the development of science. This

(20) R. K. MERTON, "Singletons and Multiples in Scientific Discovery", cited in fn. 3. See also an abbreviated version of this paper, "The role of genius in scientific advance", *New Scientist*, No. 259, Nov. 2, 1961, 306-308.

new theory does away with the false disjunction between an heroic theory of science, that ascribes all basic advances to genius, and an environmental theory, that holds these geniuses to have been altogether dispensable, since if they had not lived, things would have turned out pretty much as they did. These traditionally opposed theories are not inherently opposed; they become so only when, as has been the case, they are pushed to indefensible extremes. In an enlarged sociological conception, men of scientific genius are precisely those whose discoveries, had they remained contemporaneously unknown, would eventually be rediscovered. *But* these rediscoveries would be made, not by a single scientist, but by an aggregate of scientists. On this view, the individual man of scientific genius is the functional equivalent of a considerable array of other scientists of varying degrees of talent. The evidence for this conception is in part provided by the multiplicity of multiples in which men of undeniable scientific genius have been involved.

An *eighth* and, for present purposes, final use has to do with what might be described as the therapeutic function which the study of multiples serves for the community of scientists. But I shall postpone further examination of this use until the close of this paper, when we shall have covered some of the evidence indicating that there is ample need for this therapeutic function among scientists of our own day just as there was among scientists of the past.

Perhaps enough has been said about the rationale for the systematic study of multiples. If there is any merit to the opinion that the subject has at least an eight-fold promise for enlarging our understanding of how science develops, there naturally arises the question: why, then, has such systematic (21) study been almost absent? Why has the theory of multiples remained almost static during all these many years?

(21) To avoid misunderstanding, it should be reiterated that I refer only to the *systematic* investigation of multiples and frequent conflicts over priority. The ubiquity of the events themselves has required historians of science and biographers of scientists to record a good deal of evidence on the subject. But the methodical study of the sources of multiples and priority-conflicts, of their structure and consequences for the advancement of science, has remained in much the same undeveloped state for a long time.

## 2. *Sources of resistance* *.

Many of the endlessly recurrent facts about multiples and priorities are readily accessible—in the diaries and letters, the notebooks, scientific papers and biographies of scientists. This only compounds the mystery of why so little systematic attention has been accorded the subject. The facts have been noted, for they are too conspicuous to remain unobserved, but then they have been quickly put aside, swept under the rug and forgotten. We seem to have here something like motivated neglect of this aspect of the behavior of scientists and that is precisely the hypothesis I want to examine now.

This resistance to the study of multiples and priorities can be conceived as a resultant of intense forces pressing for public recognition of scientific accomplishments that are held in check by countervailing forces, inherent in the social role of scientists, which press for the modest acknowledgment of limitations, if not for downright humility. Such resistance is a sign of malintegration of the social institution of science which incorporates potentially incompatible values : among them, the value set upon originality, which leads scientists to want their priority to be recognized and the value set upon due humility, which leads them to insist on how little they have in fact been able to accomplish. These values are not real contradictories, of course—" 'tis a poor thing, but my own"—but they do call for opposed kinds of behavior. To blend these potential incompatibles into a single orientation and to reconcile them in practice is no easy matter. Rather, as we shall now see, the tension between these kindred values creates an inner conflict among men of science who have internalized both of them. Among other things, the tension generates a distinct resistance to the systematic study of multiples and often associated conflicts over priority (22).

* A condensed version of the following pages was presented as the third Daniel Coit Gilman lecture at the Johns Hopkins School of Medicine and published under the title, "The ambivalence of scientists", *Bulletin of the Johns Hopkins Hospital*, Feb. 1963, 112, pp. 77-97.

(22) This paragraph draws upon a fuller account of the workings of these values n the social institution of science : MERTON, "Priorities in scientific discovery", *op. cit.* 645-646.

I can here do little more than hint at some of the overt behavior which I interpret as expressions of such resistance. For one thing, it is expressed in the recurrent pattern of trying to trivialize or to incidentalize the facts of multiples and priority in science. When these matters are discussed in print, they are typically treated as though they were either rare and aberrant (although they are extraordinarily frequent and typical) or as though they were inconsequential both for the lives of scientists and for the advancement of science (although they are demonstrably significant for both).

Understandably enough, many scientists themselves regard these matters as unfortunate interruptions to their getting on with the main job. Kelvin, for example, remarks that "questions of priority, however interesting they may be to the persons concerned, sink into insignificance" as one turns to the proper concern of advancing knowledge (23). As indeed they do : but sentiments such as these also pervade the historical and sociological study of the behavior of scientists so that systematic inquiry into these matters also goes by default. Or again, it is felt that "the question of priority plays only an insignificant role in the scientific literature of our time" (24) so that, once again, this becomes regarded as a subject which can no longer provide a basis for clarifying the complex motivations and behavior of scientists (if indeed it ever was so regarded).

Now the practice of seeking to trivialize what can be shown to be significant is a well-known manifestation of resistance. Statements of this sort read almost as though they were a paraphrase of the old maxim that the law does not concern itself with exceedingly small matters ; *de minimis non curat scientia* [*lex*]. Not that there has been a conspiracy of silence about these intensely human conflicts in the world of the intellect and especially in science. These have been far too conspicuous to be denied altogether. Rather, the repeated conflict-behavior of great and small men of science has been incidentalized as not reflecting any conceivably significant aspects of their role as scientists.

Resistance is expressed also in various kinds of distortions : in motivated misperceptions or in an hiatus in recall and reporting.

(23) Silvanus P. THOMPSON, *The Life of William Thomson, Baron Kelvin of Largs* (London, Macmillan, 1910), II, 602.

(24) Otto BLÜH, "The value of inspiration : a study of Julius Robert Mayer and Josef Popper-Lynkeus", *Isis*, XLIII (1952), 211-220, at 211.

It often leads to those wish-fulfilling beliefs and false memories that we describe as illusions. And of such behavior, the annals that treat of multiples and priorities are uncommonly full. So much so, that I have arrived at a rule-of-thumb that seems to work out fairly well. The rule is this : whenever the biography or autobiography of a scientist announces that he had little or no concern with priority of discovery, there is a reasonably good chance that, not many pages later in the book, we shall find him deeply embroiled in one or another battle over priority. A few cases must stand here for many :

Of the great surgeon, W. S. Halsted (who together with Osler, Kelly and Welch founded the Johns Hopkins Medical School), Harvey Cushing writes : he was "overmodest about his work, indifferent to matters of priority [...]" (25). Our rule of thumb leads us to expect what we find : some twenty pages later in the book where this is cited, we find a letter by Halsted about his work on cocaine as an anesthesia : "I anticipated all of Schleich's work by about six years (or five) [...] [In Vienna,] I showed Wölfler how to use cocaine. He had declared that it was useless in surgery. But before I left Vienna he published an enthusiastic article in one of the daily papers on the subject. It did not, however, occur to him to mention my name" (26).

Or again, the authoritative biography of that great psychiatrist of the Salpêtrière, Charcot, approvingly quotes the eulogy which says, among other things, that despite his many discoveries, Charcot "never thought for a moment to claim priority or reward". Alerted by our rule-of-thumb, we find some thirty pages later an account of Charcot insisting on his having been the first to recognize exophthalmic goiter and, a little later, emphatically affirming that he "would like to claim priority" for the idea of isolating patients who are suffering from hysteria (27).

But perhaps the most apt case of such denial of an accessible reality is that of Ernest Jones, writing in his comprehensive biography that "Although Freud was never interested in questions of priority, which he found merely boring"—surely this is a classic case of trivialization at work—"he was fond of exploring the source

(25) In his magisterial biography, *Harvey Cushing* (Springfield, Charles C. Thomas, 1946), 119-120, John F. FULTON describes Cushing's biographical sketch of Halsted, from which this excerpt is quoted, as "an excellent objective description".

(26) *Ibid.* p. 142.

(27) Georges GUILLAIN, *J.-M. Charcot, His Life, His Work.* Edited and translated by Pearce Bailey (New York, Paul B. Hoeber, 1959), 61, 95-96, 142-143.

of what appeared to be original ideas, particularly his own [...]" (28). This is an extraordinarily illuminating statement. For, of course, no one could have "known" better than Jones—"known" in the narrowly cognitive sense—how very often Freud turned to matters of priority : in his own work, in the work of his colleagues (both friends and enemies) and in the history of psychology altogether.

In point of fact, Dr. Elinor Barber and I have identified more than one hundred and fifty occasions on which Freud exhibited an interest in priority. Freud himself reports, with characteristic self-awareness, that he even dreamt about priority and the due allocation of credit for accomplishments in science (29). He

(28) Ernest JONES, *Sigmund Freud : Life and Work* (London, Hogarth Press, 1957), III, 105. Contrast David Riesman who takes ample note of Freud's interest in priority in *Individualism Reconsidered* (Glencoe, The Free Press, 1954), pp. 314-5, 378.

(29) Sigmund FREUD, *The Interpretation of Dreams*, trans. by A. A. Brill (London, Allen & Unwin, 3rd ed., 1932), p. 175.

"Now [my dream] means : 'I am indeed the man who has written that valuable and successful treatise (on cocaine)' ". This nearmiss in being recognized as the discoverer of cocaine as a local anesthetic is of periodic interest to Freud throughout the greater part of his life. Freud simply cannot put it to rest. At the time he is moving toward the idea, in 1884, he writes his fiancée, Martha, about his "toying with a project [...]; perhaps nothing will come of this, either. It is a therapeutic experiment involving the use of cocaine [...] There may be any number of other people experimenting on it already; perhaps it won't work. But I am certainly going to try it and, as you know, if one tries something often enough and goes on wanting it, one day it may succeed" (*Letters of Sigmund Freud*, ed. by Ernst L. Freud [New York, Basic Books, 1960], pp. 107-8). Seven months later, he writes his future sister-in-law that " 'Cocaine has brought me a great deal of credit, but the lion's share has gone elsewhere' " (quoted by Ernest Jones in his detailed chapter on "The Cocaine Episode", *op. cit.* I, 98). Two years later, he is writing Martha about an episode in the Salpêtrière when the distinguished American ophthalmologist, Hermann Knapp, "who has written a lot about cocaine" says to another of Freud, "[...] it was he who started it all." (*Ibid.* 209). Evidently the episode stung, for not to cite the other intervening allusions to it, Freud is writing Fritz Wittels about "the cocaine story", some thirty-eight years later, on the occasion of an English translation of Wittels' objectionable biography of Freud : "I guessed its usefulness for the eye, but for private reasons (in order to travel) had to drop the experiment and personally charged my friend Königstein to test the drug on the eye [...] Königstein (it was *he*, not I, who so deeply regretted having missed winning these laurels) then claimed to be considered the codiscoverer [with Koller] and [...] both Königstein and Koller chose Julius Wagner *and myself* as the arbitrators. I think it did us both honor that each of us took the side of the opposing client. Wagner, as Koller's delegate, voted in favor of recognizing Königstein's claim, whereas I was wholeheartedly in favor of awarding the credit to Koller alone. I can no longer remember [reports Freud] what compromise we decided on." (*Letters of Sigmund Freud*, p. 351.) About the same time, Freud puts all this in print (in *An Autobiographical Study* [London, Hogarth Press, 1948], first published in 1925, pp. 24-25), explaining that, "While I was in the middle of this work, an opportunity arose for making a journey to visit my fiancée, from whom I had been parted for two years. I hastily wound up my investigation of cocaine and contented myself in my book on the subject with prophesying that further uses for it would soon be found. I suggested, however, to my friend Königstein, the ophthalmologist, oscillates between the poles of his ambivalence toward priority: occasionally seeing multiples as more or less inevitable as when he reports a fantasy in which "science would ignore me entirely during my lifetime; some decades later, someone else would infallibly come upon the same things—for which the time was not now ripe—, would achieve recognition for them and bring me honour as a forerunner whose failure had been inevitable"... (30). On other occasions, he sometimes reluctantly, sometimes calmly and insistently, acknowledges anticipations of his own ideas or reports his own anticipations of others (31); he "implores" his disciple Lou Andreas-Salomé to finish an essay in order "not to give me precedence in time" (32); he admonishes Adler for what he describes as his "uncontrolled craving for priority" (33) just as he admonishes Georg Groddeck for being unable to conquer "that banal ambition which hankers after originality and priority" (34);

that he should investigate the question of how far the anæsthetizing properties of cocaine were applicable in diseases of the eye. When I returned from my holiday, I found that not he, but another of my friends, Carl Koller (now in New York), whom I had also spoken to about cocaine, had made the decisive experiments... Koller is therefore rightly regarded as the discoverer of local anæsthesia by cocaine, which has become so important in minor surgery; [but adds Freud in so many words] I bore my fiancée no grudge for the interruption of my work." All apart from the cocaine story, Freud, with the resolute self-scrutiny that left little place for self-deception, analyzes another of his dreams as having at its root "an arrogant phantasy of ambition, but that in its stead only its suppression and abasement has reached the dream-content". *Interpretation of Dreams*, p. 440.

(30) Sigmund FREUD, « On the history of the psycho-analytic movement », in *The Standard Edition of* [...] *Freud*, XIV, p. 22.

(31) The dozens of such instances need not be cited here, but see only the remarkable paper in which Freud reports that « careful psychological investigation [...] reveals hidden and long-forgotten sources which gave the stimulus to the apparently original ideas, and it replaces the ostensible new creation by a revival of something forgotten applied to fresh material. There is nothing to regret in this; we had no right to expect that what was 'original' could be untraceable and undetermined.

"In my case, too, the originality of many of the new ideas employed by me in the interpretation of dreams and in psychoanalysis has evaporated in this way. I am ignorant of the source of only one of these ideas. It was no less than the key to my view of dreams and helped me to solve their riddles [...] I started out from the strange, confused and senseless character of so many dreams, and hit upon the notion that dreams were bound to become like that because something was struggling for expression in them which was opposed by a resistance from other mental forces [...].

"Precisely this essential part of my theory of dreams was, however, discovered by Popper-Lynkeus independently [...] [His story, *Träumen wie Wachen*] was certainly written in ignorance of the theory of dreams which I published in 1900, just as I was then in ignorance of Lynkeus's *Phantasien*." Sigmund FREUD, "Josef Popper-Lynkeus and the theory of dreams", *Standard Edition* [...] *of Freud*, XIX, pp. 261-263.

(32) In his letter of May 25, 1916, *Letters of Sigmund Freud*, p. 313.

(33) FREUD, "History of the Psychoanalytic movement", *Standard Edition* [...] *of Freud*, XIV, p. 51.

(34) *Letters of Sigmund Freud*, p. 317. I shall have occasion to return to the rest of this letter later on in this paper, when we examine the basic *uncertainty* of genuinely independent originality in science.

he assesses and repeatedly re-assesses the distinctive roles of Breuer and himself in establishing psycho-analysis (35); he returns time

(35) It would take a paper in itself to trace out in detail and to interpret Freud's repeated and developing efforts, over a span of more than thirty years, to disentangle Breuer's and his own contributions to the emergence of psycho-analysis. As he became the object of social pressure to identify the contributions of the two and as the differences gradually became clear to him, he worked toward more discriminating distinctions between their respective intellectual roles in that development. Consider only these few cases in point:

[1896] "I owe my conclusions to the use of the new psycho-analytic method, the probing procedure of J. Breuer [...]" ("Heredity and the aetiology of the neuroses", in Freud, *Collected Papers*, I, p. 148). This, as the editor indicates, is the first use of the term, "psycho-analytic", and since the thirty-year-old Freud cannot yet know what will eventually turn out to be encompassed by this method, he simply identifies it with the "probing procedure" of Breuer.

[1896] In his paper "The Aetiology of Hysteria", published in the same year, Freud of course continues to refer to "Breuer's method" and starts with "the momentous discovery of J. Breuer: that the symptoms of hysteria (apart from stigmata) are determined by certain experiences of the patient's which operate traumatically and are reproduced in his psychic life as memory-symbols of these experiences". This is the paper in which he reports, without reservations, that "at the bottom of every case of hysteria will be found one or more experiences of premature sexual experience, belonging to the first years of childhood, which may be reproduced by analytic work though whole decades have intervened"—a judgment which he was of course to find mistaken and one which he was to retract and, courageously and imaginatively, to convert into the problem of why these traumatic experiences were so often a matter of phantasy. In it, he refers to "Breuer's method" on a half-dozen or so occasions, but we begin to see how he differentiates some of his own ideas from those of Breuer (*Collected Papers*, I, pp. 183-219).

[1904] By this time, Freud becomes clear and makes it clear to others how he has moved beyond Breuer: *e.g.* "The particular method of psychotherapy which Freud practises and terms psycho-analysis is an outgrowth [*n.b.*] of the so-called cathartic treatment discussed by him in collaboration with J. Breuer [...] At the personal suggestion of Breuer, Freud revived this method and tried it with a large number of patients [...] The changes which Freud introduced in Breuer's cathartic method of treatment were at first changes in technique; these, however, brought about new results and have finally necessitated a different though not contradictory conception of the therapeutic task." ("Freud's psycho-analytic method", *Collected Papers*, I, pp. 264-265, this being Freud's contribution to Löwenfeld's *Psychische Zwangerscheinungen*).

[1905] There is something of a regression here, from the newly perceived differentiation, when Freud refers to "that cathartic or psycho-analytic investigation, discovered by J. Breuer and me" ("Three contributions to the theory of sex", in *The Basic Writings of Sigmund Freud*, trans. and ed. by A. A. Brill [New York, Modern Library, 1938], p. 573).

[1905] But in the same year, Freud definitely dissociates himself from one of Breuer's ideas, saying that: "If, where a piece of joint work is in question, it is legitimate to make a subsequent division of property, I should like to take this opportunity of stating that the hypothesis of 'hypnoid states'—which many reviewers were inclined to regard as the central portion of our work—sprang entirely from the initiative of Breuer. I regard the use of such a term as superfluous and misleading [...]" ("Fragment of an analysis of a case of hysteria", *Collected Papers*, III, 35n.)

[1909] Attaching great importance to the international recognition accorded psycho-analysis by the invitation to speak at the celebration of the twentieth anniversary of Clark University, Freud was carried away, temporarily abandoning the distinctive roles he had gradually assigned Breuer and himself, and said unequivocally : "Granted that it is a merit to have created psycho-analysis, it is not my merit. I was a student busy with the passing of my last examinations, when another physician of Vienna, Dr. Joseph Breuer, made the first application of this method to a case of an hysterical girl (1880-1882)." Sigmund Freud, "Origin and development of psycho-analysis", *American Journal of Psychology*, XXI (1910), pp. 181-218, at 181. The paper, with this statement, appeared simultaneously in English and German and was soon translated into Dutch, Hungarian, Polish, Russian and Italian.

[1914] Five years later, Freud expressed second thoughts on the matter : "In 1909, in the lecture-room of an American university, I had my first opportunity of speaking in public about psycho-analysis. The occasion was a momentous one for my work, and moved by this thought I then declared that it was not I who had brought psycho-analysis into existence : the credit for this was due to someone else; to Joseph Breuer... Since I gave those lectures, however, well-disposed friends have suggested to me a doubt whether my gratitude was not expressed too extravagantly on that occasion. In their view, I ought to have done as I had previously been accustomed to do : treated Breuer's 'cathartic procedure' as a preliminary stage of psycho-analysis [...] It is of no great importance in any case [*n.b.* in the light of Freud's repeated worrying of the matter over a period of twenty years] whether the history of psycho-analysis is reckoned as beginning with the cathartic method or with my modification of it; I refer to this uninteresting point [*n.b.*] merely because certain opponents of psycho-analysis have a habit of occasionally recollecting that, after all, the art of psycho-analysis was not invented by me, but by Breuer. This only happens, of course, if their views allow them to find something in it deserving attention; if they set no such limits to their rejection of it, psycho-analysis is always without question my work alone. I have never heard that Breuer's great share in psycho-analysis has earned him a proportionate measure of criticism and abuse. As I have long recognized that to stir up contradiction and arouse bitterness is the inevitable fate of psycho-analysis, I have come to the conclusion that I must be the true originator of all that is particularly characteristic in it. I am happy to be able to add that none of the efforts to minimize my part in creating this much-abused analysis have ever come from Breuer himself or could claim any support from him.

"Breuer's discoveries [include a 'fragment of theory' holding that symptoms of hysteria] represented an abnormal employment of amounts of excitation which had not been disposed of (conversion). Whenever Breuer, in his theoretical contribution to the *Studies on Hysteria* (1895), referred to this process of conversion, he always added my name in brackets after it,* as though the priority for this first attempt at theoretical evaluation belonged to me. I believe that actually this distinction relates only to the name, and that the conception came to us simultaneously and together." ("On the history of the psycho-analytic movement", *Standard Edition of* [...] *Freud*, XIV, pp. 7-9).

[1924] Ten years later, Freud reverts to all this in a settled and consistent fashion, writing : "Soon after the publication of the studies in hysteria the collaboration of Breuer and Freud came to an end. Breuer, who was really a general practitioner, gave up the treatment of nervous diseases, while Freud took pains to further perfect the instrument left to him by his older colleague. The technical innovations which he initiated and the new discoveries which he made transformed the cathartic method into psycho-analysis." ("Psycho-analysis : exploring the hidden recesses of the mind", *These Eventful Years* [London and New York, 1924], II, 513). |

[1925] Freud's obituary of Breuer will be taken as a final source in point : "I have repeatedly attempted [...] to define

* The editor notes : "There seems to be some mistake here. In the course of Breuer's contribution he uses the term 'conversion' (or its derivatives) at least fifteen times. But only once (the first time he uses it, *Standard Ed.*, II, p. 206) does he add Freud's name in brackets. It seems possible that Freud saw some preliminary version of Breuer's manuscript and dissuaded him from adding his name more than once in the printed book." Whether this last conjecture is true or not, the fact attests once again Freud's abiding interest with matters of priority and its corollary, the meticulous effort to have 'credit' for originality properly allocated.

and again to his priority-conflict with Janet (36), reporting that he had brought the recalcitrant Breuer to agree to an early publication of their joint monograph because "in the meantime, Janet's work had anticipated some of his [Breuer's] results" (37); he writes nostalgically about the days of 'my splendid isolation' when "there was nothing to hustle me [...] My publications, which I was able to place with a little trouble, could always lag far behind my knowledge and could be postponed as long as I pleased, since there was no doubtful 'priority' to be defended" (38); he repeatedly allocates priorities among others (Le Bon, Ferenczi,

my share in the *Studies* which we published jointly. My merit lay chiefly in reviving in Breuer an interest which seemed to have become extinct, and in then urging him on to publication [...]. I found reason later to suppose that a purely emotional factor, too, had given him an aversion to further work on the elucidation of the neuroses. He had come up against something that is never absent—his patient's transference on to her physician, and he had not grasped the impersonal nature of the process [...]. Besides the case history of his first patient Breuer contributed a theoretical paper to the *Studies.* It is very far from being out of date; on the contrary, it conceals thoughts and suggestions which have even now not been turned to sufficient account. Anyone immersing himself in this speculative essay will form a true impression of the mental build of this man, *whose scientific interests were, alas, turned in the direction of our psychopathology during only one short episode of his long life.*" ("Josef Breuer", *Standard Edition*, XIX, 279-280).

This short synopsis of Freud's recurring attempts over a span of some forty years to distinguish his contributions from those of Breuer's suggests the possibility that, partly owing to the social pressures upon him to establish the nature of his own originality, he was not altogether uninterested in what he described as "of no great importance" and as an "uninteresting point"; not, at least, if matters of 'interest' are those which engage the attention.

(36) Of the many occasions on which Freud returned to this matter of Pierre Janet's claim to priority, I cite only "On the history of the psycho-analytic movement", *Standard Edition*, XIV, pp. 32-33; and *An Autobiographical Study*, pp. 21, 33, 54-55, where he seeks "to put an end to the glib repetition of the view that whatever is of value in psycho-analysis is merely borrowed from the ideas of Janet [...] Historically, psycho-analysis is completely independent of Janet's discoveries, just as in its content it diverges from them and goes far beyond them". For some of Janet's not always delicate insinuations, see his *Psychological Healing* (New York, Macmillan, 1925), I, pp. 601-640.

(37) *An Autobiographical Study*, pp. 36-37; "Josef Breuer", *Standard Edition*, XIX, pp. 279-80: "At the date of the publication of our *Studies*, we were able to appeal to Charcot's writings and to Pierre Janet's investigations, which had by that time deprived Breuer's discoveries of some of their priority. But when Breuer was treating his first case (in 1881-2) none of this was as yet available. Janet's *Automatisme psychologique* appeared in 1889 and his second work, *L'état mental des hystériques*, not until 1892. It seems that Breuer's researches were wholly original, and were directed only by the hints offered to him by the material of his case."

(38) "On the history of the psycho-analytic movement", *Standard Edition* [...] *of Freud*, XIV, p. 22. With regard to the pattern of biographers and disciples imposing their illusory convictions upon the actual experience of men of science, consider that the translation of this passage by A. A. Brill completely omits, presumably as inconsequential, the phrase: "There was no doubtful 'priority' to be defended." See *The Basic Writings of Sigmund Freud*, p. 943.

Bleuler, Stekel, being only a few among the many) (39); he even credits Adler with priority for an error (40); and, to prolong the types of occasions no further, he repeatedly intervenes in priority-battles among his disciples and current or former colleagues (for example, between Abraham and Jung) (41), saying that he could not "stifle the disputes about priority for which there were so many opportunities under these conditions of work in common" (42).

In view of even this small sampling of cases in point, it may not be audacious to interpret as a sign of resistance, Jones's remarkable statement that "Freud was never interested in questions of priority, which he found merely boring [...]" That Freud was ambivalent toward priority, true; that he was pained by conflicts over priority, indisputable; that he was concerned to establish the priority of others as of himself, beyond doubt; but to describe him as "never interested" in the question of priority and as "bored" by it requires the extraordinary feat of denying, as though they had never occurred, scores of occasions on which Freud exhibited profound interest in the question, many of these being occasions which Jones himself has detailed with the loving care of a genuine scholar. True, Freud appears to have been no more concerned with these matters than were Newton or Galileo, Laplace or Darwin, or any of the other giants of science about whom biographers and others have announced their entire lack of interest in priority just before, as honest scholars, they inundate us with a flood of evidence to the contrary. This denial of the realities they report and segregate seems to be an instance of that keeping of intellect and perception in abeyance which so typically reflects deep-seated resistance.

To propose that such resistance helps account for the studied neglect of systematic study of multiples and priority is still, of course, to leave open the question of what brings the resistance about. It would seem to have obvious parallels with other oc-

(39) References to these will be found scattered through Freud's publications and letters: *e.g. Group Psychology and Analysis of the Ego* (London, Hogarth Press, 1921), pp. 23-24, alludes to Le Bon having been anticipated by Sighele in his most important idea of "the collective inhibition of intellectual functioning and the heightening of affectivity in groups". On this case, see R. K. MERTON, introduction to Gustave Le Bon, *The Crowd* (New York, The Viking Press, 1960), pp. vii-xviii. To Ferenczi, he writes: "Your priority in all this is evident." JONES, *Freud*, III, pp. 353-4.

(40) Grimly, Freud writes in the midst of his counter-attack on the secessionist: "Adler must also be credited with priority in confusing dreams with latent dream-thoughts [...]" ("On the history of the psycho-analytic movement", *Standard Edition* [...], XIV, p. 57).

(41) JONES, *Freud*, II, pp. 52-56.

(42) "On the history of the psycho-analytic movement", *Standard Edition*, XIV, p. 25.

casions in the history of thought, not least with psycho-analysis itself, when amply available facts, having far-reaching theoretical implications, were experienced as unedifying or unsavory, ignoble or trivial and so were conscientiously ignored. It is a little like psychologists having once largely ignored sexuality because it was not a subject fit for polite society or having regarded dreams or incomplete actions as manifestly trivial and so undeserving of thorough inquiry.

What complicates the problem in the case of multiples and priority is that the study calls for detached examination of the behavior of some scientists by other scientists. Even to assemble the facts of the case is to be charged with blemishing the record of undeniably great men of science; as though one were a raker of muck that a gentleman would pass by in silence. Even more, to investigate the subject systematically is to be regarded not merely as a muck-raker, but as a muck-maker (43).

The behavior of fellow-scientists involved in multiples and priority-contests tends to be condemned or applauded rather than analyzed. It is morally evaluated, not systematically investigated. Disputes over priority are simply described as "unfortunate" and the moral judgment is substituted for the effort to understand what

(43) Historians of scientific and other ideas are nevertheless rebelling against bowdlerized versions of the life and work of scientists. George SARTON, for example, urges attention "to the long travail and maybe the suffering which led to each [discovery], the mistakes which were made, the false tracks which were followed, the misunderstandings, the quarrels, the victories and the failures; [...] the gradual unveiling of all the contingencies and hazards which constitute the warp and woof of living science". *A Guide to the History of Science* (Waltham, Mass., Chronica Botanica Co., 1952), p. 41. A. C. CROMBIE observes that "we must completely misunderstand Newton the man, and we run the risk of missing the essential processes of a mind so profoundly original and individual as his, if we exclude all those influences and interests that may be distasteful to us, or seem to us odd in a scientist. On closer examination it may turn out in fact that it was those very things that were his chief interest and that most profoundly affected his scientific imagination". ("Newton's conception of scientific method", *Bulletin of the Institute of Physics*, Nov. 1957, 350-362, at 361). And Jacques BARZUN finds merely tiresome the homilies that pass as descriptions of scientists at work, reminding us that "science is made by man, in the light of interests, errors and hopes, just like poetry, philosophy and human history itself. To say this is not to degrade science, as naive persons might think; it is on the contrary to enhance its achievements by showing that they sprang not from patience on a monument but from genius toiling in the mud". *Teacher in America* (Doubleday Anchor Books; Garden City, Doubleday, 1954), p. 90. As far back as the 1840's, Augustus de Morgan had complained about the "curious tendency of biographers [particularly of scientists] to exalt those of whom they write into monsters of perfection". No one could ever accuse de Morgan of this practice, particularly when he was writing about Newton. See his *Essays on the Life and Work of Newton* (Chicago, Open Court Publishing Co., 1914), pp. 62-63.

this implies for the psychology of scientists and the sociology of science as an institution. We find Goethe referring to "all those foolish quarrels about earlier and later discovery, plagiary, and quasi-purloinings" (44). We are free, of course, to find this behavior unfortunate or foolish or comic or sad. But these affective responses to the behavior of our ancestors or brothers-in-science seem to have usurped the place that might be given over to analysis of this behavior and its implications for the ways in which science develops. It is a little as though the physician were to respond only evaluatively to illness, describe it as unfortunate or painful, and consider his task done or as though the psychiatrist were to describe the behavior of schizophrenics as absurd and let it go at that or as though the criminologist were to substitute his sentiment that certain crimes are appalling and despicable for the effort to discover what brings these crimes about. The history of the sciences shows that the provisional emancipation from sentiment in order to investigate phenomena methodically has been a most difficult task, has occurred at different times in the various sciences and at different times for selected problems within each of the sciences. Emancipation from sentiment came fairly early in the history of much of medicine; it came very late in the history of treatment of the mentally ill and the analysis of criminal behavior. I suggest that only now are we beginning to emancipate the study of the concrete behavior of scientists from the altogether human tendency to respond to it in terms of the sentiments and values which we have made our own rather than to examine some of that behavior in reasonably detached fashion.

In regard to the study of multiples and priorities, apparently, we must remember again, what we all know in the abstract but are sometimes inclined to forget when we get down to new cases, that, as Clerk Maxwell noted, "It was a great step in science when men became convinced that, in order to understand the nature of things, they must begin by asking, not whether a thing is good or bad, noxious or beneficial, but of what kind it is ? and how much is there of it ? Quality and quantity were then first recognized as the primary features to be observed in scientific inquiry" (45).

Contributing to the substitution of sentiment for analysis and

(44) GOETHE's *Briefe, Werke* (Weimar, Hermann Boehlaus, 1903), XXVII, pp. 219-223. I am indebted to Aaron Noland, of the *Journal of the History of Ideas*, for calling my attention to this passage.

(45) James Clerk MAXWELL, "Relation of mathematics and physics", British Association for the Advancement of Science, Address, 1870.

so to the resistance against systematic study of multiples and their often connected disputes over priority is the often painful contrast between the actual behavior of scientists and the behavior ideally prescribed for them. The behavior of many scientists, when they are confronted with the fact that their discovery is "only" a rediscovery or, much worse, when confronted with the suggestion that it is a plagiary, scarcely matches the image of the dispassionate man of science, exclusively intent upon his scientific work. It is often seen as ugly, harsh and greedy for fame. And in the bitter social conflict that ensues, the standards governing behavior deteriorate. One or another of the discoverers caught up in a multiple—or often a colleague or fellow-national—suggests that he rather than his rival was really first, and that the independence of the rival is at least unproved. Grouping their forces, the other side counters with the opinion that plagiary had indeed occurred, that let him whom the shoe fits wear it and furthermore, to make matters quite clear, the shoe is on the other foot. Reinforced by group loyalties and sometimes by ethnocentrism, the controversy gains force, mutual charges of plagiary abound, and there develops an atmosphere of thoroughgoing hostility and mutual distrust (46).

This is not exactly in accord with the ideal image of scientists and particularly, of the greatest among them. When we identify ourselves with the role-models provided by great scientists of the past and by lesser as well as outstanding ones of the present, we find it painful to observe their behavior in these situations of conflict. Regarded in terms of values rather than of understanding, it may seem a bit sordid for a Galileo to "descend"—as the telling phrase has it—to seemingly egotistic attacks on one Grassi who tried "to diminish whatever praise there may be in this [invention of the telescope] which belongs to me"; or to go on to assail another who "attempted to rob me of that glory which was mine, pretending not to have seen my writings and trying to represent themselves as the original discoverer of these marvels"; or, finally, to say of a third that he "had the gall to claim that he had observed the Medicean planets [...] before I had [and used] a sly way of attempting to establish his priority" (47).

For all of us who harbor the ideal image of the scientist, it may be disconcerting to have Edmond Halley forthrightly described by

(46) MERTON, "Priorities in scientific discovery", *op. cit.* p. 653.

(47) GALILEO, *The Assayer*, 1623, trans. by Stillman Drake in *Discoveries and Opinions of Galileo* (New York, Doubleday, 1957), pp. 232-233, 245.

the first Astronomer Royal, John Flamsteed, as being just as "lazy and slothful as he is corrupt". And then, bringing an even greater name into the drama, going on to write :

With my lunar observations he [Halley] gives her true places and latitudes, which are copied from the three large synopses that I imparted to Sir Isaac Newton, under this condition that he should not impart them to anybody, without my leave. Yet so true to his word, and so candid is the Knight, that he immediately imparted it to Halley; who has printed them as far as they reach [...] the lazy and malicious thief would scarce be at the pains to gather them himself (48).

Or as Flamsteed put it most plainly, he found Newton, "always insidious, ambitious, and excessively covetous of praise" (49).

Almost all those firmly placed in the pantheon of science—a Newton, Descartes, Pascal, Leibniz or Huyghens, a Lister, Faraday, Laplace or Davy— have at one time or another been caught up in these fierce disputes. Nor has this been otherwise in the social and psychological sciences. As we know, sociology was officially born only after a long period of abnormally severe labor. The postpartum was not any more tranquil. It was disturbed by violent disputes between the followers of Saint-Simon and of Comte as they quarreled over the delicate question of which of the two was the father of sociology and which merely the obstetrician. At one time the secretary and research assistant of Saint-Simon, Comte became persuaded that his mentor had stolen his best ideas and ended by describing him as a "superficial and depraved charlatan" (50).

Nor do matters fare differently in other quarters of the emerging social sciences. We find the eighteenth-century Adam Ferguson replying to the charge of having plagiarized the lectures of his friend, Adam Smith, by admitting that "he had derived many notions from a French author, and that Smith had been there before him" (51). (Incidentally, this polemic pattern of "you

(48) Francis BAILEY, *An Account of the Revd John Flamsteed, the First Astronomer-Royal; Compiled from his own Manuscripts, and other Authentic Documents, never before published* (London, Printed by Order of the Lords Commissioners of the Admiralty, 1835), pp. 323-4. Much of this volume is devoted to the "notorious" and angry disputes over priority and intellectual property engaging Flamsteed and Newton and Halley, among others.

(49) *Ibid.* pp. 73-74.

(50) Frank E. MANUEL, *The New World of Henri de Saint-Simon* (Cambridge, Harvard University Press, 1956), pp. 340-2.

(51) William Robert SCOTT, *Adam Smith as Student and Professor* (Glasgow, Jackson, Son & Co., 1937), p. 119. In this circle of friends, Alexander Carlyle cites this equivocal 'defense' of Ferguson; cf. Carlyle's *Autobiography*, (Boston, Ticknor & Fields, 1860), p. 285. On Newton's use of swift counterattack, see his letter of 20 June 1686 to Halley in which he writes: "I am told by one who had it from another lately present at one of your meetings, how

too" or "you, rather than I" was by then well-established; Newton, for example, had adopted this kind of counter-attack in reply to Robert Hooke.) Another friend, Adam Robertson, is held to have made unacknowledged use of ideas which Smith had set forth in lectures and conversations, and "in order to establish priority", Smith is provoked into a public lecture which "gave a fairly long list of his new ideas" (52).

In the same vein, we find Saint-Simon ironically extending his "sincere thanks" to the historian Guizot for having "popularized the observations I published in the *Organisateur* [...]" and asking Guizot to read the letter of thanks "with great care [since] it is highly desirable, both for the public and for me, that he appropriate its content as fully as he did my first ideas [...]" (53).

Or again, we are at the turn of our century, listening to the long and bitter dispute between Gustave Le Bon and Scipio Sighele, in which the Italian complains that Le Bon "uses my observations on the psychology of crowds without citing me", and adds, "that without any trace of irony, I believe that no higher or less suspect praise can be given than by this adoption of my ideas without citing me" (54).

In another time and place, we find Lester Ward writing to E. A. Ross about Albion Small's *General Sociology* that "I suppose I ought to be amused instead of provoked. But a big volume filled with nothing but the things that you and I have been saying for years, only said over again in a verbose language which strains to avoid the particular words used by others and to palm off some other words for new ideas, is certainly exasperating" (55).

that Mr. Hooke should there make a great stir, pretending that I had it all from him, and desiring they would see that he had justice done him. This carriage toward me is very strange and undeserved; so that I cannot forbear in stating the point of justice, to tell you further, that he has published Borell[i]'s hypothesis in his own name; and the asserting of this to himself, and completing it as his own, seems to me the ground of all the stir he makes." The letter is reproduced in David Brewster, *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton* (Edinburgh, Thomas Constable, 1855), I, p. 442.

(52) Scott, *op. cit.* pp. 55, 101, 119.

(53) *The Doctrine of Saint-Simon : An Exposition. First Year, 1828-1829*, trans. by Georg G. Iggers (Boston, Beacon Press, 1958), p. 23n.

(54) Scipio Sighele, *La foule criminelle : essai de psychologie collective*, 2e éd. (Paris, Alcan, 1901), Pt. II, chap. II, under the title "physiologie du succès", which is introduced by a note stating that the chapter first appeared in *Revue des Revues*, 1 Oct. 1894, the date being cited to safeguard his priority from Le Bon.

(55) In a letter of 18 March 1906, reprinted in "The Ward-Ross Correspondence, IV, 1906-1912", edited by B. J. Stern, *American Sociological Review*, XIV (1949), pp. 88-119, at 90. In his reply, Ross writes that "I agree with you about Small's book. Having no quarrel with the matter of the book I resolutely shut my eyes to the form. But there is no denying that the cloudiness and prolixity

And so it goes, on and on. Ignoring for the moment the great volume of such angry complaints in the physical and life sciences, we hear Comte denouncing Saint-Simon and the Saint-Simonians, Comte; Spencer in turn upbraiding the Comtists for holding him to be a mere imitator of Comte (56); Marx berating Hyndman as an out-and-out robber of his ideas (57); the usually equable Gaetano Mosca fuming at "the Marquess Pareto" over his double crime of first having appropriated Mosca's theory of "the political class" and then re-christening the idea by the far more popular term, "elite" (58); Jungians accusing Freud (59), Freud accusing Adler (with the further and by now familiar charge that the borrowed ideas become "labelled as his own by a change in nomenclature") (60), and Adlerians accusing Freud and a variety of others (61).

As we approach our own day in the social sciences, we hear echoes of these angry and agitated words reverberating through the

will hurt the book with the public and may give sociology something of a black eye. Already I notice a feeling of 'If this be sociology, Good Lord deliver us'. However sociology has endured many things like it and my faith in its ultimate triumph never wavers". *Ibid.* p. 93.

(56) For example, in David DUNCAN, *The Life and Letters of Herbert Spencer* (London, Methuen and Co., 1908), Appendix B, pp. 565-572.

(57) On this, see Isaiah Berlin, *Karl Marx* 2d ed. (London, Oxford University Press, 1948), p. 267. Berlin goes on to note : "Marx held violent opinions on plagiarism" as we know from his unrestrained attacks on Malthus and Bastiat, among others.

(58) This prolonged conflict over priority rankled enough for Mosca to return to it over a span of more than thirty-five years. A detailed account will be found in chapter VIII of James H. MEISEL, *The Myth of the Ruling Class : Gaetano Mosca and the 'Elite'* (Ann Arbor, University of Michigan Press, 1958), Mosca did not hesitate to note that other contemporaries, among them J. Novicov and Otto Ammon, had independently reached much the same conclusions but that for these and other ideas, " 'the only case in which I was not able to convince myself of that same spontaneity is that of Professor Pareto' ". Mosca then goes on to explain : " 'Plagiarism in the social sciences cannot be as easily established as in literary productions, because what matters most in the former is the concept, not the form, and it is always possible to repeat and to reproduce a concept by changing words around [...] An educated and shrewd man may always introduce modifications and even add a little something of his own.' " Quoted by Meisel, p. 173.

(59) For one detailed account of this polemic, see Edward GLOVEG, *Freud or Jung* (New York, W. W. Norton, 1950).

(60) See Freud's all-out attack on Adler in which he says, among much else, "At the Vienna Psycho-analytical Society we once actually heard him claim priority for the conception of the 'unity of the neuroses' and for the 'dynamic view' of them. This came as a great surprise to me, for I had always believed that these two principles were stated by me before I ever made Adler's acquaintance." "On the history of the psycho-analytic movement", *Standard Edition [...] of Freud*, XIV, pp. 51-58.

(61) See, for example, Heinz L. and Rowena R. ANSBACHER, *The Individual Psychology of Alfred Adler* (New York, Basic Books, 1956), and the counter-attack by David Rapaport on it and on a review by R. W. White who "in certain respects" gave the palm to Adler, *Contemporary Psychology*, November 1957, II, pp. 303-4. See also JONES, *Freud*, III, p. 296.

corridors of the peaceful temple of science. Since these episodes involve our contemporaries and often our associates, they become, we must suppose, even more painful to observe and more difficult to analyze with detachment than episodes of the distant past. Only a few presentday conflicts over priority in psychology and sociology, with their intimations or outright assertions of unacknowledged borrowing, need be reviewed to reinstitute the embarrassment and wriggling discomfort experienced by social scientists who are onlookers.

J. L. Moreno, for example, and S. R. Slavson are deep in conflict over the question of who originated group psychotherapy, with Moreno describing Slavson as "liking my concepts and terms group therapy and group psychotherapy and a few years later [beginning] to use them without quotation" (62). Slavson, in his turn, retorts that Moreno was not really the inventor of psychodrama and that priority actually belongs to Karl Joergensen of Sweden, thus following, perhaps unwittingly, the established practice of countering with the claim of a still earlier priority (63). Or again, Moreno maintains that his ideas and some of Kurt Lewin's are not really cases of independent multiple discovery and by the time Lewin had published his work on group or action dynamics, he, Moreno, was "the acknowledged leader" "in the new developments of action and group theory" (64). Some of Lewin's students, Moreno goes on to say, "attended his workshops" and adopted his ideas and techniques under camouflaging labels employed in group dynamics. As Moreno puts it, in colorful language reminiscent of that we have seen employed by angry scientists of the past :

> It was a shrewd device to plant, at least in the mind of *some* people, the idea that by sheer coincidence of circumstances the same ideas developed independently. By using a technique of quoting only each other, that is, those who belong to their clique, and not quoting any of my close associates or myself, their double game became the laughing stock of the connoisseurs [...] (65).

(62) J. L. MORENO, *Who Shall Survive ? Foundations of Sociometry, Group Psychotherapy and Sociodrama* (Beacon, New York, Beacon House, Inc., 1953), 2d ed., p. lxi. Moreno goes on to observe : "He who claims priority (which is a form of superiority), however justified, becomes unpopular with the majority", p. lxii. Moreno's position is defended at length in Didier ANZIEU, *Le psychodrame analytique chez l'enfant* (Paris, Presses Universitaires de France, 1946), pp. 28-29.

(63) S. R. SLAVSON, "A preliminary note on the relation of psychodrama and group psychotherapy", *International Journal of Group Psychotherapy*, V (1955), 361-66. And see the reply by Joseph MEIERS, "Scandinavian myth about psychodrama : a counter-statement to S.R. Slavson's 'preliminary note' ", *Group Psychotherapy*, X (1957), 349-52.

(64) MORENO, *op. cit.* p. ci.

(65) *Ibid.* p. cii.

In much the same fashion but covering a broader scope, Pitirim Sorokin attacks what he describes as "amnesia and the discoverer's complex" in modern sociology and psychosocial science. He flails social scientists who he sees as having borrowed from past observers without acknowledgement and aims his heaviest guns at those he regards as having filched ideas from their contemporaries which are then put forward as their own. Once again, the language is that of an angry Galileo, Flamsteed or Hooke. Thus, the concept of the "basic personality structure" is described as "a vague variation of a very old concept 'pilfered' from sociologists" (66). Leopold von Wiese is approvingly quoted as having written that certain social theorists have "a strange lack of references to their predecessors" and, despite the "essential similarity" of sociological framework, they have a "complete lack of references to theories of mine [Sorokin's] published many years before [...]" (67). Most often, Sorokin says, claims to priority are probably due to "the ignorance of our pseudo-discoverers, many of whom are newcomers from other fields", such as statistics, and have failed to live up to the obligation to find out what has gone before (68). Beyond this merely ignorant group, he writes, the "wouldbe Columbuses" of social science today include "an insignificant fraction of deliberate plagiarists". Some of these

pseudo-discoverers are the victims of ambitions far exceeding their creative potential and of our society's competitive mores and its cult of success. Driven by their Narcissistic complex and by the ever-operating forces of rivalry, they are eager to overestimate their archievements, to advertise them as 'discoveries made for the first time', and with a semirational naïveté they are apt sincerely to fool themselves and others with their claims (69).

And finally, almost as an echo of Ward writing to Ross about Small, or of Mosca writing about Pareto, or Freud about Adler, Sorokin refers to the "technique of using new terms for old concepts to give them a look of originality. These and similar devices help to sell, especially to a credulous public, the old intellectual merchandise as the new" (70).

In the climate created by such denunciations, even the social scientists who are not directly involved, at least for the moment, feel acutely uncomfortable. Uneasy and distressed, they can hardly bring themselves to study this behavior. For when socio-

(66) Pitirim A. SOROKIN, *Fads and Foibles in Modern Sociology and Related Sciences* (Chicago, Henry Regnery Co., 1956), p. 13.

(67) *Ibid.* p. 14.

(68) *Ibid.* p. 17.

(69) *Ibid.* p. 19.

(70) *Ibid.* p. 19.

logical analysis is stripped bare of sentiment, it often leaves the sociologist shivering in the cold. Since his own sentiments and allegiances are involved, it becomes all the more difficult to examine the hot conflicts of associates with required detachment. The sociological and psychological study of multiples and priorities accordingly tends to remain undeveloped.

The disputants themselves manifest ambivalence toward their own behavior. Even while he is assembling documents to prove his priority, for example, Darwin registers his mixed feelings, writing Lyell : "My good dear friend, forgive me. This is a trumpery letter, influenced by trumpery feelings." In a postscript he assures Lyell that "I will never trouble you or Hooker on the subject again". The next day, he writes : "It seems hard on me that I should lose my priority of many years' standing." Then, a few days later, he writes again to say : "Do not waste much time [on this matter]. It is miserable in me to care at all about priority" (71).

Freud also recognizes his own ambivalence when he writes of his work on the Moses of Michelangelo that, having come upon a little book (of 46 pages) published in 1863 by an Englishman, Watkiss Lloyd, he read it

> with mixed feelings. I once more had occasion to experience in myself what unworthy and puerile motives enter into our thoughts and acts even in a serious cause. My first feeling was of regret that the author should have anticipated so much of my thought, which seemed precious to me because it was the result of my own efforts ; and it was only in the second instance that I was able to get pleasure from its unexpected confirmation of my opinion. Our views, however, diverge on one very important point (72).

Ambivalence is otherwise expressed when Moreno concludes his assault on those who do not acknowledge his originality of conception. He remarks that the

> motives for exposing interpersonal conflicts with former associates have little to do with 'priority' or 'recognition'. My craving for ego-satisfaction, for 'being loved and admired', has been comfortably reciprocated. If a father of ideas gets fifty percent-returns he can consider himself lucky, and I got more than this (73).

Almost it is as though we were once again reading Descartes as he manages to write both that he "does not boast of being the first discoverer" and then proceeding on other occasions to insist

(71) *The Life and Letters of Charles Darwin*, ed. by Francis Darwin (New York, D. Appleton & Co., 1925), I, pp. 474-476.

(72) "The Moses of Michelangelo", in FREUD, *Collected Papers*, IV, pp. 284-5.

(73) MORENO, *op. cit.* p. cvi.

on his priority over Pascal or to write his friend Mersenne: "I also beg you to tell him [Hobbes] as little as possible about [...] my unpublished opinions, for if I'm not greatly mistaken, he is a man who is seeking to acquire a reputation at my expense and through shady practices" (74).

The ambivalence toward claims of priority means that scientists are contemptuous of the very attitudes they have acquired from the institution to which they subscribe. The sentiments they have acquired from the institution of science, with its great premium on originality, makes it difficult to give up a claim to a new idea or new finding. Yet the same institution emphasizes the selfless dedication to the advancement of knowledge for its own sake. Concern with priority and ambivalence toward that concern register in the individual what is generated by the value-system of science (75).

The self-contempt often expressed by scientists as they observe with dismay their own concern with having their originality of discovery recognized is evidently based upon the widespread though uncritical assumption that behavior is actuated by a single motive, which can then be appraised as good or bad, as noble or ignoble. They assume that the truly dedicated scientist must be concerned only with advancing knowledge. As a result, their deep interest in having their priority of discovery recognized by peers is seen as marring their nobility of purpose as men of science (although it might be remembered that 'noble' means the widely-known). This assumption has a germ of psychological truth: any reward—money, fame, position—is morally ambiguous and potentially subversive of culturally esteemed motives. For as rewards are meted out,—fame, for example,—the motive of seeking the reward can displace the original motive, concern with recognition can displace concern with advancing knowledge (76). But this is only a possibility, not an inevitability. When the institution of science works effectively, and like other social institutions it does not always do so, recognition and esteem accrue to those scientists who have best fulfilled their roles, to those who have made important contributions to the common stock of knowledge. Then

(74) DESCARTES, *Œuvres* (edited by Charles Adam and Paul Tannery), *Correspondance*, III (Paris, 1899), 283 ff.; V (1903), 366.

(75) Lionel Trilling has observed that the "scientist also loves fame, but illicitly: it is not in accord with his professional legend that he should do so, and he is ashamed if his guilty passion is discovered". *A Gathering of Fugitives* (Boston, Beacon Press, 1956), pp. 143-144.

(76) On the displacement of goals, see MERTON, *Social Theory and Social Structure*, pp. 199-200.

are found those happy circumstances in which moral obligation and self-interest coincide and fuse. The observed ambivalence of scientists toward their own interest in having their priority recognized—an ambivalence we have seen registered even by that most astute of psychologists, Freud—shows them to assume that such an ancillary motive somehow tarnishes the 'purity' of their interest in scientific inquiry. Yet it need not be that scientists seek only to win the applause of their peers but, rather, that they are comforted and gratified by it, when it does ring out.

Occasionally, a scientist senses all this and vigorously challenges the assumption underlying the shame over interest in recognition; for example, a Hans Selye who asks his peers :

Why is everybody so anxious to deny that he works for recognition ? In my walk of life, I have met a great many scientists, among them some of the most prominent scholars of our century; but I doubt if any one of them would have thought that public recognition of his achievements—by a title, a medal, a prize, or an honorary degree—played a decisive role in motivating his enthusiasm for research. When a prize brings both honor and cash, many scientists would even be more inclined to admit being pleased about the money ('one must live') than about the public recognition ('I am not sensitive to flattery'). Why do even the greatest minds stoop to such falsehoods ? For, without being conscious lies, these ratiocinations are undoubtedly false. Many of the really talented scientists are not at all money-minded; nor do they condone greed for wealth either in themselves or in others. On the other hand, all the scientists I know sufficiently well to judge (and I include myself in this group) are extremely anxious to have their work recognized and approved by others. Is it not below the dignity of an objective scientific mind to permit such a distortion of his true motives ? Besides, what is there to be ashamed of ? (77).

Dr. Selye's final question need not remain a rhetorical one. Shame is experienced when one's identity and self-image are suddenly violated by one's actual behavior—as in the case of the shame we have seen expressed by Darwin when his own behavior forced him to realize that recognition of his priority meant more to him than he had ever been willing to suppose. To admit to a deep-seated wish for recognition may seem to prefer recognition to the joy of discovery as an end in itself, activating the further awareness that the pleasure of recognition for accomplishment could, and perhaps momentarily did, replace the pleasure of scientific work for its own sake.

On the surface, this hunger for recognition appears as mere personal vanity, generated from within and craving satisfaction from without. But this is truly a superficial diagnosis, compounded of a moralizing deprecation of self or others and representing a

(77) Hans SELYE, *The Stress of Life* (New York, McGraw-Hill, 1956), p. 288.

classic instance of the fallacy of misplaced concreteness in which relevant sociological details are suppressed by exclusive attention to the feeling-states of the particular individual scientist. When we reach deeper and wider, into the institutional complex that gives point to this hunger for recognition, it turns out to be anything but personal and individual, repeated as it is with slight variation by one scientist after another. Vanity, so-called, is then seen as the outer face of the inner need for assurance that one's work really matters, that one has measured up to the hard standards maintained by a community of scientists. It then becomes clear that the institution of science reinforces, when it does not create, this deep-rooted need for validation of work accomplished. Sometimes, of course, the need is stepped up until it gets out of hand : the desire for recognition becomes a driving lust for acclaim (even when unwarranted), megalomania replaces the comfort of reassurance. But the extreme case need not be mistaken for the modal one. In general, the need to have accomplishment recognized, which for the scientist means that his knowing peers judge his work worth the while, is the result of deep devotion to the advancement of knowledge as an ultimate value. Rather than necessarily being at odds with dedication to science, the concern with recognition is ordinarily a direct expression of it. This becomes evident only if one does not stop analysis by characterizing this concern as a matter of vanity or self-aggrandizement but goes on to consider that, sociologically, recognition of accomplishment by informed others represents a mechanism of social validation of that accomplishment. Science in particular is a social world, not an aggregate of solipsistic worlds. Continued appraisal of work and recognition for work well done constitute one of the mechanisms that unite the world of science.

### 3. *The eureka syndrome.*

All this can be seen in a somewhat different context : the deep concern with establishing priority or at least independence of discovery is only the other side of the coin of the socially reinforced elation that comes with having arrived at a new and true scientific idea or result. And the deeper the commitment to a discovery, the greater, presumably, the reaction to the threat of having its novelty denied. Concern with priority is often only the counterpart to elation in discovery—the eureka syndrome. We have only to remember what is perhaps the most ecstatic expression of joy in discovery in the annals of science : here is Kepler on his discovery of the third planetary law :

What I prophesied 22 years ago as soon as I found the heavenly orbits were of the same number as the five (regular) solids, what I fully believed long before I had seen Ptolemy's Harmonics, what I promised my friends in the name of this book, which I christened before I was 16 years old, what I urged as an end to be sought, that for which I joined Tycho Brahe, for which I settled in Prague, for which I have spent most of my life at astronomical calculations—at last I have brought to light, and seen to be true beyond my fondest hopes. It is not 18 months since I saw the first ray of light, three months since the unclouded sun-glorious sight burst upon me! Let nothing confine me; I will indulge my sacred ecstasy. I will triumph over mankind by the honest confession that I have stolen the golden vases of the Egyptians to raise a tabernacle for my God far away from the lands of Egypt. If you forgive me, I rejoice; if you are angry, I cannot help it. The book is written; the die is cast. Let it be read now or by posterity, I care not which. It may well wait a century for a reader, as God has waited 6000 years for an observer (77 *a*).

We can only surmise how deep would have been Kepler's anguish had another claimed that he had long before come upon the third law. So, too, with a Gay-Lussac, seizing upon the person nearest him for a victory waltz so that he could "express his ecstasy on the occasion of a new discovery by the poetry of motion" (78). Or, to come closer home, William James "all aflame" with his idea of pragmatism and hardly able to contain his exhilaration over it (79). Or, in more restrained exuberance, Joseph Henry, once he had hit upon a new way of constructing electro-magnets, reporting that "when this conception came into my brain, I was so pleased with it that I could not help rising to my feet and giving it my hearty approbation" (80). Or finally, the young Freud writing his "darling girl", Martha, of his "joy" in a "discovery which may not be insignificant" : a new technique of staining nervous tissue with a solution of gold chloride (81), or, years later,

(77 *a*) As translated in William S. Knickerbocker, ed., *Classics of Modern Science* (New York, Knopf, 1927), p. 30.

(78) Edward THORPE, C.B., L.L.D., F.R.S., *Essays in Historical Chemistry* (London, Macmillan, 1931).

(79) See James's letter to Flournoy, *in* Ralph Barton PERRY, *The Thought and Character of William James* (Boston, Little, Brown, 1936), II, p. 452.

(80) Thomas COULSON, *Joseph Henry : His Life and Work* (Princeton, Princeton University Press, 1950), 49-50. The self-effacing Henry, it will be remembered, was periodically involved in multiples and, on occasion, in disputes over priority as evidenced not least in his candid report of great disappointment over Faraday's having been regarded as the prior discoverer of electro-magnetic induction.

(81) *Letters of Freud*, *op. cit.* p. 72.

reminding Karl Abraham that "we have the incomparable pleasure of gaining the first insights" (82).

In short, when a scientist has made a genuine discovery, he is as happy as a scientist can be. But the peak of exhilaration may only deepen the plunge into despair should the discovery be taken from him (83). If the loss is occasioned only by finding that it was, in truth, not a first but a later independent discovery, the blow may be severe enough, though mitigated by the sad consolation that at least the idea has been confirmed by another. But this is as nothing, of course, when compared with the traumatizing charge that not only was the discovery later than another of like kind but that it really was borrowed or even stolen. Rather than being mutually exclusive, joy in discovery and eagerness for recognition by scientific peers are stamped out of the same psychological coin. They can both express a basic commitment to the value of advancing knowledge.

These complex patterns of behavior persist and make for resistance to the detached and systematic study of multiples and priorities in science. Yet the resistance may be decreasing, if we can judge from what appears to be a declining tendency to engage in conflicts over priority in cases of multiple discoveries. This, at least, is one preliminary result of a methodical study of the subject. From among the multitude of multiples, Dr. Elinor Barber and I have undertaken to examine 264 intensively. Of the 36 multiples before 1700 in this list, 92 % were the object of strenuous conflicts over priority; this figure drops to 72 % in the 18th century; remains at about the same level (74 %) in the first half of the 19th century and declines to 59 % in the latter half, reaching a low of 33 % in the first half of this century. It may be that scientists are becoming more fully aware that with vastly enlarged numbers of investigators at work in each field of science, a discovery is apt to be made by others as well by themselves.

### *Cryptomnesia ("Unconscious Plagiary").*

Further complicating the already complex emotions that attend multiple discoveries is the phenomenon of so-called "unconscious

(82) *Ibid.* p. 286.

(83) For an example, witness the account sent to Gauss by Schumacher, of Niels Abel's dismay upon learning that he had been anticipated by Jacobi, with Abel needing some brandy to sustain himself, and Schumacher's concluding remark : "Wenn Sie einmal Ihre Untersuchungen bekannt machen, wird es ihm wahrscheinlich noch mehr an Schnapps kosten."

plagiary". Interestingly enough, the potpourri term itself testifies to the admixture of moralizing and analysis that commonly enters into discussions of the subject. It is compounded of an loosely-conceived psychological component ("unconscious") and a legal-moralistic component ("plagiary", with all its connotations of violating a code and attendant guilt). As a concept, "unconscious plagiary" is just as misplaced or obsolete in psychosocial studies as is that of insanity, which was rightly relegated to the sphere of law, where it continues to lead a harrowing existence. The neutral and analytical term, cryptomnesia, serves us better, referring as it does to seemingly creative thought in which ideas based upon unrecalled past experience are taken to be new.

The fact that cryptomnesia can occur at all subjects the scientist (or other creative minds) to the ever-present possibility that his most cherished original idea may actually be the forgotten residue of what he had once read or heard elsewhere. This fear may give rise to either of two conflicting patterns of behavior : in some cases, it may lie behind the emphatic insistence of an imaginative mind that he is beholden to no one else for his newfound ideas (84). This pattern of a possibly cryptomnesic scientist who protests-his-originality-too-much, not knowing whether he is right or not, differs of course from the pattern of the-lady-who-doth-protest-too-much, knowing as she does that her act will belie her words. In other cases, the scientist who knows that cryptomnesia can occur may assume that he has unwittingly assimilated an idea which he once believed to have been original with him. This may hold for big ideas or small ones. I know that the statistician, W. Allen Wallis, will not mind my citing such a minor episode from his experience. In the well-known textbook of statistics which Harry Roberts and he published in 1956, they introduced the convenient practice of numbering tables and charts, not seriatim as is ordinarily done, but by the number of the page on which they appear. This has the advantage that later cross-references to the tables or charts at once indicate the page on which they are found. This useful little idea also turns out to be a multiple. The book is published and Wallis soon receives a friendly letter from an economist notifying him that this system of numbering had been employed

*Briefwechsel zwischen C. F. Gauss und H. C. Schumacher*, C.A.F. Peters, ed. (Altona, Gustav Esch, 1860), II, p. 179.

(84) But to take one of the most familiar cases, it is by no means clear that Montesquieu intended by his motto—*Prolem sine matre creatam*—that the *Spirit of the Laws* was only a source and indebted to none before him.

by Dunlap and Kurtz in a handbook of statistics published back in 1932. Wallis's reply exemplifies the uncertainty that comes from realizing that cryptomnesia is an ever-present possibility :

> I was much interested in your letter [...] The numbering method seemed to me so good that it obviously must have been thought of before [...] The Dunlap and Kurtz book is, I am virtually certain, in my office in Chicago. When I arrived at Chicago ten years ago as Ted Yntema's successor, he very kindly left a considerable portion of his statistical library for me. I have noticed this volume, though I do not recall ever looking in it. Nevertheless, there does seem to me to be a real possibility that on some occasion I did look at it, note the numbering system, forget it, but then did think it up 'fresh' when faced with a numbering problem (85).

What holds for this little instance holds also for discoveries of consequence to science : the possibility of cryptomnesia leads some to doubt their own powers of recall and to assume that what they once thought to be their original idea may be, after all, the trace of a forgotten exposure to the idea as set forth by another.

Among the many cases in point, consider only these few. Having had the experience at age nineteen of learning that his discovery in optics was 'only' a rediscovery, William Rowan Hamilton, the mathematical genius who discovered quaternions (in part, independently invented by Grassmann), developed a lifelong preoccupation with the twin fear of being plagiarized and of unwittingly plagiarizing others. As he put it on one of the many occasions on which he turned to this subject in his correspondence with de Morgan : "As to myself, I am *sure* that I *must* have often reproduced things which I had read long before, without being able to identify them as belonging to other persons" (86). Or again : "But about the 'sighing'—am I to quarrel with Dickens, or figure in one of his publications of a later date ? Where is the priority business to end ? I am sick of it as you can be ; but still, in anything important as regards science, I should take it as a favour to be *warned*, if I were inadvertently exposing myself to the charge of plagiarising" (87).

Turning from mathematics to psychology, we find Freud charac-

(85) W. Allen Wallis, personal communication.

(86) R. P. GRAVES, *Life of Sir William Rowan Hamilton* (Dublin, Hodges, Figgis, 1882), III, p. 297. This extensive biography includes scores of letters by Hamilton which report his pervasive concern with matters of priority, rediscovery, the giving of credit for originality in science, plagiary, scientists' desire for immortal fame, anticipations, fear of being forestalled, *etc.* As de Morgan observed, Hamilton was obsessed by possible cryptomnesia : "He had a morbid fear of being a plagiarist ; and the letters which he wrote to those who had treated like subjects with himself sometimes contained curious and far-fetched misgivings about his own priority." *Ibid.* III, p. 217.

(87) *Ibid.* III, p. 368.

teristically examining his own experience, remembering that he had been given Börne's works when he was fourteen and still had the book fifty years later, so that although "he could not remember the essay in question", which dealt with free association as a procedure for creative writing, "it does not appear impossible to us that this hint may perhaps have uncovered that piece of cryptomnesia which, in so many cases, may be suspected behind an apparent originality" (88). In reviewing the multiple discovery of part of the "theory of dreams" by Popper-Lynkeus and himself, Freud has this to say :

> [...] the subjective side of originality also deserves consideration. A scientific worker may sometimes ask himself what was the source of the ideas peculiar to himself which he has applied to his material. As regards some of them he will discover without much reflection the hints from which they were derived, the statements made by other people which he has picked out and modified and whose implications he has elaborated. But as regards others of his ideas he can make no such acknowledgements ; he can only suppose that these thoughts and lines of approach were generated—he cannot tell how—in his own mental activity, and it is on them that he bases his claim to originality.
>
> Careful psychological investigation, however, diminishes this claim still further. It reveals hidden and long-forgotten sources which gave the stimulus to the apparently original ideas, and it replaces the ostensible new creation by a revival of something forgotten applied to fresh material. There is nothing to regret in this ; we had no right to expect that what was 'original' could be untraceable and undetermined.
>
> In my case, too, the originality of many of the new ideas employed by me in the interpretation of dreams and in psycho-analysis has evaporated in this way. I am ignorant of the source of only one of these ideas ['dream-censorship'] (89).

Most incisively, Freud exemplifies the basic uncertainty inherent in the fact that cryptomnesia *can* occur, when he writes in "Analysis Terminable and Interminable" :

> My delight was proportionally great when I recently discovered that that theory [of the 'death instinct'] was held by one of the great thinkers of ancient Greece. For the sake of this confirmation I am happy to sacrifice the prestige of originality, especially as I read so widely in earlier years that I can never be quite certain that what I thought was a creation of my own mind may not really have been an outcome of cryptomnesia (90).

It was this sort of thing, no doubt, that prompted the irrepressible Mark Twain to declare : "What a good thing Adam had —when he said a thing he knew nobody had said it before."

(88) Quoted from Freud's anonymous paper, "A note on the pre-history of the technique of analysis", by Lewis W. Brandt in his instructive paper dealing with Schiller as a possibly cryptomnesic source for Freud : "Freud and Schiller", *Psychoanalysis and the Psychoanalytic Review*, Winter 1960, 46, 97-101.

(89) Freud, "Joseph Popper-Lynkeus and the theory of dreams", *Standard Edition* [...] *of Freud*, XIX, 261. This same passage is translated from the German in the paper by Brandt.

(90) I am indebted to Lewis W. Brandt for calling my attention to this passage.

Celebrated cases of seeming cryptomnesia abound in all fields of creative work. To take only one dramatic example, Helen Keller writes in despair of having published a story that was "so much alike in thought and language [to another] that it was evident" the earlier one must have been read to her and that "mine was a—" [even in print, she pauses, draws a deep breath and only then, can bring herself to say] that "mine was a—plagiary". "No one drank deeper of the cup of bitterness than I did", she concludes, and concludes this although it proved impossible to find anyone who had actually read her the story (91).

Contributing further to the uncertainty about the extent of one's originality is the recurrence of episodes in which a scientist has unwittingly borrowed ideas from himself. Many scientists and scholars have found, to their combined chagrin and disbelief, that an idea which seemed to have come to them out of the blue had actually been formulated by them years before, and then forgotten. An old notebook, a resurrected paper, a colleague cursed with total recall, a former student—any of these can make it plain that what was thought to be a new departure was actually a repetition (or at most, an extended and improved version) of what they had worked out for themselves in the past. Of many such cases, consider only a few, some of a century or more ago, others of contemporary vintage :

Joseph Priestley records with chagrin that "I have so completely forgotten what I have myself published, that in reading my own writings, what I find in them often appears perfectly new to me, and I have more than once made experiments, the results of which had been published by me" (92).

The ingenious and jovial mathematician, Augustus de Morgan, has his own lively version of the experience : "I have read a Paper (but not on mathematics) before now, have said to myself, I perfectly agree with this man, he is a very sensible fellow, and have found out at last that it was an old Paper of my own I was reading, and very much flattered I was with my own unbiased testimony to my own merits" (93).

And it is told of the distinguished mathematician, James Joseph Sylvester, that he "had difficulty in remembering his own inventions and once even disputed that a certain theorem of his own could possibly be true" (94).

Or consider a brace of cryptomnesic borrowings from self in our own day :

The Nobel Laureate, Otto Loewi, reports having waked in the middle of the night, jotting down some notes on what he sensed to be a momentous discovery,

(91) Helen KELLER, *The Story of My Life* (New York, Doubleday, Page and Co., 1908), 63-72, at 65.

(92) *Life of Priestley*, Centenary Edition, p. 74.

(93) Letter of Augustus de Morgan to W. R. Hamilton, in GRAVES, *Hamilton*, *op. cit.* III, p. 494.

(94) E.T. BELL, *Men of Mathematics* (New York, Simon and Schuster, 1937), p. 386.

going back to sleep, awaking to find that he could not possibly decipher his scrawl, spending the day in a miserable and unavailing effort to remember what he had had in mind, being again aroused from his slumber at three the next morning, racing to the laboratory, making an experiment and two hours later conclusively proving the chemical transmission of nervous impulse. So far, so good : another case, evidently, of the pattern of subconscious creativity unforgettably described by Poincaré. But some years later, when Loewi, upon request, reported all this to the International Physiological Congress, he was reminded by a former student that, eighteen years before that nocturnal discovery, he had fully reported his basic idea. "This", says Loewi, "I had entirely forgotten" (95).

The psychologist Edwin Boring writes me of a colleague who came to him in an excited Eureka frame of mind, announcing that he had just worked out a new technique for scales of sensory measurement, and that he is now hunting for a name for it. And then, before "the shine of the new idea had rubbed off, he discovers that he had discussed this in print some six years before and had even given it a tentative name".

And to advert to Freud, as I have so often done if only because his intellectual experience is uncommonly documented, Jones reports several instances of his "obtaining a clear insight which he subsequently forgot, and then later suddenly coming across it again as a new revelation" (96). As Freud noted in another connection, "it is familiar ground that a sense of conviction of the accuracy of one's memory has no objective value [...]" (97).

If cryptomnesia is possible in relation to one's own earlier work, then it is surely possible in relation to the work of others. And this can undermine the calm assurance that one has, in truth, worked out a new idea for oneself when confronted with another version of the same idea worked out by someone else.

Various contexts may affect the probability of cryptomnesia in relation to one's own work. It may be the more probable, the more the scientist has worked in a variety of problem-areas rather than narrowly restricting his research focus to problems having marked continuity. Looking at this hypothesis, not in terms of the individual scientist but in terms of the relative frequency of self-cryptomnesia in different sciences, we should expect it to be more frequent in the newer sciences, with their large variety of

(95) Otto Loewi, *From the Workshop of Discoveries* (Lawrence, Kansas, University of Kansas Press, 1953), 33-34.

(96) Jones, *Freud, op. cit.* III, p. 271. One such case, for example, is Freud's conception of paranoid jealousy as an instance of repressed homosexuality.

(97) This observation appears in his paper of 1913, on "Fausse Reconnaissance ('Déjà raconté') *in* psycho-analytic treatment", *Collected Papers*, II, pp. 334-341. This same paper, devoted to paramnesia, has Freud reporting a multiple discovery and assuring the reader (and himself) that it is just that, and not a case of cryptomnesia : "In 1907, in the second edition of my *Psychopathologie des Alltagslebens*, I proposed an exactly similar explanation for this form of apparent paramnesia without mentioning Grasset's paper [of 1904] or knowing of its existence. By way of excuse I may remark that I arrived at my conclusion as the result of a psycho-analytic investigation which I was able to make of an example of *déjà vu* [...] [that] had occurred twenty-eight years earlier", p. 337.

prime and largely untapped problem-areas. In these sciences, investigators can move from one to another area with substantial gains in knowledge, in contrast to the older, better established sciences where continuous digging is more often the practice. To the extent that these patterned differences in choice of research problems occur, we should expect more cryptomnesia in relation to one's own work in the social sciences.

The frequency of such cryptomnesia should also be affected by the social organization of scientific work, which seems to affect every aspect of multiple discoveries in science. When research is organized in teams, it would be less likely, we must suppose, that earlier ideas and findings would be altogether forgotten. For if some members of the team forget them, others will not. Moreover, repeated interaction between collaborators will tend to fix these ideas and findings in memory.

The conspicuous changes in the social organization of scientific research should have a marked effect, not only on this matter of self-cryptomnesia, but on every aspect of multiples and priorities in science. The trend toward collaborative investigation in research organization is reflected in patterns of publication, with more and more research papers being by several authors rather than by only one. The extent of this change differs among the various disciplines. The sciences which have developed cogent theory, complex and often costly instrumentation and rigorous experiments or sets of observations seem to have experienced this change earlier and at a more rapid rate than the sciences which are less well developed in these respects. By way of illustration, consider the pattern of publication in just three cases : one drawn from the measurement of constants in physics; a second, from psychology; and the third, from sociology. I have tabulated the number of authors of each of 414 papers on the measurement of physical constants cited in an authoritative monograph on the subject (98). The results, in brief, are these : of the papers published before 1920, 93 % were by single authors; for those between 1920 and 1940, this declines to 65 %; and for those since 1940, to 26 %. Taking only the most recent period, we find that 28 % were by two authors; 19 % by three, 14 % by four and 13 % by five or more authors (with some 2 % of these being by ten or more co-authors).

(98) E. Richard COHEN, Kenneth M. CROWE and Jesse W. M. DUMOND, *The Fundamental Concepts of Physics* (New York, Interscience Publishers, Inc., 1957), pp. 92-102.

Much the same trend, but far less marked, is found for the papers published in the *Journal of Abnormal and Social Psychology* since 1936: arrayed by consecutive five-year periods, single-author papers decline from 80 % of all in 1936 to 75 % to 69 % to 54 % and finally to 49 % during the last five years. And the *American Sociological Review* for the same period witnesses a similar but even more restrained trend, with single-authored papers declining from 92 % in 1936, to 90 % to 87 % to 76 %, and in the last five years, to 65 % (99).

Although the facts are far from conclusive, this continuing change in the social structure of research, as registered by publications, seems to make for a greater concern among scientists with the question of "how will my contribution be identified" in collaborative work than with the historically dominant pattern of wanting to ensure their priority over others in the field. Not that the latter has been wholly displaced, as we have seen. But it may be that institutionally induced concern with priority is becoming overshadowed by the structurally induced concern with the allocation of credit among collaborators. One study of a team of thirty economists and behavioral scientists, for example, found that "the behavioral scientists were apt to be less concerned about 'piracy' and 'credit' than economists. This difference may be due to the greater emphasis on joint authorship in the behavioral sciences than in economics" (100).

For our purposes, the import of these changes in collaboration is, first, that the degree of concern with priority in science is probably not an historical constant; second, that it varies with the changing organization of scientific work; and third, that these changes may eventually and indirectly help make for the dispassionate and methodical study of multiples and priority in science, as resistance to that study is undercut by widespread recognition of the ubiquity of multiples in science.

Nevertheless, although scientists *know* that genuinely inde-

(99) The extent of these differences between patterns of collaboration in the major scientific and humanistic disciplines is now being investigated by Harriet Zuckerman at Columbia University. Extensive results are reported in her unpublished paper, "Collaboration in Science: A Study in Social and Cultural Change." Bernard Berelson has found that for the year, 1957-58, among a sample of those who had received their doctorate ten years before, the relative numbers of publications with single authors ranged from 17 % in chemistry, and 30 % in biology to 96 % in history and 97 % in English. See BERELSON, *Graduate Education in the United States* (New York, McGraw-Hill, 1960), p. 55.

(100) Warren G. BENNIS, "Some barriers to teamwork in social research", *Social Problems*, III (1956), 223-235, at 228-9.

pendent discoveries in science occur, many of them do not manage, as we have seen, to draw the implications of this for their own work. For reasons I have tried to intimate, they find it difficult, and sometimes impossible, to accept the fact that they have been anticipated, or that a contemporary has come to the same result just as the time they did, or that the others were truly independent of them. As we have also seen, the values in the social institution of science and the penumbra of uncertainty that surrounds the independence of thought combine to prevent the ready acceptance of events that undercut one's assurance of unique originality, an assurance born of the hard labor required to produce the new idea or new result. Consequently, multiple discoveries are experienced at best as an unpleasant reality and at worst as proof that deliberate or cryptomnesic borrowing has occurred. The reasonably detached study of multiples and priorities may possibly counter these tendencies to dismay or suspicion.

Such studies will probably not create the Olympian mood of a Goethe vigorously reaffirming Ecclesiastes : "No one can take from us the joy of the first becoming aware of something, the so-called discovery. But if we also demand the honor, it can be utterly spoiled for us for we are usually not the first. What does discovery mean, and who can say that he has discovered this or that ? After all, it's pure idiocy to brag about priority ; for it's simply unconscious conceit, not to admit frankly that one is a plagiarist" (101). But multiple discoveries can be recognized as having their uses, not only, as we noted before, for enlarging the likelihood that the discovery will be promptly caught up in the advancement of science but also for the individual discoverers. For, as we have seen Freud affirming in an effort to rouse himself from his ambivalence toward having been anticipated by Watkiss Lloyd, independent multiples do seem to lend confirmation to an idea or finding. Furthermore, even W. R. Hamilton, tormented his life long by the fear that he was being plagiarized or by the anxiety that he himself might be an 'innocent plagiarist', managed

(101) Quoted in the epigraph of his book by Lancelot Law Whyte, *The Unconscious Before Freud* (New York, Basic Books, 1960). We need not mark the irony that the maxim, there is nothing new under the sun, has itself variously recurred : remember only Terence, beset by charges of wholesale theft, saying : "nihil est dictum quod non sit dictum prius." Or five centuries later, Donatus exclaiming : "Pereant qui ante nos nostra dixerunt." Or Shakespeare, in Sonnet LIX :

If there be nothing new, but that which is
Hath been before, how are our brains [beguil'd,
Which, labouring for invention, bear amiss
The second burthen of a former child !

on at least one occasion to note, as did Freud, the secondary benefits of a multiple, when, in an effort to dissolve his ambivalence, he wrote Herschel :

I persuade myself that, if those results had been anticipated, the learning it would have given me no pain; for it was, so far as I could analyze my sensations, without any feeling of vexation that I learned that the result respecting the relation of the lines of curvature to the circular sections was known before. The field of pure, not to say of mixed, mathematics is far too large and rich to leave one excusable for sitting down to complain, when he finds that this or that spot which he was beginning to cultivate as his own has been already appropriated. [And now comes his hard-won and, sad to tell, temporary, insight :] There is even a stronger feeling inspired of the presence of that Truth to which we all profess to minister, when we find our own discoveries, such as they are, coincide independently with the discoveries of other men. The voice which is heard by two at once appears to be more real and external—one is more *sure* that it is no personal and private fancy, no idiosyncratic peculiarity, no ringing in sick ears, no flashes seen by rubbing our own eyes (102).

And then, unable to contain himself, Hamilton goes on to announce in the same letter that he had anticipated the work on ellipsoids by Joachimstal in "a long extinct periodical of whose *existence* he [Joachimstal] probably never heard, with a date which happened to be a *precise decennium* earlier [...]" (103).

If the fluctuating ailment of that genius Hamilton proves that the knowledge of multiples is no panacea for ambivalence toward priority, his moment of insight suggests that it may be some small help. The mathematician, R. L. Wilder, is, to my knowledge, the only one who has seen this clearly and has, to my mingled pleasure and discomfiture, anticipated me in suggesting that the study of multiples may have a therapeutic function for the community of scientists. Since he has anticipated my observation, let me then borrow his words :

I wish to inquire, above the individual level, into the manner in which mathematical concepts originate, and to study those factors that encourage their formation and influence their growth. I think that much benefit might be derived from such an inquiry. For example, if the individual working mathematician understands that when a concept is about to make its appearance, it is most likely to do so through the medium of more than one creative mathematician; and if, furthermore, he knows the reasons for this phenomenon, then we can expect less indulgence in bad feelings and suspicion of plagiarism to ensue than we find in notable past intances. Mathematical history contains numerous cases of arguments over priority, with nothing settled after the smoke of battle has cleared away except that when you come right down to it practically the same thing was thought of by someone else several years previously, only he didn't quite realize the full significance of what he had, or did not have the good luck

(102) GRAVES, *Hamilton*, *op. cit.* II, p. 533, in a letter to Herschel, Nov. 23, 1846.

(103) *Ibid.* II, p. 534.

to possess the tools wherewith to exploit it [...] [Yet] it is exactly what one should expect if he is acquainted with the manner in which concepts evolve (104).

All this does not deny, of course, the possibility that in particular cases, the unwitting or deliberate use of ideas and findings without acknowledgement may occur. I have tried elsewhere (105) to show how the institution of science, with its premium upon originality, indirectly motivates just that kind of deviant behavior among some scientists. But for our understanding of how scientific knowledge develops, we have long since needed, among other things, to overcome the resistance toward the dispassionate and methodical study of multiples and attendant priority-conflicts, rather than to neglect this study altogether or to come to it only when we plunge, as emotionally involved participants, into conflicts over rights to intellectual property. After all, one of the roles assigned the sociologist is to investigate the behavior of all manner of men, including men of science, without giving way to the entirely human tendency to substitute for that investigation a clucking of tongues and a condemning of that which is and ought not to be *.

(104) R. L. WILDER, "The origin and growth of mathematical concepts", *Bulletin of the American Mathematical Society*, LXIX (1953), 423-448, at 425.

(105) MERTON, "Priorities in scientific discovery", *op. cit.*

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