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PRIORITIES IN SCIENTIFIC DISCOVERY: A CHAPTER IN THE SOCIOLOGY OF SCIENCE *

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We can only guess what historians of the future will say about the condition of present-day sociology. But it seems safe to anticipate one of their observations. When the Trevelyans of 2050 come to write that history—as they well might, for this clan of historians promises to go on forever—they will doubtless find it strange that so few sociologists (and historians) of the twentieth century could bring themselves, in their work, to treat science as one of the great social institutions of the time. They will observe that long after the sociology of science became an identifiable field of inquiry,[1] it remained little cultivated in a world where science loomed large enough to present mankind with the choice of destruction or survival. They may even suggest that somewhere in the process by which social scientists take note of the world as it is and as it once was, a sense of values appears to have become badly scrambled.

This spacious area of neglect may therefore have room for a paper which tries to examine science as a social institution, not in the large but in terms of a few of its principal components.

A Calendar of Disputes over Priority. We begin by noting the great frequency with which the history of science is punctuated by disputes, often by sordid disputes, over priority of discovery. During the last three centuries in which modern science developed, numerous scientists, both great and small, have engaged in such acrimonious controversy. Recall only these few: Keenly aware of the importance of his inventions and discoveries, Galileo became a seasoned campaigner as he vigorously defended his rights to priority first, in his *Defense against the Calumnies and Impostures of Baldassar Capar,* where he showed how his invention of the "geometric and military compass" had been taken from him, and then, in *The Assayer,* where he flayed four other would be rivals; Father Horatio Grassi, who tried "to diminish whatever praise there may be in this [invention of the telescope for use in astronomy] which belongs to me"; Christopher Scheiner, who claimed to have been first to observe the sunspots (although, unknown to both Scheiner and Galileo, Johann Fabricius had published such observations before); an unspecified villain (probably the Frenchman Jean Tarde) 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 discoverers of these marvels"; and finally, Simon Mayr, who "had the gall to claim that he had observed the Medicean planets which revolve about Jupiter before I had [and

* Presidential address read at the annual meeting of the American Sociological Society, August, 1957.

[1] The rudiments of a sociology of science can be found in an overview of the subject by Bernard Barber, *Science and the Social Order,* Glencoe: The Free Press, 1952; Bernard Barber, "Sociology of Science: A Trend Report and Bibliography," *Current Sociology,* Vol. 5, No. 2, Paris: UNESCO, 1957.

used] a sly way of attempting to establish his priority." [2]

The peerless Newton fought several battles with Robert Hooke over priority in optics and celestial mechanics and entered into a long and painful controversy with Leibniz over the invention of the calculus. Hooke,[3] who has been described as the "universal claimant" because "there was scarcely a discovery in his time which he did not conceive himself to claim," (and, it might be added, often justly so, for he was one of the most inventive men in his century of genius), Hooke, in turn, contested priority not only with Newton but with Huygens over the important invention of the spiral-spring balance for regulating watches to eliminate the effect of gravity.

The calendar of disputes was full also in the eighteenth century. Perhaps the most tedious and sectarian of these was the great "Water Controversy" in which that shy, rich, and noble genius of science, Henry Cavendish, was pushed into a three-way tug-of-war with Watt and Lavoisier over the question of which one had first demonstrated the compound nature of water and thereby removed it from its millennia-long position as one of the elements. Earthy battles raged also over claims to the first discovery of heavenly bodies, as in the case of the most dramatic astronomical discovery of the century in which the Englishman John Couch Adams and the Frenchman Urban Jean LeVerrier inferred the existence and predicted the position of the planet now known as Neptune, which was found where their independent computations showed it would be. Medicine had its share of conflicts over priority; for example, Jenner believed himself first to demonstrate that vaccination afforded security against smallpox, but the advocates of Pearson and Rabaut believed otherwise.

Throughout the nineteenth century and down to the present, disputes over priority continued to be frequent and intense. Lister knew he had first introduced antisepsis, but others insisted that Lemaire had done so before. The sensitive and modest Faraday was wounded by the claims of others to several of his major discoveries in physics: one among these, the discovery of electro-magnetic rotation, was said to have been made before by Wollaston; Faraday's onetime mentor, Sir Humphrey Davy (who had himself been involved in similar disputes) actually opposed Faraday's election to the Royal Society on the ground that his was not the original discovery.[4] Laplace, several of the Bernoullis, Legendre, Gauss, Cauchy were only a few of the giants among mathematicians embroiled in quarrels over priority.

What is true of physics, chemistry, astronomy, medicine and mathematics is true also of all the other scientific disciplines, not excluding the social and psychological sciences. As we know, sociology was officially born only after a long period of abnormally severe labor. Nor was the postpartum any more tranquil. It was disturbed by violent controversies between the followers of St.-Simon and Comte as they quarreled over the delicate question of which of the two was the father of sociology and which merely the obstetrician. And to come to the very recent past, Janet is but one among several to have claimed that they had the essentials of psycho-analysis before Freud.

To extend the list of priority fights would be industrious and, for this occasion, superfluous. For the moment, it is enough to note that these controversies, far from being a rare exception in science, have long been frequent, harsh, and ugly. They have practically become an integral part of the social

[2] Galileo, *The Assayer,* 1623, translated by Stillman Drake in *Discoveries and Opinions of Galileo,* New York: Doubleday, 1957, pp. 232–233, 245. Galileo thought it crafty of Mayr to date his book as published in 1609 by using the Julian calendar without indicating that, as a Protestant, he had not accepted the Gregorian calendar adopted by "us Catholics" which would have shifted the date of publication to January 1610, when Galileo had reported having made his first observations. Later in this paper, I shall have more to say about the implications of attaching importance to such short intervals separating rival claims to priority.

[3] For scholarly reappraisals of Hooke's role in developing the theory of gravitation, see Louis Diehl Patterson, "Hooke's Gravitation Theory and Its Influence on Newton," *Isis,* 40 (November, 1949), pp. 327–341; 41 (March, 1950), pp. 32–45; and E. N. da C. Andrade, "Robert Hooke," Wilkins Lecture, *Proceedings of the Royal Society,* Series B, Biological Sciences, 137 (24 July, 1950). The recent biography by Margaret 'Espinasse is too uncritical and defensive of Hooke to be satisfactory; *Robert Hooke,* London: Heinemann, 1956.

[4] Bence Jones, *The Life and Letters of Faraday,* London: Longmans, Green, 1870, Vol. I, pp. 336–352.

relations between scientists. Indeed, the pattern is so common that the Germans have characteristically compounded a word for it, *Prioritätsstreit.*

On the face of it, the pattern of conflict over priority can be easily explained. It seems to be merely a consequence of the same discoveries being made simultaneously, or nearly so, a recurrent event in the history of science which has not exactly escaped the notice of sociologists, or of others, at least since the definitive work of William Ogburn and Dorothy Thomas. But on second glance, the matter does not appear quite so simple.

The bunching of similar or identical discoveries in science is only an *occasion* [5] for disputes over priority, not their *cause* or their *grounds*. After all, scientists also know that discoveries are often made independently. (As we shall see, they not only know this but fear it, and this often activates a rush to ensure their priority.) It would therefore seem a simple matter for scientists to acknowledge that their simultaneous discoveries were independent and that the question of priority is consequently beside the point. On occasion, this is just what has happened, as we shall see in that most moving of all cases of *noblesse oblige* in the history of science, when Darwin and Wallace tried to outdo one another in giving credit to the other for what each had separately worked out. Fifty years after the event, Wallace was still insisting upon the contrast between his own hurried work, written within a week after the great idea came to him, and Darwin's work, based on twenty years of collecting evidence. "*I* was then (as often since) the 'young man in a hurry,' " said the reminiscing Wallace; "*he*, the painstaking and patient student seeking ever the full demonstration of the truth he had discovered, rather than to achieve immediate personal fame." [6]

On other occasions, self-denial has gone even further. For example, the incomparable Euler withheld his long "sought solution to the calculus of variations, until the twenty-three-year-old Lagrange, who had developed a new method needed to reach the solution, could put it into print, " 'so as not to deprive you,' Euler informed the young man, 'of any part of the glory which is your due.' " [7] Apart from these and many other examples of generosity in the annals of science, there have doubtless been many more that never found their way into the pages of history. Nevertheless, the recurrent struggles for priority, with all their intensity of affect, far overshadow these cases of *noblesse oblige,* and it still remains necessary to account for them.

Alleged Sources of Conflicts over Priority. One explanation of these disputes would regard them as mere expressions of human nature. On this view, egotism is natural to the species; scientists, being human, will have their due share and will sometimes express their egotism through self-aggrandizing claims to priority. But, of course, this interpretation does not stand up. The history of social thought is strewn with the corpses of those who have tried, in their theory, to make the hazardous leap from human nature to particular forms of social conduct, as has been observed from the time of Montesquieu, through Comte and Durkheim, to the present.[8]

A second explanation derives these conflicts not from the original nature shared by all men, but from propensities toward egotism found among some men. It assumes that, like other occupations, the occupation of science attracts some ego-centered people, and assumes further that it might even attract many such people, who, hungry for fame, elect to enter a profession that promises enduring fame to the successful. Unlike

[5] And not always even the occasion. Disputes over priority have occurred when alleged or actual anticipations of an idea have been placed decades or, at times, even centuries or millennia earlier, when they are generally described as "rediscoveries."

[6] This remark is taken from Wallace's commentary at the semi-centenary of the joint discovery, a classic of self-abnegation that deserves to be rescued from the near-oblivion into which it has fallen. For a transcript, see James Marchant, *Alfred Russel Wallace: Letters and Reminiscences,* New York: Harper, 1916, pp. 91–96.

[7] E. T. Bell, *Men of Mathematics,* New York: Simon and Schuster, 1937, pp. 155–156. And see the comparable act of generosity on the part of the venerable Legendre toward the mathematical genius, Niels Abel, then in his twenties, *ibid.,* p. 337.

[8] Émile Durkheim had traced this basic theme in sociological theory as early as his Latin thesis of 1892, which has fortunately been translated into French for the benefit of some of us later sociologists. See his *Montesquieu et Rousseau: Précurseurs de la Sociologie,* Paris: Marcel Rivière, 1953, esp. Chapter I.

the argument from nature, this one, dealing with processes of self-selection and of social selection, is not defective in principle. It is possible that differing kinds of personalities tend to be recruited by various occupations and, though I happen to doubt it, it is possible that quarrelsome or contentious personalities are especially apt to be attracted to science and recruited into it. The extent to which this is so is a still unanswered question, but developing inquiry into the type of personality characteristic of those entering the various professions may in due course discover how far it is so.[9] In any event, it should not be difficult to find *some* aggressive men of science.

But even should the processes of selection result in the recruitment of contentious men, there are theoretical reasons for believing that this does not adequately account for the great amount of contention over priority that flares up in science. For one thing, these controversies often involve men of ordinarily modest disposition who act in seemingly self-assertive ways only when they come to defend their rights to intellectual property. This has often been remarked, and sometimes with great puzzlement. As Sir Humphrey Davy asked at the time of the great Water Controversy between Cavendish and Watt, how does it happen that this conflict over priority should engage such a man as Cavendish, "unambitious, unassuming, with difficulty . . . persuaded to bring forward his important discoveries . . . and . . . fearful of the voice of fame."[10] And the biographer of Cavendish, writing about the same episode, describes it as "a perplexing dilemma. Two unusually modest and unambitious men, universally respected for their integrity, famous for their discoveries and inventions, are suddenly found standing in a hostile position towards each other. . . ."[11] Evidently, ingrained egotism is not required to engage in a fight for priority.

A second strategic fact shows the inadequacy of explaining these many struggles as owing to egotistic personalities. Very often, the principals themselves, the discoverers or inventors, take no part in arguing their claims to priority (or withdraw from the controversy as they find that it places them in the distasteful role of insisting upon their own merits or of deprecating the merits of their rivals). Instead, it is their friends and followers, or other more detached scientists, who commonly see the assignment of priority as a moral issue that must be fought to a conclusion. For example, it was Wollaston's friends, rather than the distinguished scientist himself, who insinuated that the young Faraday had usurped credit for the experiments on electro-magnetic rotation.[12] Similarly, it was Priestley, De Luc and Blagden, "all men eminent in science and of unblemished character," who embroiled the shy Cavendish and the unassertive Watt in the Water Controversy.[13] Finally, it was the

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[9] Information about this is sparse and unsatisfactory. As a bare beginning, a study of the Thematic Apperception Test protocols of 64 eminent biological, physical, and social scientists found no signs of their being "particularly aggressive." Anne Roe, *The Making of a Scientist,* New York: Dodd, Mead, 1953, p. 192.

[10] Sir Humphrey Davy, *Collected Works,* VII, p. 128, quoted by George Wilson, *The Life of the Honorable Henry Cavendish,* London, 1851, p. 63.

[11] Wilson, *op. cit.,* p. 64. There can be little doubt about the unassuming character of Cavendish, the pathologically shy recluse, whose unpublished notebooks were crowded with discoveries disproving then widely-held theories and anticipating discoveries not made again for a long time to come. He stands as the example *a fortiori,* for even such a man as this was drawn into a controversy over priority.

The history of science evidently has its own brand of chain-reactions. It was the reading of Wilson's *Life of Cavendish* with its report of Cavendish's long-forgotten experiment on the sparking of air over alkalis which led Ramsay (just as the same experiment led Rayleigh) to the discovery of the element argon. Both Rayleigh and Ramsay delicately set out their respective claims to the discovery, claims not easily disentangled since the two had been in such close touch. They finally agreed to joint publication as "the only solution" to the problem of assigning appropriate credit. The episode gave rise to a great controversy over priority in which neither of the discoverers would take part; the debate is continued in the biographies of the two: by the old friend and collaborator of Ramsay, Morris W. Travers, in *A Life of Sir William Ramsay,* London: Edward Arnold Ltd., 1956, pp. 100, 121–122, 292, *passim;* and by the son of Lord Rayleigh, *John William Strutt: Third Baron Rayleigh,* London: Edward Arnold, 1924, Chapter XI.

[12] Jones, *op. cit.,* pp. 351–352; see also the informative book by T. W. Chalmers, *Historic Researches: Chapters in the History of Physical and Chemical Discovery,* New York: Scribner's, 1952, p. 54.

[13] This is the contemporary judgment by Wilson, *op. cit.,* pp. 63–64.

quarrelsome, eminent, and justly esteemed scientist François Arago (whom we shall meet again) and a crowd of astronomers, principally in France and England but also in Germany and Russia, rather than "the shy, gentle and unaffected" co-discoverer of Neptune, Adams, who stirred the pot of conflict over priority until it boiled over and then simmered down into general acknowledgment that the planet had been independently discovered by Adams and LeVerrier.[14] And so, in one after another of the historic quarrels over priority in science.

Now these argumentative associates and bystanders stand to gain little or nothing from successfully prosecuting the claims of their candidate, except in the pickwickian sense of having identified themselves with him or with the nation of which they are all a part. Their behavior can scarcely be explained by egotism. They do not suffer from rival claims to precedence. Their personal status is not being threatened. And yet, over and again, they take up the cudgels in the status-battle [15] and, uninhibited by any semblance of indulging in self-praise, express their great moral indignation over the outrage being perpetrated upon their candidate.

This is, I believe, a particularly significant fact. For, as we know from the sociological theory of institutions, the expression of disinterested moral indignation is a signpost announcing the violation of a social norm.[16] Although the indignant bystanders are themselves not injured by what they take to be the misbehavior of the culprit, they respond with hostility and want to see "fair play," to see that behavior conforms to the rules of the game. The very fact of their entering the fray goes to show that science is a social institution with a distinctive body of norms exerting moral authority and that these norms are invoked particularly when it is felt that they are being violated. In this sense, fights over priority, with all their typical vehemence and passionate feelings, are not merely expressions of hot tempers, although these may of course raise the temperature of controversy; basically, they constitute responses to what are taken to be violations of the institutional norms of intellectual property.

INSTITUTIONAL NORMS OF SCIENCE

To say that these frequent conflicts over priority are rooted in the egotism of human nature, then, explains next to nothing; to say that they are rooted in the contentious personalities of those recruited by science may explain part, but not enough; to say, however, that these conflicts are largely a consequence of the institutional norms of science itself comes closer, I think, to the truth. For, as I shall suggest, it is these norms that exert pressure upon scientists to assert their claims, and this goes far toward explaining the seeming paradox that even those meek and unaggressive men, ordinarily slow to press their own claims in other spheres of life, will often do so in their scientific work.

The ways in which the norms of science help produce this result seem clear enough. On every side, the scientist is reminded that it is his role to advance knowledge and his happiest fulfillment of that role, to advance knowledge greatly. This is only to say, of course, that in the institution of science originality is at a premium. For it is through originality, in greater or smaller increments, that knowledge advances. When the institution of science works efficiently, and like other social institutions, it does not always do so, recognition and esteem accrue to those who have best fulfilled their roles, to those who have made genuinely original contributions to the common stock of knowledge. Then are found those happy circumstances in which self-interest and moral obligation coincide and fuse.

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14 Sir Harold Spencer Jones, "John Couch Adams and the Discovery of Neptune," reprinted in James R. Newman, *The World of Mathematics,* New York: Simon and Schuster, 1956, II, pp. 822–839. A list of cases in which associates, rather than principals, took the lead in these conflicts is a very long one. I do not include it here.

15 Sometimes, of course, they act as judges and arbitrators rather than advocates, as was true of Lyell and Hooker in the episode involving Darwin and Wallace. But, as we shall see, the same institutional norms are variously called into play in all these cases.

16 For an acute analysis of the theoretical place of moral obligation, and its correlate, moral indignation, in the theory of institutions, particularly as this was developed in the long course of Durkheim's work, see Talcott Parsons, *The Structure of Social Action,* Glencoe: The Free Press, 1949, pp. 368–470; for further formulations and citations of additional literature, see R. K. Merton, *Social Theory and Social Structure,* Glencoe: The Free Press, 1957 (rev. ed.), pp. 361 ff.

Recognition of what one has accomplished is thus largely a motive derived from institutional emphases. Recognition for originality becomes socially validated testimony that one has successfully lived up to the most exacting requirements of one's role as scientist. The self-image of the individual scientist will also depend greatly on the appraisals by his scientific peers of the extent to which he has lived up to this exacting and critically important aspect of his role. As Darwin once phrased it, "My love of natural science . . . has been much aided by the ambition to be esteemed by my fellow naturalists."

Interest in recognition,[17] therefore, need not be, though it can readily become, simply a desire for self-aggrandizement or an expression of egotism. It is, rather, the motivational counterpart on the psychological plane to the emphasis upon originality on the institutional plane. It is not necessary that individual scientists begin with a lust for fame; it is enough that science, with its abiding and often functional emphasis on originality and its assigning of large rewards for originality, makes recognition of priority uppermost. Recognition and fame then become symbol and reward for having done one's job well.

This means that long before we know anything about the distinctive personality of this or that scientist, we know that he will be under pressure to make his contributions to knowledge known to other scientists and that they, in turn, will be under pressure to acknowledge his rights to his intellectual property. To be sure, some scientists are more vulnerable to these pressures than others—some are self-effacing, others self-assertive; some generous in granting recognition, others stingy. But the great frequency of struggles over priority does not result merely from these traits of individual scientists but from the institution of science, which defines originality as a supreme value and thereby makes recognition of one's originality a major concern.[18]

When this recognition of priority is either not granted or fades from view, the scientist loses his scientific property. Although this kind of property shares with other types general recognition of the "owner's" rights, it contrasts sharply in all other respects. Once he has made his contribution, the scientist no longer has exclusive rights of access to it. It becomes part of the public domain of science. Nor has he the right of regulating its use by others by withholding it unless it is acknowledged as his. In short, property rights[19] in science become whittled down to just this one: the recognition by others of the scientist's distinctive part in having brought the result into being.

It may be that this concentration of the numerous rights ordinarily bound up in other forms of property into the one right of recognition by others helps produce the great concentration of affect that commonly characterizes disputes over priority. Often, the intensity of affect seems disproportionate to the occasion; for example, when a scientist feels he has not been given enough recogni-

[17] It is not only the institution of science, of course, that instills and reinforces the concern with recognition; in some degree, all institutions do. This is evident since the time W. I. Thomas included 'recognition' as one of what he called "the four wishes" of men. The point is, rather, that with its emphasis on originality, the institution of science greatly reinforces this concern and indirectly leads scientists to vigorous self-assertion of their priority. For Thomas's fullest account of the four wishes, see *The Unadjusted Girl,* Boston: Little, Brown, 1925, Chapter I.

[18] In developing this view, I do not mean to imply that scientists, any more than other men, are merely obedient puppets doing exactly what social institutions require of them. But I do mean to say that, like men in other institutional spheres, scientists tend to develop the values and to channel their motivations in directions the institution defines for them. For an extended formulation of the general theory of institutionalized motivation, see Talcott Parsons, *Essays in Sociological Theory,* Glencoe: The Free Press, 1954 (rev. ed.), esp. Chapters II and III.

[19] That the notion of property is part and parcel of the institution of science can be seen from the language employed by scientists in speaking of their work. Ramsay, for example, asks Rayleigh's "permission to look into atmospheric nitrogen" on which Rayleigh had been working; the young Clerk Maxwell writes William Thomson, "I do not know the Game laws and Patent laws of science . . . but I certainly intend to poach among your electrical images"; Norbert Wiener describes "differential space, the space of the Brownian motion" as "wholly mine in its purely mathematical aspects, whereas I was only a junior partner in the theory of Banach spaces." Borrowing, trespassing, poaching, credit, stealing, a concept which "belongs" to us—these are only a few of the many terms in the lexicon of property adopted by scientists as a matter of course.

tion for what is, in truth, a minor contribution to knowledge, he may respond with as much indignation as the truly inventive scientist, or even with more, if he secretly senses that this is the outermost limit of what he can reasonably hope to contribute.[20] This same concentration of property-rights into the one right of recognition may also account for the deep moral indignation expressed by scientists when one of their number has had his rights to priority denied or challenged. Even though they have no personal stake in the particular episode, they feel strongly about the single property-norm and the expression of their hostility serves the latent function of reaffirming the moral validity of this norm.

National Claims to Priority. In a world made up of national states, each with its own share of ethnocentrism, the new discovery redounds to the credit of the discoverer not as an individual only, but also as a national. From at least the seventeenth century, Britons, Frenchmen, Germans, Dutchmen, and Italians have urged their country's claims to priority; a little later, Americans entered the lists to make it clear that they had primacy.

The seventeenth-century English scientist Wallis, for example, writes: "I would very fain that Mr. Hooke and Mr. Newton would set themselves in earnest for promoting the designs about telescopes, that others may not steal from us what our nation invents, only for the neglect to publish them ourselves." So, also, Halley says of his comet that "if it should return according to our prediction about the year 1758 [as of course it did], impartial posterity will not refuse to acknowledge that this was first discovered by an Englishman." [21]

Or to move abruptly to the present, we see the Russians, now that they have taken a powerful place on the world-scene, beginning to insist on the national character of science and on the importance of finding out who first made a discovery. Although the pattern of national claims to priority is old, the formulation of its rationale in a Russian journal deserves quotation if only because it is so vigorously outspoken:

> Marxism-Leninism shatters into bits the cosmopolitan fiction concerning supra-class, non-national, "universal" science, and definitely proves that science, like all culture in modern society, is national in form and class in content. . . . The slightest inattention to questions of priority in science, the slightest neglect of them, must therefore be condemned, for it plays into the hands of our enemies, who cover their ideological aggression with cosmo-

[20] Some of this had occurred to Galileo in his counterattack on Sarsi (pseudonym for Grassi): "Only too clearly does Sarsi show his desire to strip me completely of any praise. Not content with having disproved our reasoning set forth to explain the fact that the tails of comets sometimes appear to be bent in an arc, he adds that nothing new was achieved by me in this, as it had all been published long ago, and then refuted, by Johann Kepler. In the mind of the reader who goes no more deeply than Sarsi's account, the idea will remain that I am not only a thief of other men's ideas, but a petty, mean thief at that, who goes about pilfering even what has been refuted. And who knows; perhaps in Sarsi's eyes the pettiness of the theft does not render me more blameworthy than I would be if I had bravely applied myself to greater thefts. If, instead of filching some trifle, I had more nobly set myself to search out books by some reputable author not as well known in these parts, and had then tried to suppress his name and attribute all his labors to myself, perhaps Sarsi would consider such an enterprise as grand and heroic as the other seems to him cowardly and abject." (Galileo, *The Assayer, op. cit.*, pp. 261–262.)

This type of reaction to what I describe as the "professional adumbrationist" (in the unpublished part of this paper) was expressed also by Benjamin Franklin after he had suffered from claims by others that they had first worked out the experiment of the lightning kite. As he said in part (the rest of his observations are almost equally in point), "The smaller your invention is, the more mortification you receive in having the credit of it disputed with you by a rival, whom the jealousy and envy of others are ready to support against you, at least so far as to make the point doubtful. It is not in itself of importance enough for a dispute; no one would think your proofs and reasons worth their attention: and yet if you do not dispute the point, and demonstrate your right, you not only lose the credit of being in that instance *ingenious,* but you suffer the disgrace of not being *ingenuous;* not only of being a plagiary but of being a plagiary for trifles. Had the invention been greater it would have disgraced you less; for men have not so contemptible an idea of him that robs for gold on the highway, as of him that can pick pockets for half-pence and farthings." (Quoted in the informed and far-reaching monograph by I. B. Cohen, *Franklin and Newton,* Philadelphia: The American Philosophical Society, 1956, pp. 75–76.)

[21] Louis T. More, *Isaac Newton,* New York: Scribner's, 1934, pp. 146–147, and pp. 241; 477–478.

politan talk about the supposed non-existence of questions of priority in science, *i.e.*, the questions concerning which peoples [here, be it noted, collectivities displace the individual scientist] made what contribution to the general store of world culture . . . [And summarizing the answers to these questions in compact summary] The Russian people has the richest history. In the course of this history, it has created the richest culture, and all the other countries of the world have drawn upon it and continue to draw upon it to this day.[22]

Against this background of affirmation, one can better appreciate the recent statement by Khrushchev that "we Russians had the H-bomb before you" and the comment by the New York Times that "the question of priority in the explosion of the hydrogen bomb is . . . a matter of semantics," to be settled only when we know whether the "prototype-bomb" or the full-fledged bomb" is in question.[23]

The recent propensity of the Russians to claim priority in all manner of inventions and scientific discoveries thus energetically reduplicates the earlier, and now less forceful though far from vanished, propensity of other nations to claim like priorities. The restraint often shown by individual scientists in making such claims becomes rather inconspicuous when official or self-constituted representatives of nations put in their claims.

THE REWARD-SYSTEM IN SCIENCE

Like other institutions, the institution of science has developed an elaborate system for allocating rewards to those who variously live up to its norms. Of course, this was not always so. The evolution of this system has been the work of centuries, and it is probably far from finished. In the early days of modern science, Francis Bacon could explain and complain all in one by saying that "it is enough to check the growth of science, that efforts and labours in this field go unrewarded. . . . And it is nothing strange if a thing not held in honour does not prosper."[24] And a half-century later, much the same could be said by Thomas Sprat, the Bishop of Rochester, in his official history of the newly-established Royal Society:

> . . . it is not to be wonder'd, if men have not been very zealous about those studies, which have been so farr remov'd, from present benefit, and from the applause of men. For what should incite them, to bestow their time, and Art, in revealing to mankind, those Mysteries for which, it may be, they would be onely despis'd at last? How few must there needs be, who will be willing, to be impoverish'd for the common good? while they shall see, all the rewards, which might give life to their Industry, passing by them, and bestow'd on the deserts of easier studies?[25]

The echo of these complaints still reverberates in the halls of universities and scientific societies, but chiefly with regard to material rather than honorific rewards. With the growth and professionalization of science, the system of honorific rewards has become diversely elaborated, and apparently at an accelerated rate.

Heading the list of the immensely varied forms of recognition long in use is eponymy,[26]

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[22] An editorial, "Against the Bourgeois Ideology of Cosmopolitanism," *Voprosy filosofi*, 1948, No. 2, as translated in the *Current Digest of the Soviet Press*, February 1, 1949, Vol. 1, No. 1, pp. 9–10, 12. For an informed account, see David Joravsky, "Soviet Views on the History of Science," *Isis*, 46 (March, 1955), pp. 3–13, esp. at pp. 9n. and 11, which treat of changing Russian attitudes toward priority and simultaneous invention; see also Merton, *Social Theory and Social Structure*, *op. cit.*, pp. 556–560.

[23] *New York Times*, July 27, 1957, p. 3, col. 1.

[24] Francis Bacon, *Novum Organum*, trans. by Ellis and Spedding, London: Routledge, n. d. Book I, Aphorism XCI. The ellipsis in the text above was for brevity's sake; it should be filled out here below because of the pertinence of what Bacon went on to say: "For it does not rest with the same persons to cultivate sciences and to reward them. The growth of them comes from great wits, the prizes and rewards of them are in the hands of the people, or of great persons, who are but in very few cases even moderately learned. Moreover this kind of progress is not only unrewarded with prizes and substantial benefits; it has not even the advantage of popular applause. For it is a greater matter than the generality of men can take in, and is apt to be overwhelmed and extinguished by the gales of popular opinions."

[25] Thomas Sprat, *The History of the Royal Society*, London, 1667, p. 27.

[26] Galileo begins his "Message from the Stars," announcing his discovery of the satellites of Jupiter, with a paean to the practice of eponymy which opens with these words: "Surely a distinguished public service has been rendered by those who have protected from envy the noble achievements of men who have excelled in virtue, and have thus

the practice of affixing the name of the scientist to all or part of what he has found, as with the Copernican system, Hooke's law, Planck's constant, or Halley's comet. In this way, scientists leave their signatures indelibly in history; their names enter into all the scientific languages of the world.

At the rugged and thinly populated peak of this system of eponymy are the men who have put their stamp upon the science and thought of their age. Such men are naturally in very short supply, and these few sometimes have an entire epoch named after them, as when we speak of the Newtonian epoch, the Darwinian era, or the Freudian age.

The gradations of eponymy have the character of a Guttman scale in which those men assigned highest rank are also assigned lesser degrees of honorific recognition. Accordingly, these peerless scientists are typically included also in the next highest ranks of eponymy, in which they are credited with having fathered a new science or a new branch of science (at times, according to the heroic theory, through a kind of parthenogenesis for which they apparently needed no collaborators). Of the illustrious Fathers of this or that science (or of this or that specialty), there is an end, but an end not easily reached. Consider only these few, culled from a list many times this length:

Morgagni, the Father of Pathology
Cuvier, the Father of Palaeontology
Faraday, the Father of Electrotechnics
Daniel Bernoulli, the Father of Mathematical Physics
Bichat, the Father of Histology
van Leeuwenhoek, the Father of Protozoology and Bacteriology
Jenner, the Father of Preventive Medicine
Chladni, the Father of Modern Acoustics
Herbart, the Father of Scientific Pedagogy
Wundt, the Father of Experimental Psychology
Pearson, the Father of Biometry;
and, of course,
Comte, the Father of Sociology.

In a science as farflung and differentiated as chemistry, there is room for several paternities. If Robert Boyle is the undisputed Father of Chemistry (and, as his Irish epitaph has it, also the Uncle of the Earl of Cork), then Priestley is the Father of Pneumatic Chemistry, Lavoisier the Father of Modern Chemistry, and the nonpareil Willard Gibbs, the Father of Physical Chemistry.

On occasion, the presumed father of a science is called upon, in the persons of his immediate disciples or later adherents, to prove his paternity, as with Johannes Müller and Albrecht von Haller, who are severally regarded as the Father of Experimental Physiology.

Once established, this eponymous pattern is stepped up to extremes. Each new specialty has its own parent, whose identity is often known only to those at work within the specialty. Thus, Manuel Garcia emerges as the Father of Laryngoscopy, Adolphe Brongiart as the Father of Modern Palaeobotany, Timothy Bright as the Father of Modern Shorthand, and Father Johann Dzierson (whose important work may have influenced Mendel) as the Father of Modern Rational Beekeeping.

Sometimes, a particular form of a discipline bears eponymous witness to the man who first gave it shape, as with Hippocratic medicine, Aristotelian logic, Euclidean geometry, Boolean algebra, and Keynesian economics. Most rarely, the same individual acquires a double immortality, both for what he achieved and for what he failed to achieve, as in the cases of Euclidean and non-Euclidean geometries, and Aristotelian and non-Aristotelian logics.

In rough hierarchic order, the next echelon is comprised by thousands of eponymous laws, theories, theorems, hypotheses, instruments, constants and distributions. No short list can hope to be representative of the wide range of these scientific contributions that have immortalized the men who made them. But a few examples in haphazard array might include the Brownian movement, the Zeeman effect, Rydberg's constant, Moseley's atomic number, and the Lorenz curve or to come closer home, where we refer only to assured contemporary recognition rather than to possibly permanent fame, the Spearman rank-correlation coefficient, the Rorschach ink-blot, the Thurstone scale, the Bogardus social-distance scale, the Bales categories of interaction, the Guttman scalogram and the Lazarsfeld latent-structure analysis.

preserved from oblivion and neglect those names which deserve immortality." (*Op. cit.*, p. 23.) He then proceeds to call the satellites "the Medicean Stars" in honor of the Grand Duke of Tuscany, who soon becomes his patron.

Each science, or art based on science, evolves its own distinctive patterns of eponymy to honor those who have made it what it is. In the medical sciences, for example, the attention of posterity is assured to the discoverer or first describer of parts of the body (as with the Eustachian tube, the circle of Willis, Graffian follicles, Wharton's duct, and the canal of Nuck) though, oddly enough, Vesalius, commonly described as the Father of Modern Anatomy has been accorded no one part of the body as distinctly his own. In medicine, also, eponymy registers the first diagnostician of a disease (as with Addison's, Bright's, Hodgkin's, Menière's and Parkinson's diseases); the inventor of diagnostic tests (as with Romberg's sign, the Wassermann reaction, the Calmette test, and the Babinski reflex); and the inventor of instruments used in research or practice (as with the Kelly pad, the Kelly clamp, and the Kelly rectoscope). Yet, however numerous and diversified this array of eponyms in medicine,[27] they are still reserved, of course, to only a small fraction of the many who have labored in the medical vineyard. Eponymy is a prize that, though large in absolute aggregate, is limited to the relatively few.

Time does not permit, nor does the occasion require, detailed examination of eponymous practices in all the other sciences. Consider, then, only two other patterns: In a special branch of physics, it became the practice to honor great physicists by attaching their names to electrical and magnetic units (as with volt, ohm, ampere, coulomb, farad, joule, watt, henry, maxwell, gauss, gilbert and oersted). In biology, it is the long-standing practice to append the name of the first describer to the name of a species, a custom which greatly agitated Darwin since, as he saw it, this put "a premium on hasty and careless work" as the "species-mongers" among naturalists try to achieve an easy immortality by "miserably describ[ing] a species in two or three lines."[28] (This, I may say, will not be the last occasion for us to see how the system of rewards in science can be stepped up to such lengths as to get out of hand and defeat its original purposes.)

Eponymy is only the most enduring and perhaps most prestigious kind of recognition institutionalized in science. Were the reward-system confined to this, it would not provide for the many other distinguished scientists without whose work the revolutionary discoveries could not have been made. Graded rewards in the coin of the scientific realm—honorific recognition by fellow-scientists—are distributed among the stratified layers of scientific accomplishment. Merely to list some of these other but still considerable forms of recognition will perhaps be enough to remind us of the complex structure of the reward-system in science.

In recent generations, the Nobel Prize, with nominations for it made by scientists of distinction throughout the world, is perhaps the pre-eminent token of recognized achievement in science.[29] There is also an iconography of fame in science, with medals honoring famous scientists and the recipients of the award alike (as with the Rumford medal and the Arago medal). Beyond these, are memberships in honorary academies and sciences (for example, the Royal Society and the French Academy of Sciences), and fellowships in national and local societies. In those nations that still preserve a titled aristocracy, scientists have been ennobled, as in England since the time when Queen Anne added laurels to her crown by knighting Newton, not, as might be supposed, because of his superb administrative work as Master

[27] It has been suggested that, in medicine at least, eponymic titles are given to diseases only so long as they are poorly understood. "Any disease designated by an eponym is a good subject for research." (O. H. Perry Pepper, *Medical Etymology*, Philadelphia: W. B. Saunders Co., 1949, pp. 11–12.)

[28] Exercised by the excesses eponymy in natural history had reached, the usually mild Darwin repeatedly denounced this "miserable and degrading passion of mere species naming." What is most in point for us is the way in which the pathological exaggeration of eponymizing highlights the normal role of eponymy in providing its share of incentives for serious and sustained work in science. Francis Darwin, ed., *The Life and Letters of Charles Darwin*, New York: Appelton, 1925, Vol. I, pp. 332-344.

[29] On the machinery and results of the Nobel and other prize-awards, see Barber, *Science and the Social Order, op. cit.*, pp. 108 ff.; Leo Moulin, "The Nobel Prizes for the Sciences, 1901–1950," *British Journal of Sociology*, 6 (September, 1955), pp. 246–263.

of the Mint, but for his scientific discoveries. These things move slowly; it required almost two centuries before another Queen of England would, in 1892, confer a peerage of the realm upon a man of science for his work in science, and thus transform the pre-eminent Sir William Thomson into the no less eminent Lord Kelvin.[30] Scientists themselves have distinguished the stars from the supporting cast by issuing directories of "starred men of science" and universities have been known to accord honorary degrees to scientists along with the larger company of philanthropists, industrialists, businessmen, statesmen and politicians.

Recognition is finally allocated by those guardians of posthumous fame, the historians of science. From the most disciplined scholarly works to the vulgarized and sentimentalized accounts designed for the millions, great attention is paid to priority of discovery, to the iteration and reiteration of 'firsts.' In this way, many historians of science help maintain the prevailing institutional emphasis on the importance of priority. One of the most eminent among them, the late George Sarton, at once expresses and exemplifies the commemorative function of historiography when he writes that ". . . the first scholar to conceive that subject [the history of science] as an independent discipline and to realize its importance was . . . Auguste Comte." He then goes on to propose that great scholar, Paul Tannery, as most deserving to be called "the father of our studies," and finally states the thesis that ". . . as the historian is expected to determine not only the relative truth of scientific ideas at different chronological states, but also their relative novelty, he is irresistibly led to the fixation of *first* events." [31]

Although scientific knowledge is impersonal, although its claim to truth must be assessed entirely apart from its source, the historian of science is called upon to prevent scientific knowledge from sinking (or rising) into anonymity, to preserve the collective memory of its origins. Anonymous givers have no place in this scheme of things. Eponymity, not anonymity, is the standard. And, as we have seen, outstanding scientists, in turn, labor hard to have their names inscribed in the golden book of firsts.[32]

Seen in composite, from the eponyms enduringly recording the names of scientists in the international language of science to the immense array of parochial and ephemeral prizes, the reward-system of science reinforces and perpetuates the institutional emphasis upon originality. It is in this specific sense that originality can be said to be a major institutional goal of modern science, at times, the paramount one, and recognition for originality a derived, but often as heavily

[30] For caustic comment on the lag in according such recognition to men of science, see excerpts from newspapers of the day in Silvanus P. Thompson, *The Life of William Thomson: Baron Kelvin of Largs,* London: Macmillan, 1910, Vol. II, pp. 906–907.

[31] George Sarton, *The Study of the History of Science,* Cambridge: Harvard University Press, 1936, pp. 3–4, 35–36. Sarton goes on to observe that this practice of identifying first events "never fails to involve him [the historian] in new difficulties, because creations absolutely *de novo* are very rare, if they occur at all; most novelties are only novel combinations of old elements and the degree of novelty is thus a matter of interpretation, which may vary considerably according to the historian's experience, standpoint, or prejudices. . . . It is always risky, yet when every reasonable precaution has been taken one must be willing to run the risk and make the challenge, for this is the only means of being corrected, if correction be needed." (*Ibid.,* p. 36.) This is a telling sign of the deep-rooted sentiment that recognition for originality in science must be expressed, that it is an obligation—"the historian is expected . . ."—to search out the 'first' to contribute an idea or finding, even though a comprehensive view of the cumulative and interlocking character of scientific inquiry suggests that the attribution of 'firsts' is often difficult and sometimes arbitrary. For a further statement on this matter of priority, see George Sarton, *The Study of the History of Mathematics,* Cambridge: Harvard University Press, 1936, pp. 33–36.

I cannot undertake here to examine the attitudes commonly manifested by historians of science toward this emphasis on searching out priorities. It can be said that these too are often ambivalent.

[32] This was presumably not always so. As is well known, medieval authors often tried to cloak their writings in anonymity. But this is not the place to examine the complex subject of variations in cultural emphases upon originality and recognition. For some observations on this, see George Sarton, *A Guide to the History of Science,* Waltham, Mass.: Chronica Botanica Co., 1952, p. 23, who reminds us of ancient and medieval practices in which "modest authors would try to pass off their own compositions under the name of an illustrious author of an earlier time," ghost-writing in reverse. See also R. K. Merton, *Science, Technology and Society in Seventeenth Century England,* Bruges, Belgium: Osiris, 1938, pp. 360–632, at p. 528.

emphasized, goal. In the organized competition to contribute to man's scientific knowledge, the race *is* to the swift, to him who gets there first with his contribution in hand.

Institutional Norm of Humility. If the institution of science placed great value *only* on originality, scientists would perhaps attach even more importance to recognition of priority than they do. But, of course, this value does not stand alone. It is only one of a complex set making up the ethos of science —disinterestedness, universalism, organized scepticism, communism of intellectual property, and humility being some of the others.[33] Among these, the socially enforced value of humility is in most immediate point, serving, as it does, to reduce the misbehavior of scientists below the rate that would occur if importance were assigned only to originality and the establishing of priority.

The value of humility takes diverse expression. One form is the practice of acknowledging the heavy indebtedness to the legacy of knowledge bequeathed by predecessors. This kind of humility is perhaps best expressed in the epigram Newton made his own: "If I have seen farther, it is by standing on the shoulders of giants" (this, incidentally, in a letter to Hooke who was then challenging Newton's priority in the theory of colors.) [34] That this tradition has not always been honored in practice can be inferred from the admiration that Darwin, himself lavish in such acknowledgments, expressed to Lyell for "the elaborate honesty with which you quote the words of all living and dead geologists." [35] Exploring the literature of a field of science becomes not only an instrumental practice, designed to learn from the past, but a commemorative practice, designed to pay homage to those who have prepared the way for one's work.

Humility is expected also in the form of the scientist's insisting upon his personal limitations and the limitations of scientific knowledge altogether. Galileo taught himself and his pupils to say, "I do not know." Perhaps another often-quoted image by Newton most fully expresses this kind of humility in the face of what is yet to be known:

> I do not know what I may appear to the world, but to myself I seem to have been only like a boy playing on the seashore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me.[36]

If this contrast between public image ("what I may appear to the world") and self-image ("but to myself I seem") is fitting for the greatest among scientists, it is presumably not entirely out of place for the rest. The same theme continues unabated. Laplace, the Newton of France, in spite of what has been described as "his desire to shine in the constantly changing spotlight of public esteem," reportedly utters an epigrammatic paraphrase of Newton in his last words, "What we know is not much; what we do not know is immense." [37] Lagrange summarizes his lifetime of discovery in the one phrase, "I do not know." And Lord Kelvin, at the Jubilee celebrating his fifty years as a distinguished scientist in the course of which he was honored by scores of scientific societies and academies, characterizes his lifelong effort to develop a grand and

[33] For a review of other values of science, see Barber, *op. cit.*, Chapter IV; Merton, *Social Theory and Social Structure, op. cit.*, pp. 552–561; H. A. Shepard, "The Value System of a University Research Group," *American Sociological Review,* 19 (August, 1954), pp. 456–462.

[34] Alexander Koyré, "An unpublished letter of Robert Hooke to Isaac Newton," *Isis,* 43 (December, 1952), pp. 312–337, at p. 315.

[35] Darwin, *op. cit.*, I, p. 263.

[36] David Brewster, *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton,* Edinburgh and London, 1855, Volume II, Chapter xxvii. For our purposes, unlike those of the historian, it is a matter of indifference whether Newton actually felt acutely modest or was merely conforming to expectation. In either case, he expresses the norm of personal humility, which is widely held to be appropriate. I. B. Cohen, (*op. cit.*, pp. 47, 58, passim) repeatedly and incisively makes the point that both admirers and critics of Newton have failed to make the indispensable distinction between what he said and what he did.

[37] Bell, *op. cit.*, p. 172. Bell refers also to "a common and engaging trait of the truly eminent scientist in his frequent confession of how little he knows. . . ." What he describes as a trait of the scientist can also be seen as an expectation on the part of the community of scientists. It is not that many scientists *happen* to be humble men; they are *expected* to be humble. See E. T. Bell, "Mathematics and Speculation," *The Scientific Monthly,* 32 (March, 1931), pp. 193–209, at p. 204.

comprehensive theory of the properties of matter by the one word, "Failure." [38]

Like all human values, the value of modesty can be vulgarized and run into the ground by excessive and thoughtless repetition. It can become merely conventional, emptied of substance and genuine feeling. There really *can* be too much of a good thing. It is perhaps this excess which led Charles Richet, himself a Nobel laureate, to report the quiet self-appraisal by a celebrated scientist: "I possess every good quality, but the one that distinguishes me above all is modesty." [39] Other scientists, for example, the great Harvard mathematician, George Birkhoff, will have no truck with modesty, whether false, prim, or genuine. Having been told by a Mexican physicist of his hope that the United States would continue "to send us savants of your stature," Birkhoff sturdily replied. "Professor Erro, in the States I *am* the only one of my stature." And as Norbert Wiener is reported to have said in his obituary address for Birkhoff, "He was the first among us and he accepted the fact. He was not modest." [40] Nevertheless, such forthright acknowledgement of one's eminence is not quite the norm among scientists.

It would appear, then, that the institution of science, like other institutions, incorporates potentially incompatible values: among them, the value of originality, which leads scientists to want their priority to be recognized, and the value of humility, which leads them to insist on how little they have 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 [41] into a single orientation, to reconcile them in practice, is no easy matter. Rather, as we shall now see, the tension between these kindred values—kindred as Cain and Abel were kin—creates an inner conflict among men of science who have internalized both of them and generates a distinct ambivalence toward the claiming of priorities.

AMBIVALENCE TOWARD PRIORITY

The components of this ambivalence are fairly clear. After all, to insist on one's originality by claiming priority is not exactly humble and to dismiss one's priority by ignoring it is not exactly to affirm the value of originality.[42] As a result of this conflict, scientists come to despise themselves for wanting that which the institutional values of science have led them to want.

With the rare candor that distinguishes him, Darwin so clearly exhibits this agitated ambivalence in its every detail that this one case can be taken as paradigmatic for many others (which are matters of less detailed and less candid record). In his *Autobiography*, he writes that, even before his historic voyage on the Beagle in 1831, he was "ambitious to take a fair place among scientific men—whether more ambitious or less so than most of my fellow-workers, I can form no

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38 G. F. Fitzgerald, *Lord Kelvin, 1846–99.* Jubilee Commemoration Volume, with an Essay on his Works, 1899; S. P. Thompson, *Life of William Thomson,* Vol. II, Chapter XXIV.

39 See the gallery of trenchant pen-portraits of scientists in Charles Richet, *The Natural History of a Savant,* trans. by Sir Oliver Lodge, New York: Doran, 1927, p. 86.

40 Carlos Graef Fernandez (as transcribed by Samuel Kaplan), "My Tilt with Albert Einstein," *American Scientist,* 44 (April, 1956), pp. 204–211, at p. 204.

41 For further examination of the problem of blending incompatible norms into stable patterns of behavior, in this case among physicians, see R. K. Merton, "Some Preliminaries to a Sociology of Medical Education," in R. K. Merton, G. G. Reader and P. L. Kendall, eds., *The Student-Physician,* Cambridge: Harvard University Press, 1957, p. 72 ff. As is well known, R. S. Lynd has set forth the general notion that institutional norms are organized as near-incompatibles; see his *Knowledge for What?,* Princeton: Princeton University Press, 1939, Chapter III.

42 Strictly speaking, originality and priority are of course not the same thing. Belated independent rediscoveries of what was long since known may represent great originality on the part of the rediscoverer, as is perhaps best shown in the remarkable case of the self-taught twentieth-century Indian mathematician, Srinivasa Ramanujan, who, all unknowing that it had been done before, re-created much of early nineteenth-century mathematics, and more besides. *Cf.* G. H. Hardy, *Ramanujan: Twelve Lectures Suggested by His Life and Work,* Cambridge: Harvard University Press, 1940. Edwin G. Boring, who has long been interested in the subject of priority in science, has, among many other perceptive observations, noted the lack of identity between originality and priority. See, for example, his early paper, "The Problem of Originality in Science," *American Journal of Psychology,* 39 (December, 1927), pp. 70–90, esp. at p. 78.

opinion."[43] A quarter of a century after this voyage, he is still wrestling with his ambition, exclaiming in a letter that "I wish I could set less value on the bauble fame, either present or posthumous, than I do, but not, I think, to any extreme degree. . . ."[44]

Two years before the traumatizing news from Wallace, reporting his formulation of the theory of evolution, Darwin writes his now-famous letter to Lyell, explaining that he is not quite ready to publish his views, as Lyell had suggested he do in order not to be forestalled, and again expressing his uncontrollable ambivalence in these words: "I rather hate the idea of writing for priority, yet I certainly should be vexed if any one were to publish my doctrines before me."[45]

And then, in June 1858, the blow falls. What Lyell warned would happen and what Darwin could not bring himself to believe could happen, as all the world knows, did happen. Here is Darwin writing Lyell of the crushing event:

> [Wallace] has today sent me the enclosed, and asked me to forward it to you. It seems to me well worth reading. Your words have come true with a vengeance—that I should be forestalled. . . . I never saw a more striking coincidence; if Wallace had my MS. sketch written out in 1842, he could not have made a better short abstract! Even his terms now stand as heads of my chapters. . . . So all my originality, whatever it may amount to, will be smashed. . . .[46]

Humility and disinterestedness urge Darwin to give up his claim to priority; the wish for originality and recognition urges him that all need not be lost. At first, with typical magnanimity, but without pretense of equanimity, he makes the desperate decision to step aside altogether. A week later, he is writing Lyell again; perhaps he might publish a short version of his long-standing text, "a dozen pages or so." And yet, he says in his anguished letter, "I cannot persuade myself that I can do so honourably." Torn by his mixed feelings, he concludes his letter, "My good dear friend, forgive me. This is a trumpery letter, influenced by trumpery feelings." And in an effort finally to purge himself of his feelings, he appends a postscript, "I will never trouble you or Hooker on the subject again."[47]

The next day he writes Lyell once more, this time to repudiate the postscript. Again, he registers his ambivalence: "It seems hard on me that I should lose my priority of many years' standing, but I cannot feel at all sure that this alters the justice of the case. First impressions are generally right, and I at first thought it would be dishonourable in me now to publish."[48]

As fate would have it, Darwin is just then prostrated by the death of his infant daughter. He manages to respond to the request of his friend Hooker and sends him the Wallace manuscript and his own original sketch of 1844, "solely," he writes, "that you may see by your own handwriting that you did read it. . . . Do not waste much time. It is miserable in me to care at all about priority."[49]

Other members of the scientific community do what the tormented Darwin will not do for himself. Lyell and Hooker take matters in hand and arrange for that momentous session in which both papers are read at the Linnean Society. And as they put it in their letter prefacing the publication of the joint paper of "Messrs. C. Darwin and A. Wallace," "in adopting our present course . . . we have explained to him [Darwin] that we are not solely considering the relative claims to priority of himself and his friend, but the interests of science generally."[50] Despite this disclaimer of interest in priority, be it noted that scientific *knowledge* is not the richer or the poorer for having credit given where credit is due; it is the social *institution* of science and individual men of science that would suffer from repeated failures to allocate credit justly.

This historic and not merely historical episode so plainly exhibits the ambivalence occasioned by the double concern with priority and modesty that it need not be examined

[43] Darwin, *op. cit.*, p. 54.

[44] *Ibid.*, p. 452.

[45] *Ibid.*, pp. 426–427.

[46] *Ibid.*, p. 473.

[47] *Ibid.*, pp. 474–475.

[48] *Ibid.*, p. 475.

[49] *Ibid.*, p. 476.

[50] "On the Tendency of Species to Form Varieties and on the Perpetuation of Varieties and Species by Natural Means of Selection," by C. Darwin and A. R. Wallace. Communicated by Sir C. Lyell and J. D. Hooker, *Journal of the Linnean Society,* 3 (1859), p. 45. Read July 1, 1858.

further. Had the institutionalized emphasis on originality been alone in point, the claim to priority would have invited neither self-blame nor self-contempt; publication of the long antecedent work would have proclaimed its own originality. But the value of originality was joined with the value of humility and modesty. To insist on priority would be to trumpet one's own excellence, but scientific peers and friends of the discoverers, acting as a third party in accord with the institutional norms, could with full propriety announce the joint claims to originality that the discoverers could not bring themselves to do. Underneath it all lies a deep and agitated ambivalence toward priority.

I have not yet counted the recorded cases of debates about priority in science and the manner of their outcome. Such a count, moreover, will not tell the full story for it will not include the doubtless numerous instances in which independent ideas and discoveries were never announced by those who found their ideas anticipated in print. Nevertheless, I have the strong impression that disputes, even bitter disputes, over priority outnumber the cases of despondent but unreserved admission that the other fellow had made the discovery first.

The institutional values of modesty and humility are apparently not always enough to counteract both the institutional emphasis upon originality and the actual workings of the system of allocating rewards. Originality, as exemplified by the new idea or the new finding, is more readily observable by others in science and is more fully rewarded than the often unobservable kind of humility that keeps an independent discoverer from reporting that he too had had the same idea or the same finding. Moreover, after publication by another, it is often difficult, if not impossible, to demonstrate that one had independently arrived at the same result. For these and other reasons, it is generally an unequal contest between the values of recognized originality and of modesty. Great modesty may elicit respect, but great originality promises everlasting fame.

In short, the social organization of science allocates honor in a way that tends to vitiate the institutional emphasis upon modesty. It is this, I believe, which goes far toward explaining why so many scientists, even those who are ordinarily men of the most scrupulous integrity, will go to great lengths to press their claims to priority of discovery. As I have often suggested, perhaps too often, any *extreme* institutional

> emphasis upon achievement—whether this be scientific productivity, accumulation of wealth or, by a small stretch of the imagination, the conquests of a Don Juan—will attenuate conformity to the institutional norms governing behavior designed to achieve the particular form of 'success,' especially among those who are socially disadvantaged in the competitive race.[51]

Or more specifically and more completely, great concern with the goal of recognition for originality can generate a tendency toward sharp practices just inside the rules of the game or sharper practices far outside. That this has been the case with the behavior of scientists who were all-out to have their originality recognized, the rest of this paper will try to show.

TYPES OF RESPONSE TO CULTURAL EMPHASIS ON ORIGINALITY

Fraud in Science. The extreme form of deviant behavior in science would of course

[51] Merton, *op. cit.*, p. 166. Scientists do not all occupy similar positions in the social structure; there are, consequently, differentials in access to *opportunity* for scientific achievement (and, of course, differences of individual capacity for achievement). The theory of the relations of social structure to anomie requires us to explore differential pressures upon those scientists variously located in the social structure. Contrast only the disputatious Robert Hooke, a socially mobile man whose rise in status resulted wholly from his scientific achievements, and the singularly undisputatious Henry Cavendish, high-born and very rich (far richer, and, by the canons of Burke's peerage, more elevated even than that other great aristocrat of science, Robert Boyle) who, in the words of Biot, was *"le plus riche de tous les savans; et probablement aussi, le plus savant de tous les riches."* Or consider what Norbert Wiener has said of himself, "I was competitive beyond the run of younger mathematicians, and I knew equally that this was not a very pretty attitude. However, it was not an attitude which I was free to assume or to reject. I was quite aware that I was an out among ins and I would get no shred of recognition that I did not force." (*I Am a Mathematician*, New York: Doubleday, 1956, p. 87.) But these are only straws in the wind; once again, limitations of space allow me only to identify a problem, not to examine it.

be the use of fraud to obtain credit for an original discovery. For reasons to be examined, the annals of science include very few instances of downright fraud although, in the nature of the case, an accurate estimate of frequency is impossible. Darwin, for example, said that he knew of only "three intentionally falsified statements" in science.[52] Yet, some time before, his contemporary, Charles Babbage, the mathematician and inventor of calculating machines (one of which prophetically made use of perforated cards), had angrily taken a classified inventory of fraud in science.[53]

At the extreme are hoaxes and forgery: the concocting of false data in science and learning—or, more accurately, in pseudoscience and anti-scholarship. Literary documents have been forged in abundance, at times, by men of previously unblemished reputation, in order to gain money or fame. Though no one can say with confidence, it appears that love of money was at the root of the forgery of fifty or so rare nineteenth-century pamphlets by that prince of bibliographers, that court of last appeal for the authentication of rare books and manuscripts, Thomas J. Wise. Of quite another stripe was John Payne Collier, the Shaksperian scholar who, unrivalled for his genuine finds in Elizabethan drama and "encouraged by the steadily growing plaudits of his colleagues," could not rest content with this measure of fame and proceeded to forge, with great and knowledgeable skill, a yet-uncounted array of literary papers.[54] But these rogues seem idle alongside the fecund and audacious Vrain-Lucas who, in the space of eight years, created more than 27,000 pieces of manuscript, all duly sold to Michel Chasles, perhaps the outstanding French geometer of the mid-nineteenth century, whose credulity stretches our own, inasmuch as this vast collection included letters by Pontius Pilate, Mary Magdalene, the resurrected Lazarus, Ovid, Luther, Dante, Shakspere, Galileo, Pascal and Newton, all written on paper and in modern French. Most provocative among these documents was the correspondence between Pascal and the then eleven-year-old Newton (all in French, of course, although even at the advanced age of thirty-one Newton could struggle through French only with the aid of a dictionary), for these letters made it plain that Pascal, not Newton, had, to the greater glory of France, first discovered the law of gravitation, a momentous correction of history, which for several years excited the interest of the *Académie des Sciences* and usurped many pages of the *Comptes Rendus* until, in 1869, Vrain-Lucas was finally brought to book and sentenced to two years in prison. For our purposes, it is altogether fitting that Vrain-Lucas should have had Pascal address this maxim to the boy Newton: *"Tout homme qui n'aspire pas à se faire un nom n'exécutera jamais rien de grand."* [55]

Such lavish forgery is unknown to science proper, but the pressure to demonstrate the truth of a theory or to produce a sensational discovery has occasionally led to the faking of scientific evidence. The biologist Paul Kammerer produced specimens of spotted salamanders designed to prove the Lamarckian thesis experimentally; was thereupon offered a chair at the University of Moscow where in 1925 the Lamarckian views of Michurin held reign; and upon proof that the specimens were fakes, attributed the fraud to a research assistant and committed

[52] Darwin, *op. cit.*, p. 84.

[53] Charles Babbage, *The Decline of Science in England,* London, 1830, pp. 174–183. George Lundberg has independently noted that "a scientist's greed for applause [sometimes] becomes greater that his devotion to truth." [*Social Research,* New York: Longmans Green, 1929, p. 34 (and in less detail, in the second edition, 1946, p. 52).]

[54] I have drawn these examples of frauds in anti-scholarship from the zestful and careful account by Richard D. Altick, *The Scholar Adventurers,* New York: Macmillan, 1951, Chapters 2 and 6.

[55] The definitive reports on the Vrain-Lucas affair by M. P. Faugère and by Henri Bordier and Mabille are not available to me at this telling; substantial details, including extracts from the court-proceedings, are given by the paleographer, Étienne Charavay, *Affair Vrain-Lucas: Étude Critique,* Paris, 1870; a more accessible summary that does not, however, do full justice to the prodigious inventiveness of Vrain-Lucas is provided by J. A. Farrer, *Literary Forgeries,* London: Longmans Green, 1907, Chapter XII. The biographer of Newton, Sir David Brewster, at the age of 87, did his share to safeguard the integrity of historical scholarship, but this did not prevent Chasles from prizing the three thousand letters of Galileo which he had acquired from his friend, although they happened to be in French, rather than in the Latin or Italian in which Galileo wrote.

suicide.[56] Most recently, the Piltdown man —that is, the skull and jaw from which his existence was inferred—has been shown, after forty years of uneasy acceptance, to be a carefully contrived hoax.[57]

Excessive concern with "success" in scientific work has on occasion led to the types of fraud Babbage picturesquely described as "trimming" and "cooking." The trimmer clips off "little bits here and there from observations which differ most in excess from the mean, and [sticks] . . . them on to those which are too small . . . [for the unallowable purpose of] 'equitable adjustment.' " The cook makes "multitudes of observations" and selects only those which agree with an hypothesis and, as Babbage says, "the cook must be very unlucky if he cannot pick out fifteen or twenty which will do for serving up." This eagerness to demonstrate a thesis can, on occasion, lead even truth to be fed with cooked data, as it did for the neurotic scientist, described by Lawrence Kubie, "who had proved his case, but was so driven by his anxieties that he had to bolster an already proved theorem by falsifying some quite unneccessary additional statistical data." [58]

The great cultural emphasis upon recognition for original discovery can lead by gradations from these rare practices of outright fraud to more frequent practices just beyond the edge of acceptability, sometimes without the scientist's being aware that he has exceeded allowable limits. Scientists may find themselves reporting only "successful experiments or results, so-called, and neglecting to report 'failures.'" Alan Gregg, that informed observer of the world of medical research, practice, and education, reports the case of

> the medical scientist of the greatest distinction who told me that during his graduate fellowship at one of the great English universities he encountered for the first time the idea that in scientific work one should be really honest in reporting the results of his experiments. Before that time he had always been told and had quite naturally assumed that the point was to get his observations and theories accepted by others, and published.[59]

Yet, these deviant practices should be seen in perspective. What evidence there is suggests that they are extremely infrequent, and this temporary focus upon them will surely not be distorted into regarding the exceptional case as the typical. Apart from the moral integrity of scientists themselves and this is, of course, the major basis for honesty in science, there is much in the social organization of science that provides a further compelling basis for honest work. Scientific research is typically, if not always, under the exacting scrutiny of fellow-experts, involving, as it usually though not always does, the verifiability of results by others. Scientific inquiry is in effect subject to rigorous policing, to a degree perhaps unparalleled in any other field of human activity. Personal honesty is supported by the public and testable character of science. As Babbage remarked, "the cook would [at best] procure a temporary reputation . . . at the expense of his permanent fame."

Competition in the realm of science, intensified by the great emphasis on original and significant discoveries, may occasionally generate incentives for eclipsing rivals by illicit or dubious means. But this seldom occurs in the form of preparing fraudulent data; instead, it appears in quite other forms of deviant behavior involving spurious claims to discovery. More concretely, it is an occasional theft rather than forgery, and more often, libel and slander rather than theft that are found on the small seamy side of science.

Plagiary: Fact and Slander. Deviant behavior most often takes the form of occasional plagiaries and many slanderous charges or insinuations of plagiary. The historical record shows relatively few cases

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[56] Martin Gardner, *In the Name of Science*, New York: G. P. Putnam's Sons, 1952, p. 143; W. S. Beck, *Modern Science and the Nature of Life*, New York: Harcourt, Brace, 1957, pp. 201–202; Conway Zirkle, "The Citation of Fraudulent Data," *Science*, 120 (30 July, 1954), pp. 189–190.

[57] William L. Straus, Jr., "The Great Piltdown Hoax," *Science*, 119 (26 February, 1954), pp. 265–269.

[58] Lawrence S. Kubie, M.D., "Some Unsolved Problems of the Scientific Career," *American Scientist*, 41 (1953), pp. 596–613; 42 (1954), pp. 104–112, at p. 606.

[59] Alan Gregg, *Challenges to Contemporary Medicine*, New York: Columbia University Press, 1956, p. 115.

(and of course the record may be defective) in which one scientist actually pilfered another. We are assured that in the *Mécanique céleste* (until then, outranked only by Newton's *Principia*) "theorems and formulae are appropriated wholesale without acknowledgement" by Laplace.[60] Or, to take a marginal case, Sir Everard Home, the distinguished English surgeon who was appointed custodian of the unpublished papers of his even more distinguished brother-in-law, John Hunter, published 116 papers of uncertain origin in the *Philosophical Transactions* after Hunter's death, and burned Hunter's manuscripts, an action greatly criticized by knowledgeable and suspicious contemporaries.[61] It is true also that Robert Boyle, not impressed by the thought that theft of his ideas might be a high tribute to his talent, was in 1688 driven to the desperate expedient of printing an "Advertisement about the Loss of many of his Writings," later describing the theft of his work and reporting that he would from then on write only on loose sheets, in the hope that these would tempt theives less than "bulky packets" and, going on to say that he was resolved to send his writings to press without extensive revision in order to avoid prolonged delays.[62] But even with such cases of larceny on the grand scale, the aggregate of demonstrable theft in modern science is not large.

What does loom large is the repeated practice of charging others with pilfering scientific ideas. Falsely accused of plagiarizing Harvey in physiology, Snell in optics, and Harriot and Fermat in geometry, Descartes in turn accuses Hobbes and the teen-age Pascal of plagiarizing him.[63] To maintain his property, Descartes implores his friend Mersenne, "I also beg you to tell him [Hobbes] as little as possible about what you know of 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." [64] All unknowing that the serene and unambitious Gauss had long since discovered the method of least squares, Legendre, himself "a man of the highest character and scrupulously fair," practically accuses Gauss of having filched the idea from him and complains that Gauss, already so well-stocked with momentous discoveries, might at least have had the decency not to adopt his brainchild.[65]

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[60] As stated by the historian of astronomy, Agnes Mae Clerke, in her article on Laplace in the eleventh edition of the *Encyclopaedia Britannica.* Some of Clerke's further observations are much in point: "In the delicate task of apportioning his own large share of merit, he certainly does not err on the side of modesty; but it would perhaps be as difficult to produce an instance of injustice, as of generosity in his estimate of others. Far more serious blame attaches to his all but total suppression in the body of the work—and the fault pervades the whole of his writings—of the names of his predecessors and contemporaries . . . a production which may be described as the organized result of a century of patient toil presents itself to the world as the offspring of a single brain." And yet, since these matters are seldom all of a piece, "Biot relates that, when he himself was beginning his career, Laplace introduced him at the Institute for the purpose of explaining his supposed discovery of equations of mixed differences, and afterwards showed him, under a strict pledge of secrecy, the papers, then yellow with age, in which he had long before obtained the same results." (Vol. XVI, pp. 201–202.) As we shall see, Gauss, who was meticulous in acknowledging predecessors, treated the young Bolyai as did Laplace the young Biot.

[61] Ralph H. Major, *A History of Medicine,* Oxford: Blackwell Scientific Publications, 1954, Vol. II, p. 703.

[62] The account by A. M. Clerke in the article on Boyle in the *Dictionary of National Biography* is somewhat mistaken in attributing charges of plagiary to the published Advertisement. This speaks only of losses of manuscript through "unwelcome accidents" (e.g., the upsetting of corrosive liquors over a file of manuscripts) and at most hints at less impersonal sources of loss. But a later unpublished paper by Boyle, dug up by his biographer Birch, is levelled against the numerous plagiarists of his works. This document, running to three folio pages of print, is a compendium of the ingenious devices for thievery developed by the grand larcenists of seventeenth-century science. See *The Works of the Honourable Robert Boyle,* in six volumes, to which is prefixed The Life of the Author, by J. Birch, London, 1772, Volume I, pp. cxxv–cxxviii, ccxxii–ccxxiv.

[63] For the case of Harvey, see A. R. Hall, *The Scientific Revolution, 1500–1800,* London: Longmans, Green, 1954, p. 148; for Hobbes, see Descartes, *Oeuvres,* (edited by Charles Adam and Paul Tannery), *Correspondance,* Paris, 1899, Vol. III, pp. 283 ff.; for Pascal, see *ibid.,* 1903, Vol. V, p. 366.

[64] Descartes, *ibid.*, Vol. III, p. 320.

[65] Bell, *op. cit.,* pp. 259–260. Legendre seems to have been particularly sensitive to these matters, perhaps because he was often victimized; note Clerke's remark that between Laplace and Legendre "there was a feeling of 'more than coldness,'

At times, the rivalrous concern with priority can go so far as to set, not the Egyptians against the Egyptians, but brother against brother, as in the case of the great eighteenth-century mathematicians, the brothers Jacob and Johannes Bernoulli, who repeatedly and bitterly attacked one another's claims to priority. (Johannes improved on this by throwing his own son out of the house for having won a prize from the French Academy on which he himself had had his eye.)[66]

Or to turn to our own province, Comte, tormented by the suggestion that his law of three stages had really been originated by St. Simon, denounces his one-time master and describes him as a "superficial and depraved charlatan."[67] Again, to take Freud's own paraphrase, Janet claims that "everything good in psychoanalysis repeats, with slight modifications, the views of Janet—everything else in psychoanalysis being bad."[68] Freud refuses to lock horns with Janet in what he describes as "gladiator fights in front of the noble mob," but some years later, his disciple, Ernest Jones, reports that at a London Congress he has "put an end to" Janet's pretensions, and Freud applauds him in a letter that urges him to "strike while the iron is hot," in the interests of "fair play."[69]

So the almost changeless pattern repeats itself. Two or more scientists quietly announce a discovery. Since it is often the case that these are truly independent discoveries, with each scientist having separately exhibited originality of mind, the process is sometimes stabilized at that point, with due credit to both, as in the instance of Darwin and Wallace. But since the situation is often ambiguous with the role of each not easy to demonstrate and since each *knows* that he had himself arrived at the discovery, and since the institutionalized stakes of reputation are high and the joy of discovery immense, this is often not a stable solution. One or another of the discoverers—or frequently, his colleagues or fellow-nationals—suggests that he rather than his rival was really first, and that the independence of the rival is at least unproved. Then begins the familiar deterioration of standards governing conflictful interaction: the other side, grouping their forces, counter 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 often by chauvinism, the controversy gains force, mutual recriminations of plagiary abound, and there develops an atmosphere of thoroughgoing hostility and mutual distrust.

On some occasions, this can lead to outright deceit in order to buttress valid claims, as with Newton in his controversy with Leibniz over the invention of the calculus. When the Royal Society finally established a committee to adjudicate the rival claims, Newton, who was then president of the Royal Society, packed the committee, helped direct its activities, anonymously wrote the preface for the second published report—the draft is in his handwriting—and included in that preface a disarming reference to the old legal maxim that "no one is a proper witness for himself [and that] he would be an iniquitous Judge, and would crush underfoot the laws of all the people, who would admit anyone as a lawful witness in his own cause."[70] We can gauge the immense pres-

owing to his appropriation, with scant acknowledgment, of the other's labors." *Encyclopaedia Britannica,* Vol. XVI, p. 202.

[66] Bell, *op. cit.*, p. 134.

[67] Frank E. Manuel, *The New World of Henri Saint-Simon,* Cambridge: Harvard University Press, 1956, pp. 340–342; also Richard L. Hawkins, *Auguste Comte and the United States,* Cambridge: Harvard University Press, 1936, pp. 81–82, as cited by Manuel.

[68] Sigmund Freud, *History of the Psychoanalytic Movement,* London: Hogarth Press; also, Freud, *An Autobiographical Study,* London: Hogarth Press, 1948, pp. 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 Janet's not always delicate insinuations, see his *Psychological Healing,* New York: Macmillan, 1925, I, pp. 601–640.

[69] Ernest Jones, *Sigmund Freud: Life and Work,* London: Hogarth Press, 1955, Vol. II, p. 112.

[70] There is a sizeable library discussing the Newton-Leibniz controversy. I have drawn chiefly upon More, *op. cit.*, who devotes the whole of Chapter XV to this subject; Auguste de Morgan, *Essays on the Life and Works of Newton,* Chicago: Open Court Pub. Co., 1914, esp. Appendix II; and

sures for self-vindication that must have operated for such a man as Newton to have adopted these means for defense of his valid claims. It was not because Newton was so weak but because the institutionalized values were so strong that he was driven to such lengths.

This interplay of offensive and defensive maneuvers—no doubt, students of the theory of games can recast it more rigorously—thus gives further emphasis to priority. Scientists try to exonerate themselves in advance from possible charges of filching by going to great lengths to establish their priority of discovery. Often, this kind of anticipatory defense produces the very result it was designed to avoid by inviting others to show that prior announcement or publication need not mean there was no plagiary.

The effort to safegard priority and to have proof of one's integrity has led to a variety of institutional arrangements designed to cope with this strain on the system of rewards. In the seventeenth century, for example, and even as late as the nineteenth, discoveries were sometimes reported in the form of anagrams—as with Galileo's "triple star" of Saturn and Hooke's law of tension—for the double purpose of establishing priority of conception and of yet not putting rivals on to one's original ideas, until they had been further worked out.[71] Then, as now, complex ideas were quickly published in abstracts, as when Halley urged Newton to do so in order to secure "his invention to himself till such time as he would be at leisure to publish it."[72] There is also the long-standing practice of depositing sealed and dated manuscripts with scientific academies in order to protect both priority and idea.[73] Scientific journals often print the date on which the manuscript of a published article was received, thus serving, even apart from such intent, to register the time it first came to notice. Numerous personal expedients have been developed: for example, letters detailing one's own ideas are sent off to a potential rival, thus disarming him; preliminary and confidential reports are circulated among a chosen few; personal records of research are meticulously dated (as by Kelvin). Finally, it has often been suggested that the functional equivalent of a patent-office be established in science to adjudicate rival claims to priority.[74]

In prolonged and yet overly quick summary, these are some of the forms of deviance invited by the institutional emphasis on priority and some of the institutional expedients devised to reduce the frequency of these deviations. But as we would expect from the theory of alternative responses to excessively emphasized goals, other forms of behavior, verging toward deviance though still well within the law and not as subject to moral disapproval as the foregoing, have also made their appearance.

Alternative Responses to Emphasis on Originality. The large majority of scientists, like the large majority of artists, writers, doctors, bankers and bookkeepers, have little pros-

Brewster, *op. cit.*, Chapter XXII; *cf.* Cohen, *op. cit.*, who is properly critical of the biography by More at various points (e.g., pp. 84–85). On the basis of his examination of the Portsmouth Papers, More concludes that "the principals, and practically all those associated with them wantonly made statements which were false; and not one of them came through with a clean record." (P. 567.) E. N. da C. Andrade has aptly summed up Newton's ambivalence in this judgment: "Evidence can be cited for the view that Newton was modest or most overweening; the truth is that he was a very complex character . . . when not worried or irritated he was modest about his achievements." See also Andrade's *Sir Isaac Newton*, London: Collins, 1954, esp. pp. 131–132.

[71] The earlier widespread use of anagrams is well-known. As late as the 19th century, the physicists Balfour Stewart and P. G. Tait reintroduced this practice and "to secure priority . . . [took] the unusual step of publishing [their idea] as an anagram in *Nature* some months before the publication of their book." Sir J. J. Thomson, *Recollections and Reflections*, London: G. Bell, 1936, p. 22.

[72] Thomas Birch, *The History of the Royal Society of London*, London, 1756–1757, Vol. IV, p. 437.

[73] For a recent instance, see the episode described by Wiener in which the race between Bouligand and Wiener to contribute new concepts "in potential theory" ended in a "dead heat," since Bouligand had submitted his "results to the [French] Academy in a sealed envelope, after a custom sanctioned by centuries of academy tradition," (Wiener, *op. cit.*, p. 92.)

[74] J. Hettinger, "Problems of Scientific Property and Its Solution," *Science Progress*, 26 (January, 1932), pp. 449–461; also the paper by Dr. A. L. Soresi, of the New York Academy of Medicine, cited by Bernhard J. Stern, *Social Factors in Medical Progress*, New York: Columbia University Press, 1927, p. 108.

pect of great and decisive originality. For most of us artisans of research, getting things into print becomes a symbolic equivalent to making a significant discovery. Nor could science advance without the great unending flow of papers reporting careful investigations, even if these are routine rather than distinctly original. The indispensable reporting of research can, however, become converted into an itch to publish that, in turn, becomes aggravated by the tendency, in many academic institutions, to transform the sheer number of publications into a ritualized measure of scientific or scholarly accomplishment.[75]

The urge to publish is given a further push by the moral imperative of science to make one's work known to others; it is the obverse to the culturally repudiated practice of jealously hoarding scientific knowledge for oneself. As Priestley liked to say, "whenever he discovered a new fact in science, he instantly proclaimed it to the world, in order that other minds might be employed upon it besides his own." [76] Indeed, John Aubrey, that seventeenth-century master of the thumbnail biography and member of the Royal Society, could extend the moral imperative for communication of knowledge to justify even plagiary if the original author will not put his ideas into print. In his view it was better to have scientific goods stolen and circulated than to have them lost entirely.[77]

To this point (and I provide comfort by reporting that the end of the paper is in sight), we have examined types of deviant responses to the institutional emphasis on priority that are *active* responses: the fabrication of "data," aggressive self-assertion, the denouncing of rivals, plagiary, and charges of plagiary. Other scientists have responded to the same pressures *passively* or at least by internalizing their aggressions and directing them against themselves.[78] Since these passive responses, unlike the active ones, are private and often not publicly observable, they seldom enter the historical record. This need not mean, of course, that passive withdrawal from the competition for originality in science is infrequent; it might simply mean that the men responding in this fashion do not come to public notice, unless they do so after their accomplishments have qualified them for the pages of history.

Chief among these passive deviant responses is what has been described, on occasion, as *retreatism,* the abandoning of the once-esteemed cultural goal of originality and of practices directed toward reaching that goal. In such instances, the scientist withdraws from the field of inquiry, either by giving up science altogether or by confining himself to some alternative role in it, such as teaching or administration. (This does not say, of course, that teaching and administration do not have their own attractions, or that they are less significant than inquiry; I refer here only to the scientists who reluctantly abandon their research because it does not measure up to their own standards of excellence.)

A few historical instances of such retreatism must stand in place of more. The nineteenth-century physicist Waterston, his classic paper on molecular velocity having been rejected by the Royal Society as "noth-

[75] There is not space here to examine the institutional conditions which lead the piling up of publications to become a virtually ritualistic activity.

[76] Priestley's remark as paraphrased by his longtime friend, T. L. Hawkes, and reported by George Wilson, *op. cit.*, p. 111. The 17th-century Dutch genius of microscopy, Anton van Leeuwenhoek, also adopted a policy, as he described it, that "whenever I found out anything remarkable, I have thought it my duty to put down my discovery on paper, so that all ingenious people might be informed thereof." (Quoted by Major, *History of Medicine,* Vol. I, p. 531.) The same sentiment was expressed by St.-Simon, among many others. *Cf.* Manuel, *op. cit.,* pp. 63–64.

[77] Aubrey could say, irresponsibly and probably without malice, that the mathematician John Wallis "may stand with much glory upon his owne basis, and need not be beholding to any man, for Fame, yet he is so greedy of glorie, that he steales feathers from others to adorn his own cap; e.g. he lies at watch, at Sir Christopher Wren's discours, Mr. Robert Hooke's, &c.; putts down their notions in his note booke, and then prints it, without owneing the authors. This frequently of which they complaine. But though he does an Injury to the Inventors, he does good to Learning, in publishing such curious notions, which the author (especially Sir Christopher Wren) might never have the leisure to write of himselfe," (John Aubrey, *Brief Lives,* ed. by Andrew Clark, Oxford, 1898, Vol. II, pp. 281–282.)

[78] The distinction between active and passive forms of deviant behavior is drawn from Talcott Parsons, *The Social System,* Glencoe: The Free Press, 1951, pp. 256–267.

ing but nonsense," becomes hopelessly discouraged and leaves science altogether.[79] Deeply disappointed by the lack of response to his historic papers on heredity, Mendel refuses to publish the now-permanently lost results of his further research and, after becoming abbot of his monastery, gives up his research on heredity.[80] Robert Mayer, tormented by refusals to grant him priority for the principle of conservation of energy, tries a suicide leap from a third-story window and succeeds only in breaking his legs and being straitjacketed, for a time, in an insane asylum.[81]

Perhaps the most telling instance of retreatism in mathematics is that of Janos Bolyai, inventor of one of the non-Euclidean geometries. The young Bolyai tries to obey his mathematician-father who, out of the bitter fruits of his own experience, warns his son to give up any effort to prove the postulate on parallels—or, as his father more picturesquely put it, to "detest it just as much as lewd intercourse; it can deprive you of all your leisure, your health, your rest, and the whole happiness of your life." He dutifully becomes an army officer instead, but his demon does not permit the twenty-one-year-old Bolyai to leave the postulate alone. After years of work, he develops his geometry, sends the manuscript to his father who in turn transmits it to Gauss, the prince of mathematicians, for a magisterial opinion. Gauss sees in the work proof of authentic genius, writes the elder Bolyai so, and adds, in all truth, that he cannot express his enthusiasm as fully as he would like, for "to praise it, would be to praise myself. Indeed, the whole contents of the work, the path taken by your son, the results to which he is led, coincide almost entirely with my meditations, which have occupied my mind partly for the last thirty or thirty-five years . . . I am very glad that it is just the son of my old friend, who takes the precedence of me in such a remarkable manner." Delighted by this accolade, the elder Bolyai sends the letter to his son, innocently saying that it is "very satisfactory and redounds to the honor of our country and our nation." Young Bolyai reads the letter, but has no eye for the statements which say that his ideas are sound, that in the judgment of the incomparable Gauss he is blessed with genius. He sees only that Gauss has anticipated him. For a time, he believes that his father must have previously confided his ideas to Gauss who had thereupon made them his own.[82] His priority lost, and, with the further blow, years later, of coming upon Lobachevsky's non-Euclidean geometry, he never again publishes any work in mathematics.[83]

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[79] Murray, *op. cit.*, pp. 346–348; and David L. Watson, *Scientists are Human*, London: Watts & Co., 1938, pp. 58, 80; Baron Rayleigh, *op. cit.*, 169–171. Evidently, Sidney Lee, the editor of the *Dictionary of National Biography* by the time it reached the volume in which Waterston should have had an honored place, could not penetrate the obscurity into which the great discoverer was plunged by the unfounded rejection of his work; there is no biography of Waterston in the *DNB*.

[80] Hugo Iltis, *Life of Mendel*, New York: W. W. Norton, 1932, pp. 111–112; and see Mendel's prophetic remark, "My time will come," p. 282.

[81] Mayer's having been rejected by his liberal friends who took part in the revolution of 1848, which he as a conservative opposed, may have contributed to his disturbance. For some recent evidence on how Mayer's priority was safeguarded by the lay-sociologist Josef Popper, see Otto Blüh, "The Value of Inspiration: A Study on Julius Robert Mayer and Josef Popper-Lynkeus," *Isis*, 43 (September, 1952), pp. 211–220. Blüh's opinion that claims of priority in science are no longer taken seriously seems exaggerated.

[82] The principal source on the Bolyais, including the germane correspondence, is Paul Stäckel, *Wolfgang und Johann Bolyai, Geometrische Untersuchungen*, Leipzig: 1913, two vols. which was not available to me at this writing. An excellent short account is provided by Roberto Bonola, *Non-Euclidean Geometry* (trans. by H. S. Carslaw), La Salle, Illinois: Open Court Publishing Company, 1938, 2d rev. ed., pp. 96–113; see also Dirk J. Struik, *A Concise History of Mathematics*, New York: Dover Publications, 1948, Vol. II, pp. 251–254; Franz Schmidt, "Lebensgeschichte des Ungarischen Mathematikers Johann Bolyai de Bolya," *Abhandlungen zur Geschichte der Mathematik*, 8 (1898), pp. 135–146.

[83] Two letters provide context for Bolyai's great fall from the high peak of exhilaration into the slough of despond. In 1823, he writes his father: ". . . the goal is not yet reached, but I have made such wonderful discoveries that I have been almost overwhelmed by them, and it would be the cause of continual regret if they were lost. When you will see them, you too will recognize it. In the meantime I can say only this: *I have created a new universe from nothing*. All that I have sent you till now is but a house of cards compared to the tower. I am as fully persuaded that it will bring me honor, as if I had already completed the discovery." And just as, a generation later, Lyell was prophetically to warn Darwin of being forestalled, so does the elder Bolyai warn the younger: "If you have really succeeded in the

Apart from historical cases of notable scientists retreating from the field after denial of the recognition owing them, there are many contemporary cases that come to the notice of psychiatrists rather than historians. Since Lawrence Kubie is almost alone among psychiatrists to have described these in print, I shall draw upon his pertinent account of the maladaptations of scientists suffering from an unquenched thirst for original discovery and ensuing praise.

When the scientist's aspirations become too lofty to be realized, the result sometimes is apathy, imbued with fantasy. In Kubie's words.

> the young scientist may dwell for years in secret contemplation of his own unspoken hope of making great scientific discoveries. As time goes on, his silence begins to frighten him; and in the effort to master his fear, he may build up a secret feeling that his very silence is august, and that once he is ready to reveal his theories, they will shake the world. Thus a secret megalomania can hide among the ambitions of the young research worker.[84]

Perhaps most stressful of all is the situation in which the recognition accorded the scientist is not proportioned to his industry or even to the merit of his work. He may find himself serving primarily to remove obstacles to fundamental discoveries by others. His "negative experiments clear the road for the steady advance of science, but at the same time they clear the road for the more glamorous successes of other scientists, who may have used no greater intelligence, skill or devotion; perhaps even less."[85] Like other men, scientists become disturbed by the pan-human problem of evil, in which "the fortunes of men seem to bear practically no relation to their merits and efforts."[86]

Kubie hazards some further observations that read almost as if they were describing the behavior of delinquents in response to a condition of relative anomie. "Success or failure, whether in specific investigations or in an entire career may be almost accidental, with chance a major factor in determining not *what* is discovered, but when and by whom. . . . Yet young students are not warned that their future success may be determined by forces which are outside their own creative capacity or their willingness to work hard."[87] As a result of all this, Kubie suspects the emergence of what he calls a "new psychosocial ailment among scientists which may not be wholly unrelated to the gangster tradition of dead-end kids. Are we witnessing the development of a generation of hardened, cynical, amoral, embittered, disillusioned young scientists?"

Lacking the evidence, this had best be left as a rhetorical question. But the import of the question needs comment. There have been diagnoses of the ways in which a culture giving emphasis to aspirations for all, aspirations which cannot be realized by many, exerts a pressure for deviant behavior and for cynicism, for rejection of the reigning moralities and the rules of the game. We see here the possibility that the same pressures may in some degree be at work in the institution of science. But even though the pres-

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question, it is right that no time be lost in making it public, for two reasons: first, because ideas pass easily from one to another, who can anticipate its publication; and secondly, there is some truth in this, that many things have an epoch, in which they are found at the same time in several places, just as the violets appear on every side in spring. Also every scientific struggle is just a serious war, in which I cannot say when peace will arrive. Thus we ought to conquer when we are able, since the advantage is aways to the first comer." (Quoted by Bonola, *op. cit.*, pp. 98, 99.) Small wonder that though young Bolyai continued to work sporadically in mathematics, he never again published the results of his work.

[84] Kubie, "Some Unsolved Problems of the Scientific Career," *op. cit.*, p. 110.

[85] *Ibid.*

[86] Gilbert Murray, quoted in a similar theoretical context by Merton, *op. cit.*, p. 147.

[87] *Ibid.*, pp. 111–112. This reading of the case is not inconsistent with the facts of multiple independent discoveries and inventions. As the long history of multiple discoveries makes clear, and as W. F. Ogburn and D. S. Thomas among the sociologists have shown, certain discoveries become almost "inevitable" when the cultural base cumulates to a certain level. But this still leaves some indeterminacy in the matter of *who* will *first* make the discovery. Kubie mentions some "near-misses" of discoveries that suggest undoubted merit is not all when it comes to the *first* formulation of a discovery, and this list can be greatly extended. In the nature of the case, moreover, we often do not know of those scientists who have abandoned a line of inquiry that was moving toward a particular discovery when they found it had been made and announced by another. These "personal tragedies" of near-discovery—tragedy in terms of the prevailing cutural belief that all credit is due him who is "first,"—are the silent tragedies that leave no mark in the historiography of science.

sures are severe, they need not produce deviant behavior. There are great differences between the social structure of science and other social structures in which deviance is frequent. Among other things, the institution of science continues to have an abiding emphasis on other values that curb the culturally induced tendency toward deviation, an emphasis on the value of truth by whomsoever it is found, and a commitment to the disinterested pursuit of truth. Simply because we have focused on the deviant behavior of scientists, we should not forget how relatively rare this is. Only a few try to gain reputation by means that will lose them repute. Scientists may feel the pressures whose institutional sources I have tried to describe, but we can suppose that most will continue in the future as they manifestly have done in the past to abide by the institutional norms.

FUNCTIONS AND DYSFUNCTIONS OF EMPHASIS ON PRIORITY

It has sometimes been said that the emphasis upon recognition of priority has the function of motivating scientists to make discoveries. For example, Sir Frederick Banting, the major figure in the discovery of insulin-therapy for diabetes, was long disturbed by the conviction that the chief of his department had been given too much credit for what he had contributed to the discovery. Time and again, Banting returned to the importance of allocating due credit for a discovery: ". . . it makes research men," he said. "It stimulates the individuality and develops personality. Our religion, our moral fabric, our very basis of life are centered round the idea of reward. It is not abnormal therefore that the research man should desire the kudos of his own work and his own idea. If this is taken away from him, the greatest stimulant for work is withdrawn." [88]

From this, it would seem that the institutional emphasis is maintained with an eye to its functional utility. But as I have tried to show, the emphasis upon priority is often not confined within functional limits. Once it becomes established, forces of rivalrous interaction lead it to get out of hand. Recognition of priority, operating to reward those who advanced science materially by being the first to make a significant discovery, becomes a sentiment in its own right. Rationalized as a means of providing incentives for original work and as expressing esteem for those who have done much to advance science, it becomes transformed into an end-in-itself. It becomes stepped up to a dysfunctional extreme far beyond the limits of utility.[89] It can even reach the revealing extreme where, for example, the permanent secretary of the French Academy of Sciences, François Arago, could exclaim (apropos of the controversy involving Cavendish and Watt) that to describe discoveries as having been made " 'about the same time' proves nothing; questions as to priority may depend on weeks, on days, on hours, on minutes." [90]

When the criteria of priority become as finely discriminated as this—and Arago only put in words what many others have expressed in behavior—then priority has lost

[88] Quoted in Lloyd Stevenson, *Sir Frederick Banting,* London: Heinemann Medical Books, 1947, p. 301. Two hundred years before, John Morgan, the celebrated founder of the first American medical school, had expressed the same conception, but in sociologically more acceptable terms. To his mind, personal motivation for fame was linked with the social benefit of the advancement of science. Men of science, he said, "have the highest motives that can animate the pursuits of a generous mind. They consider themselves as under the notice of the public, to which every ingenious person labours to approve himself. A love of fame and a laudable ambition allure him with the most powerful charms. These passions have, in all ages, fired the souls of heroes, of patriots, of lovers of science, have made them renowned in war, eminent in government and peace, justly celebrated for the improvement of polite and useful knowledge." In effect, "other-directedness" can be functional to the society, providing that the criteria of judgment by others are sound. See John Morgan, *A Discourse upon the Institution of Medical Schools in America,* photo-offset reprint of first edition, Philadelphia, 1765, Baltimore: The Johns Hopkins Press, 1937, pp. 59–60.

[89] For suggestive observations on the process of "stepping up patterns to unanticipated extremities," a process which he called "perseveration," see W. I. Thomas, *Primitive Behavior,* New York: McGraw-Hill, 1937, p. 9 and passim; see also, Merton, *op. cit.,* pp. 199 ff. As I have tried to show in this paper, science has experienced this stepping-up of functional norms to an extreme at which they become dysfunctional to the workings of the institution.

[90] M. [F.] Arago, *Historical Éloge of James Watt,* trans. by J. P. Muirhead, London, 1839, p. 106. The whole of this document and Arago's role in the Adams-LeVerrier controversy clearly exemplify the forces producing conflicts over priority.

all functional significance. For when two scientists independently make the same discovery months or weeks apart, to say nothing of days or hours, it can scarcely be thought that one has exhibited greater originality than the other or that the short interim that separates them can be used to speed up the rate of scientific achievement.

CONCLUSION

The interpretation I have tried to develop here is not, I am happy to say, a new one. Nor do I consider it fully established and beyond debate. After all, neither under the laws of logic nor under the laws of any other realm, must one become permanently wed to an hypothesis simply because one has tentatively embraced it. But the interpretation does seem to account for some of the otherwise puzzling aspects of conflicts over priority in science and it is closely bound to a body of sociological theory.

In short review, the interpretation is this. Like other social institutions, the institution of science has its characteristic values, norms, and organization. Among these, the emphasis on the value of originality has a self-evident rationale, for it is originality that does much to advance science. Like other institutions also, science has its system of allocating rewards for performance of roles. These rewards are largely honorific, since even today, when science is largely professionalized, the pursuit of science is culturally defined as being primarily a disinterested search for truth and only secondarily, a means of earning a livelihood. In line with the value-emphasis, rewards are to be meted out in accord with the measure of accomplishment. When the institution operates effectively, the augmenting of knowledge and the augmenting of personal fame go hand in hand; the institutional goal and the personal reward are tied together. But these institutional values have the defects of their qualities. The institution can get partly out of control, as the emphasis upon originality and its recognition is stepped up. The more thoroughly scientists ascribe an unlimited value to originality, the more they are in this sense dedicated to the advancement of knowledge, the greater is their involvement in the successful outcome of inquiry and their emotional vulnerability to failure.

Against this cultural and social background, one can begin to glimpse the sources, other than idiosyncratic ones, of the misbehavior of individual scientists. The culture of science is, in this measure, pathogenic. It can lead scientists to develop an extreme concern with recognition which is in turn the validation by peers of the worth of their work. Contentiousness, self-assertive claims, secretiveness lest one be forestalled, reporting only the data that support an hypothesis, false charges of plagiarism, even the occasional theft of ideas and in rare cases, the fabrication of data,—all these have appeared in the history of science and can be thought of as deviant behavior in response to a discrepancy between the enormous emphasis in the culture of science upon original discovery and the actual difficulty many scientists experience in making an original discovery. In this situation of stress, all manner of adaptive behaviors are called into play, some of these being far beyond the mores of science.

All this can be put more generally. We have heard much in recent years about the dangers brought about by emphasis on the relativity of values, about the precarious condition of a society in which men do not believe in values deeply enough and do not feel strongly enough about what they do believe. If there is a lesson to be learned from this review of some consequences of a belief in the absolute importance of originality, perhaps it is the old lesson that unrestricted belief in absolutes has its dangers too. It can produce the kind of fanatic zeal in which anything goes. In its way, the absolutizing of values can be just as damaging as the decay of values to the life of men in society.[91]

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[91] Limitations of time and space do not allow me to do as I originally intended: to examine patterns of rediscovery, that is, the independent but considerably later discovery of something that had been found before but was since lost to view. These patterns have their own sociological characteristics which have not been considered here. A systematic sociological investigation of priority and rediscovery in science is being planned to test the validity of the interpretations set out in this paper and of other hypotheses mercifully omitted from it.

It is of some interest that just when this paper was in galley proof, all the world came to experience the social, political, and scientific repercussions of a spectacular "first" in science, when Russian scientists put a man-made sphere into space.