Resistance by Scientists to Scientific Discovery

This source of resistance has yet to be given the scrutiny accorded religious and ideological sources.

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In the study of the history and sociology of science, there has been a relative lack of attention to one of the interesting aspects of the social process of discovery—the resistance on the part of scientists themselves to scientific discovery. General and specialized histories of science and biographies and autobiographies of scientists, as well as intensive discussions of the processes by which discoveries are made and accepted, all tend to make, at the most, passing reference to this subject. In two systematic analyses of the social process of scientific discovery and invention, for example—analyses which tried to be as inclusive of empirical fact and theoretical problem as possible—there is only passing reference to such resistance in the one instance and none at all in the second (*1*). This neglect is all the more notable in view of the close scrutiny that scholars have given the subject of resistance to scientific discovery by social groups other than scientists. There has been a great deal of attention paid to resistance on the part of economic, technological, religious, and ideological elements and groups outside science itself (*1–3*). Indeed, the tendency of such elements to resist seems sometimes to be emphasized disproportionately as against the support which they also give to science. In the matter of religion, for example, are we not all a little too much aware that religion has resisted scientific discovery, not enough aware of the large support it has given to Western science? (*4, 5*).

The mere assertion that scientists themselves sometimes resist scientific discovery clashes, of course, with the sterotype of the scientist as "the open-minded man." The norm of open-mindedness is one of the strongest of the scientist's values. As Philipp Frank has recently put it, "Every influence of moral, religious, or political considerations upon the acceptance of a theory is regarded as 'illegitimate' by the so-called 'community of scientists.'" And Robert Oppenheimer emphasizes the "importance" of "the open mind," in a book by that title, as a value not only for science but for society as a whole (*6*). But values alone, and especially one value by itself, cannot be a sufficient basis for explaining human behavior. However strong a value is, however large its actual influence on behavior, it usually exerts this influence only in conjunction with a number of other cultural and social elements, which sometimes reinforce it, sometimes give it limits.

This article is an investigation of the elements within science which limit the norm and practice of "open-mindedness." My purpose is to draw a more accurate picture of the actual process of scientific discovery, to see resistance by scientists themselves as a constant phenomenon with specifiable cultural and social sources. This purpose, moreover, implies a practical consequence. For if we learn more about resistance to scientific discovery, we shall know more also about the sources of acceptance, just as we know more about health when we successfully study disease. By knowing more about both resistance and acceptance in scientific discovery, we may be able to reduce the former by a little bit and thereby increase the latter in the same measure.

Helmholtz, Planck, and Lister

Although the resistance by scientists themselves to scientific discovery has been neglected in systematic analysis, it would be surprising indeed if it had never been noted at all. If nowhere else, we should find it in the writings of those scientists who have suffered from resistance on the part of other scientists. Helmholtz, for example, made aware of such resistance by his own experience, commiserated with Faraday on "the fact that the greatest benefactors of mankind usually do not obtain a full reward during their life-time, and that new ideas need the more time for gaining general assent the more really original they are" (*7–9*). Max Planck is another who noticed resistance in general because he had experienced it himself, in regard to some new ideas on the second law of thermodynamics which he worked out in his doctoral dissertation submitted to the University of Munich in 1879. Ironically, one of those who resisted the ideas proposed in Planck's paper, according to his account, was Helmholtz: "None of my professors at the University had any understanding for its contents," says Planck. "I found no interest, let alone approval, even among the very physicists who were closely connected with the topic. Helmholtz probably did not even read my paper at all. Kirchhoff expressly disapproved . . . I did not succeed in reaching Clausius. He did not answer my letters, and I did not find him at home when I tried to see him in person at Bonn. I carried on a correspondence with Carl Neumann, of Leipzig, but it remained totally fruitless" (*10*, p. 18). And Lister, in a graduation address to medical students, warned them all against blindness to new ideas in science, blindness such as he had encountered in advancing his theory of antisepsis.

Scientists Are Also Human

Too often, unfortunately, where resistance by scientists has been noted, it has been merely noted, merely alleged, without detailed substantiation and without attempt at explanation. Sometimes, when explanations are offered, they are notably vague and all-inclusive, thus proving too little by trying to prove too much. One such explanation is contained in the frequently repeated phrase, "After all, scientists are also

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human beings," a phrase implying that scientists are more human when they err than when they are right (*11*). Other such vague explanations can be found in phrases such as "*Zeitgeist*," "human nature," "lack of progressive spirit," "fear of novelty," and "climate of opinion."

As one of these phrases, "fear of novelty," may indicate, there has also been a tendency, where some explanation of the sources of resistance is offered, to express a psychologistic bias—that is, to attribute resistance exclusively to inherent and ineradicable traits or instincts of the human personality. Thus, Wilfred Trotter, in discussing the response to scientific discovery, asserts that "the mind delights in a static environment," that "change from without . . . seems in its very essence to be repulsive and an object of fear," and that "a little self-examination tells us pretty easily how deeply rooted in the mind is the fear of the new" (*12*). And Beveridge, in *The Art of Scientific Investigation*, says, "there is in all of us a psychological tendency to resist new ideas" (*13*). A full understanding of resistance will, of course, have to include the psychological dimension—the factor of individual personality. But it must also include the cultural and social dimensions—those shared and patterned idea-systems and those patterns of social interaction that also contribute to resistance. It is these cultural and social elements that I shall discuss here, but with full awareness that psychological elements are contributory causes of resistance.

Because resistance by scientists has been largely neglected as a subject for systematic investigation, we find that there is sometimes a tendency, when such resistance is noted, to exaggerate the extent to which it occurs. Thus, Murray says that the discoverer must *always* expect to meet with opposition from his fellow scientists. And Trotter goes overboard in the same way: "the reception of new ideas tends always to be grudging or hostile. . . . Apart from the happy few whose work has already great prestige or lies in fields that are being actively expanded at the moment, discoverers of new truths always find their ideas resisted" (*12*, p. 26). Such exaggerations can be eliminated by more systematic and objective study.

Finally, in the absence of such systematic and objective study, many of those who have noted resistance have been excessively embittered and moralistic. Oliver Heaviside is reported to have exclaimed bitterly, when his important contributions to mathematical physics were ignored for 25 years, "Even men who are not Cambridge mathematicians deserve justice" (*14*). And Planck's reaction to the resistance he experienced was similar. "This experience," he said, "gave me also an opportunity to learn a new fact—a remarkable one, in my opinion: A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (*10*). Such bitterness is not tempered by objective understanding of resistance as a constant phenomenon in science, a pattern in which all scientists may sometimes and perhaps often participate, now on the side of the resisters, now on that of the resisted. Instead, such bitterness takes the moralistic view that resistance is due to "human vanities," to "little minds and ignoble minds." Such views impede the objective analysis that is required.

In his discussion of the Idols—idols of the tribe, of the cave, of the marketplace, and of the theatre—Francis Bacon long ago suggested that a variety of preconceived ideas, general and particular, affect the thinking of all men, especially in the face of innovation. Similarly, more recent sociological theory has shown that while the variety of idea-systems that make up a given culture are functionally necessary, on the whole, for man to carry on his life in society and in the natural environment, these several idea-systems may also have their dysfunctional or negative effects. Just because the established culture defines the situation for man, usually helpfully, it also, sometimes harmfully, blinds him to other ways of conceiving that situation. Cultural blinders are one of the constant sources of resistance to innovations of all kinds. And scientists, for all the methods they have invented to strip away their distorting idols, or cultural blinders, and for all the training they receive in evading the negative effects of such blinders, are still as other men, though surely in considerably lesser measure because of these methods and this special training. Scientists suffer, along with the rest of us, from the ironies that evil sometimes comes from good, that one noble vision may exclude another, and that good scientific ideas occasionally obstruct the introduction of better ones.

Substantive Concepts

Several different kinds of cultural resistance to discovery may be distinguished. We may turn first to the way in which the substantive concepts and theories held by scientists at any given time become a source of resistance to new ideas. And our illustrations begin with the very origins of modern science. In his magisterial discussion of the Copernican revolution, Kuhn (*3*) tells us not only about the nonscientific opposition to the heliocentric theory but also about the resistance from the astronomer-scientists of the time. Even after the publication of *De Revolutionibus*, the belief of most astronomers in the stability of the earth was unshaken. The idea of the earth's motion was either ignored or dismissed as absurd. Even the great astronomer-observer Brahe remained a life-long opponent of Copernicanism; he was unable to break with the traditional patterns of thought about the earth's lack of motion. And his immense prestige helped to postpone the conversion of other astronomers to the new theory. Of course, religious, philosophical, and ideological conceptions were closely interwoven with substantive scientific theories in the culture of the scientists of that time, but it seems clear that the latter as well as the former played their part in the resistance to the Copernican discoveries.

Moving to the early 19th century, we learn that the scientists of the day resisted Thomas Young's wave theory of light because they were, as Gillispie says, faithful to a corpuscular model (*15*). By the end of the century, when scientists had swung over to the wave theory, the validity of Young's earlier discovery was recognized. Substantive scientific theory was also one of the sources of resistance to Pasteur's discovery of the biological character of fermentation processes. The established theory that these processes are wholly chemical was held to by many scientists, including Liebig, for a long time (*16*). The same preconceptions were also the source of the resistance to Lister's germ theory of disease, although in this case, as in that of Pasteur, various other factors were important.

Because it illustrates a variety of sources of scientific resistance to discovery, I shall return several times to the case of Mendel's theory of genetic inheritance. For the present, I mention it only in connection with the source of resistance under discussion, substantive scientific theories themselves. Mendelian theory, it seems clear, was resisted from the time of its announcement, in 1865, until the end of the century, because Mendel's conception of the separate inheritance of characteristics ran counter to the predominant conception of joint and total inheritance of biological characteristics (*17, 18*). It was not until botany changed its conceptions and concentrated its research on the separate inheritance of unit characteristics that Mendel's theory and Mendel himself were independently rediscovered by de Vries, a Dutchman, by Carl Correns, working in Tübingen, and by Erich Tschermak, a Viennese, all in the same year, 1900.

New conceptions about the electronic constitution of the atom were also resisted by scientists when fundamental discoveries in this field were being made at the end of the 19th century. The established scientific notion was that of the absolute physical irreducibility of the atom. When Arrhenius published his theory of electrolytic dissociation, his ideas met with resistance for a time, though eventually, thanks in part to Ostwald, the theory was accepted and Arrhenius was given the Nobel prize for it (*19*). Similarly, Lord Kelvin regarded the announcement of Röntgen's discovery of x-rays as a hoax, and as late as 1907 he was still resisting the discovery, by Ramsay and Soddy, that helium could be produced from radium, and resisting Rutherford's theory of the electronic composition of the atom, one of the fundamental discoveries of modern physics. Throughout his long and distinguished life in science Kelvin never discarded the concept that the atom is an indivisible unit (*20*).

Let us take one final illustration, from contemporary science. In a recent case history of the role of chance in scientific discovery it was reported that two able scientists, who observed, independently and by chance, the phenomenon of floppiness in rabbits' ears after the injection of the enzyme papain, both missed making a discovery because they shared the established scientific view that cartilage is a relatively inert and uninteresting type of tissue (*21*). Eventually one of the scientists did go on to make a discovery which altered the established view of cartilage, but for a long time even he had been blinded by his scientific preconceptions. This case is especially interesting because it shows how resistance occurs not only between two or more scientists but also within an individual scientist. Because of their substantive conceptions and theories, scientists sometimes miss discoveries that are literally right before their eyes.

Methodological Conceptions

The methodological conceptions scientists entertain at any given time constitute a second cultural source of resistance to scientific discovery and are as important as substantive ideas in determining response to innovations. Some scientists, for example, tend to be antitheoretical, resisting, on that methodological ground, certain discoveries. "In Baconian science," says Gillispie, "the bird-watcher comes into his own while genius, ever theorizing in far places, is suspect. And this is why Bacon would have none of Kepler or Copernicus or Gilbert or anyone who would extend a few ideas or calculations into a system of the world" (*15*). Goethe too, as Helmholtz pointed out in his discussion of Goethe's scientific researches, was antitheoretical (*22*). A more recent discussion of Gothe's scientific work also finds him antianalytical and antiabstract (*15*). Perhaps Helmholtz had been made aware of Goethe's antitheoretical bias because his own discovery of the conservation of energy had been resisted as being too theoretical, not sufficiently experimental. German physicists were probably antitheoretical in Helmholtz's day because they feared a revival of the speculations of the Hegelian "nature-philosophy" against which they had fought so long, and eventually successfully.

Viewed in another way, Goethe's antitheoretical bias took the form of a positive preference for scientific work based on intuition and the direct evidence of the senses. "We must look upon his theory of colour as a forlorn hope," says Helmholtz, "as a desperate attempt to rescue from the attacks of science the belief in the direct truth of our sensations" (*22*). Goethe felt passionately that Newton was wrong in analyzing color into its quantitative components by means of prisms and theories. Color, for him, was a qualitative essence projected onto the physical world by the innate biological character and functioning of the human being.

Later scientists also have resisted discovery because of their preference for the evidence of the senses. Otto Hahn, noted for his discoveries in radioactivity, who received the Nobel prize for his splitting of the uranium atom in 1939, reports the following case: "Emil Fischer was also one of those who found it difficult to grasp the fact that it is also possible by radioactive methods of measurement to detect, and to recognize from their chemical properties, substances in quantities quite beyond the world of the weighable; as is the case, for example, with the active deposits of radium, thorium, and actinium. At my inaugural lecture in the spring of 1907, Fischer declared that somehow he could not believe those things. For certain substances the most delicate test was afforded by the sense of smell and no more delicate test could be found than that!" (*23*).

Another methodological source of resistance is the tendency of scientists to think in terms of established models, indeed to reject propositions just because they cannot be put in the form of some model. This seems to have been a reason for resistance to discoveries in the theory of electromagnetism during the 19th century. Ampère's theory of magnetic currents, for example, was resisted by Joseph Henry and others because they did not see how it could be fitted into the Newtonian mechanical model (*24*). They refused to accept Ampère's view that the atoms of the Newtonian model had electrical properties which caused magnetic phenomena. And Lord Kelvin's resistance to Clerk Maxwell's electromagnetic theory of light was due, says Kelvin's biographer (*20*), to the fact that Kelvin found himself unable to translate into a dynamical model the abstract equations of Maxwell's theory. Kelvin himself, in the lectures he had given in Baltimore in 1884, had said, "I never satisfy myself until I can make a mechanical model of a thing. If I can make a mechanical model I can understand it. As long as I cannot make a mechanical model all the way through I cannot understand; and that is why I cannot get the electromagnetic theory" (*20*). Thus, models, while usually extremely helpful in science, can also be a source of blindness.

Scientists' positions on the usefulness of mathematics is a last methodological source of resistance to discovery. Some scientists are excessively partial to mathematics, others excessively hostile. Thus, when Faraday made his experimental discoveries on electromagnetism, Gillispie tells us, few mathematical physicists gave them any serious attention. The discoveries were regarded with indulgence or a touch of scorn as another example of the mathematical incapacity of the British, their barbarous emphasis on experiment, and their theoretical immaturity (*15*). Clerk Maxwell, however, resolved that he "would be Faraday's mathematicus"—that is, put Faraday's experimental discoveries into more mathematical, general, and theoretical a form. Initial resistance was thus overcome. Long ago Augustus De Morgan commented on the antimathematical prejudice of English astronomers of his time. In 1845, he pointed out, the Englishman Adams had, on the basis of mathematical calculations, communicated his discovery of the planet Neptune to his English colleagues. Because they distrusted mathematics, his discovery was not published, and eight months later the Frenchman Leverrier announced and published his simultaneous discovery of the planet, once again on the basis of mathematical calculations. Because the French admired mathematics, Leverrier's discovery was published first, and thus he gained a kind of priority over Adams (*25*).

Mendel was another scientist whose ideas were resisted because of the antimathematical preconceptions of the botany of his time. "It must be admitted, however," says his biographer, Iltis, "that the attention of most of the hearers [when he read his classic monograph, "Experiments in Plant-Hybridization," before the Brünn Society for the Study of Natural Science in 1865] was inclined to wander when the lecturer was engaged in rather difficult mathematical deductions; and probably not a soul among them really understood what Mendel was driving at. . . . Many of Mendel's auditors must have been repelled by the strange linking of botany with mathematics, which may have reminded some of the less expert among them of the mystical numbers of the Pythagoreans. . . ." (*18*). Note that the alleged "difficult mathematical deductions" are what we should now consider very simple statistics. And it was not just the audience in Brünn that had no interest in or knowledge of mathematics. Mendel's other biographer, Krumbiegel, tells us that even the more sophisticated group of scientists at the Vienna Zoological-Botanical Society would have given Mendel's theory as poor a reception, and for the same reasons.

In some quarters the antimathematical prejudice persisted in biology for a long time after Mendel's discovery, indeed until after he had been rediscovered. In his biography of Galton, Karl Pearson reports that he sent a paper to the Royal Society in October 1900, eventually published in November 1901, containing statistics in application to a biological problem (*26*). Before the paper was published, he says, "a resolution of the Council [of the Royal Society] was conveyed to me, requesting that in future papers mathematics should be kept apart from biological applications." As a result of this, Pearson wrote to Galton, "I want to ask your opinion about resigning my fellowship of the Royal Society." Galton advised against resigning, but he did help Pearson to found the journal *Biometrika*, so that there would be a place in which mathematics in biology would be explicitly encouraged. Galton wrote an article for the first issue of the new journal, explaining the need for this new agency of "mutual encouragement and support" for mathematics in biology and saying that "a new science cannot depend on a welcome from the followers of the older ones, and [therefore] . . . it is advisable to establish a special Journal for Biometry" (*27*). It seems strange to us now that prejudice against mathematics should have been a source of resistance to innovation in biology only 60 years ago.

Religious Ideas

Although we have heard more of the way in which religious forces outside science have hindered its progress, the religious ideas of scientists themselves constitute, after substantive and methodological conceptions, a third cultural source of resistance to scientific innovation. Such internal resistance goes back to the beginning of modern science. We have seen that the astronomer colleagues of Copernicus resisted his ideas in part because of their religious beliefs, and we know that Leibniz, for example, criticized Newton "for failing to make providential destiny part of physics" (*15*). Scientists themselves felt that science should justify God and His world. Gradually, of course, physics and religion were accommodated one to the other, certainly among scientists themselves. But all during the first half of the 19th century resistance to discovery in geology persisted among scientists for religious reasons. The difficulty, as Gillispie has put it on the basis of his classic analysis of geology during this period, "appears to be one of religion (in a crude sense) *in* science rather than one of religion *versus* scientists." The most embarrassing obstacles faced by the new sciences were cast up by the curious providential materialism of the scientists themselves (*5*). When, in the 1840's, Robert Chambers published his *Vestiges of Creation*, declaring a developmental view of the universe, the theory of development was so at variance with the religious views which all scientists accepted that "they all spoke out: Herschel, Whewell, Forbes, Owen, Prichard, Huxley, Lyell, Sedgwick, Murchison, Buckland, Agassiz, Miller, and others" (*5*, p. 133; *28, 29*).

Religious resistance continued and was manifested against Darwin, of course, although many of the scientists who had resisted earlier versions of developmentalism accepted Darwin's evolutionary theory, Huxley being not the least among them. In England, Richard Owen offered the greatest resistance on scientific grounds, while in America and, in fact, internationally, Louis Agassiz was the leading critic of Darwinism on religious grounds (*5, 29, 30*).

In more recent times, biology, like physics before it, has been successfully accommodated to religious ideas, and religious convictions are no longer a source of resistance to innovation in these fields. Resistance to discoveries in the psychological and social sciences that stems from religious convictions is perhaps another story, but one that does not concern us here.

In addition to shared idea-systems, the patterns of social interaction among scientists also become sources of resistance to discovery. Here again we are dealing with elements that, on the whole, probably serve to advance science but that occasionally produce negative, or dysfunctional, effects.

Professional Standing

The first of these social sources of resistance is the relative professional standing of the discoverer. In general, higher professional standing in science is achieved by the more competent, those who have demonstrated their capacity for being creative in their own right and for judging the discoveries of others. But sometimes, when discoveries are made by scientists of lower standing, they are resisted by scientists of higher standing partly because of the authority the higher position provides. Huxley commented on this social source of resistance in a letter he wrote in 1852: "For instance, I know that the paper I have just sent in is very original and of some importance, and I am equally sure that if it is referred to the judgment of my 'particular' friend that it will not be published. He won't be able to say a word against it, but he will pooh-pooh it to a dead certainty. You will ask with wonderment, Why? Because for the last twenty years [. . . .] has been regarded as the great authority in these matters, and has had no one tread on his heels, until, at last, I think, he has come to look upon the Natural World as his special preserve, and 'no poachers allowed.' So I must manoeuvre a little to get my poor memoir kept out of his hands" (*8*, p. 367).

Niels Henrik Abel, early in the 19th century, made important discoveries on a classical mathematical problem, equations of the fifth degree (*31*). Not only was Abel himself unknown but there was no one of any considerable professional standing in his own country, Norway (then part of Denmark), to sponsor his work. He sent his paper to various foreign mathematicians, the great Gauss among them. But Gauss merely filed the leaflet away unread, and it was found uncut after his death, among his papers. Ohm was another whose work, in this case experimental, was ignored partly because he was of low professional standing. The researches of an obscure teacher of mathematics at the Jesuit Gymnasium in Cologne made little impression upon the more noted scientists of the German universities.

Perhaps the classical instance of low professional standing helping to create resistance to a scientist's discoveries is that of Mendel. The notion that Mendel was "obscure," in the sense that his work did not come to the attention of competent and noted professionals in his field, can no longer be accepted. First of all, the proceedings volume of the Brünn society in which his monograph was printed was exchanged with proceedings volumes of more than 120 other societies, universities, and academies at home and abroad. Copies of his monograph went to Vienna and Berlin, to London and Petersburg, to Rome and Upsala (*18*). In London, according to Bateson, the monograph was received by the Royal Society and the Linnaean Society (*32*). Moreover, we know from the extensive correspondence between them—correspondence which was later published by Mendel's rediscoverer, Correns—that Mendel sent his paper to one of the distinguished botanists of his time, Carl von Nägeli of Munich (*15, 17, 18*). Von Nägeli resisted Mendel's theories for a number of reasons: because his own substantive theories about inheritance were different and because he was unsympathetic to Mendel's use of mathematics, but also because he looked down, from his position of authority, upon the unimportant monk from Brünn. Mendel had written deferentially to von Nägeli, in letters that amounted to small monographs. In these letters, Mendel addressed von Nägeli most respectfully, as an acknowledged master of the subject in which they were both interested. But von Nägeli was the victim of his own position as a scientific pundit. Mendel seemed to him a mere amateur expressing fantastic notions, or at least notions contrary to his own. Von Nägeli's letters to Mendel seem unduly critical to present readers, more than a little supercilious. Nevertheless, the modest Mendel was delighted that the great man had even deigned to reply and sent cordial thanks for the gift of von Nägeli's monograph. On both sides, von Nägeli was defined as the great authority, Mendel as the inferior asking for consideration his position did not warrant. Ironically, Mendel took von Nägeli's advice, to change from experiments on peas to work on hawkweed, a plant not at all suitable at that time for the study of inheritance of separate characteristics. The result was that Mendel labored in a blind alley for the rest of his scientific life.

Nor was von Nägeli unique. Others, such as W. O. Focke, Hermann Hoffman, and Kerner von Marilaun, also dismissed Mendel's work because he seemed "an insignificant provincial" to them. Focke did list Mendel's monograph in his own treatise, *Die Pflanzenmischlinge*, but only for the sake of completeness. Focke paid much more attention to those botanists who had produced quantitatively large and apparently more important contributions —men such as Kölreuter, Gärtner, Wichura, and Wiegmann, of higher professional standing (*33*). Certainly, in this case, quantity of publication was inadequate as a measure of professional worth. Focke's listing of Mendel served only to bring his work, directly and indirectly, to the attention of Correns, de Vries, and von Tschermak after they had independently rediscovered the Mendelian principle of inheritance.

Mendel met with resistance from the authorities in his field after his discovery was published. But sometimes men of higher professional standing sit in judgment on lesser figures *before* publication and prevent a discovery's getting into print. This can be illustrated by an incident in the life of Lord Rayleigh. For the British Association meeting at Birmingham in 1886, Rayleigh submitted a paper under the title, "An Experiment to show that a Divided Electric Current may be greater in both Branches than in the Mains." "His name," says his son and biographer, "was either omitted or accidentally detached, and the Committee 'turned it down' as the work of one of those curious persons called paradoxers. However, when the authorship was discovered, the paper was found to have merits after all. It would seem that even in the late 19th century, and in spite of all that had been written by the apostles of free discussion, authority could prevail when argument had failed!" (*34*). So says the fourth Baron Rayleigh, and we may wonder whether his remark does not still apply, some 75 years later.

Professional Specialization

Another social source of resistance is the pattern of specialization that prevails in science at any given time. On the whole, of course, as with any social or other type of system, such specialization is efficient for internal and environmental purposes. Specialization concentrates and focuses the requisite knowledge and skill where they are needed. But occasionally the negative aspect of specialization shows itself, and innovative "outsiders" to a field of specialization are resisted by the "insiders." Thus, when Helmholtz announced his theory of the conservation of energy, it met with resistance partly because he was not a specialist in what we now think of as physics. Referring in the later years of his life to the opposition of the acknowledged experts, Helmholtz said he met with such a remark as this from some of the older men: "This has already been well known to us; what does this young medical man imagine when he thinks it necessary to explain so minutely all this to us?" (*8*, p. 97). To be sure, on the other side, medical specialists have a long history of resisting scientific innovations from what they define as "the outside." Pasteur met with violent resistance from the medical men of his time when he advanced his germ theory. He regretted that he was not a medical specialist, for the medical men thought of him as a mere chemist poaching on their scientific preserves, not worthy of their attention. In France, even before Pasteur, Magendie had met with resistance for attempting to introduce chemistry into medicine (*35*). If medicine now listens more respectfully to nonmedical science and its discoveries, it is partly because many nonmedical scientists have themselves become experts in a variety of medical-science specialties and so are no longer "outsiders."

Societies, "Schools," and Seniority

Scientific organizations, as we may safely infer from their large number and their historical persistence, serve a variety of useful purposes for their members. And of course scientific publications are indispensable for communication in science. But occasionally, when organizations or publications are incompetently staffed and run, they may serve as another social source of resistance to innovation in science. There have been no scholarly investigations into the true history of our scientific organizations and publications, but something is known and points in the direction I have suggested. In the early 19th century, for example, even the Royal Society fell on bad days. Lyons tells us that a contemporary, Granville, "severely criticized the shortcomings of the Society" during that period (*36*). Granville gave numerous instances in which the selection or rejection of papers by the Committee of Papers was the result of bad judgment. Sometimes the paper had not been read by any Fellow who was an authority on the subject with which it dealt. In other cases, none of the members of the committee who made the judgment could have had any expert opinion in the matter. It was such an incompetent committee, for example, that resisted Waterston's new molecular theory of gases when he submitted a paper making this contribution. The referee of the Royal Society who rejected the paper wrote on it, "The paper is nothing but nonsense." As a result, Waterston's work lay in utter oblivion until rescued by Rayleigh some 45 years later (*12*, p. 26). Many present-day misjudgments of this kind probably occur, although the multiplicity of publication outlets now provides more than one chance for a significant paper ignored by the incompetent to appear in print.

The rivalries of what are called "schools" are frequently alleged to be another social source of resistance in science. Huxley, for example, is reported to have said, two years before his death, " 'Authorities,' 'disciples,' and 'schools' are the curse of science; and do more to interfere with the work of the scientific spirit than all its enemies" (*37*). Murray suggests that the supposed warfare between science and theology is equaled only by the warfare among rival schools in each of the scientific specialties. Unfortunately, just what the term *school* means is usually left unclear, and no empirical evidence of anything but the most meager and unsystematic character is ever offered by way of illustration (*38*). No doubt some harmful resistance to discovery, as well as some useful competition, comes out of the rivalry of "schools" in science, but until the concept itself is clarified, with definite indicators specified, and until research is carried out on this more adequate basis, we can only feel that "there is something there" that deserves a scholarly treatment it has not yet received.

That the older resist the younger in science is another pattern that has often been noted by scientists themselves and by those who study science as a social phenomenon. "I do not," said Lavoisier in the closing sentences of his memoir *Reflections on Phlogiston* (read before the Academy of Sciences in 1785), "expect my ideas to be adopted all at once. The human mind gets creased into a way of seeing things. Those who have envisaged nature according to a certain point of view during much of their career, rise only with difficulty to new ideas. It is the passage of time, therefore, which must confirm or destroy the opinions I have presented. Meanwhile, I observe with great satisfaction that the young people are beginning to study the science without prejudice. . . ." (*15*). Or again, Hans Zinsser remarks in his autobiography, "That academies and learned societies—commonly dominated by the older foofoos of any profession—are slow to react to new ideas is in the nature of things. For, as Bacon says, *scientia inflat*, and the dignitaries who hold high honors for past accomplishment do not usually like to see the current of progress rush too rapidly out of their reach" (*39*).

Now of course the older workers in science do not always resist the younger in their innovations, nor can it be physical aging in itself that is the source of such resistance as does occur. If we scrutinize carefully the two comments I have just quoted and examine other, similar ones with equal care, we can see that *aging* is an omnibus term which actually covers a variety of cultural and social sources of resistance. Indeed, we may put it this way, that as scientists get older they are more likely to be subject to one or another of the several cultural and social sources of resistance I have analyzed here. As a scientist gets older he is more likely to be restricted in his response to innovation by his substantive and methodological preconceptions and by his other cultural accumulations; he is more likely to have high professional standing, to have specialized interests, to be a member or official of an established organization, and to be associated with a "school." The likelihood of all these things increases with the passage of time, and so the older scientist, just by living longer, is more likely to acquire a cultural and social incubus. But this is not always so, and the older workers in science are often the most ardent champions of innovation.

After this long recital of the cultural and social sources of resistance, by scientists, to scientific discovery, I need to emphasize a point I have already made. That some resistance occurs, that it has specifiable sources in culture and social interaction, that it may be in some measure inevitable, is not proof either that there is more resistance than acceptance in science or that scientists are no more open-minded than other men. On the contrary, the powerful norm of open-mindedness in science, the objective tests by which concepts and theories often can be validated, and the social mechanisms for ensuring competition among ideas new and old—all these make up a social system in which objectivity is greater than it is in other social areas, resistance less. The development of modern science demonstrates this ever so clearly. Nevertheless, some resistance remains, and it is this we seek to understand and thus perhaps to reduce. If "the edge of objectivity" in science, as Charles Gillispie has recently pointed out, requires us to take physical and biological nature as it is, without projecting our wishes upon it, so also we have to take man's social nature, or his behavior in society, as it is. As men in society, scientists are sometimes the agents, sometimes the objects, of resistance to their own discoveries (*40*).

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Science and the News

Grand Strategy: The Administration Has a Problem That It Would Rather Not Deal With in Public

The Administration, as noted here last week, faces an interesting and delicate problem in dealing with the relationship between elements in the Defense Department and three closely tied organizations which advocate an unrelentingly aggressive prosecution of the Cold War in terms which take on a coherent meaning only in a context of preparing for a surprise nuclear attack on Russia sometime within the current decade. The basis for this interpretation of the "Forward Strategy" put forth by the Foreign Policy Research Institute of the University of Pennsylvania was reported in this space last week. The Research Institute has been financed primarily by a tax-free educational foundation, the Richardson Foundation, whose director of research, Frank Barnett, is also director of research for the Institute for American Strategy, another educational foundation, which is devoted to influencing the public to support the overt aspects of the Forward Strategy.

Public notice has been attracted to the relationships of these organizations with the Defense Department through an article by Gene M. Lyons and Louis Morton, of Dartmouth, published in the March 1961 issue of the *Bulletin of the Atomic Scientists*, and by Senator Fulbright's memorandum on right-wing activities by the military.

"The activities of the institute," Lyons and Morton wrote, "began to expand with the series of strategy seminars it has sponsored during the past 2 years. This program started with the National Strategy Seminar, sponsored jointly by the institute and the Reserve Officers Association in the summer of 1959. It was repeated in 1960 and both acted as catalysts for regional seminars held in different parts of the country. What is particularly striking about the National Strategy Seminars is that through the authorization of the Joint Chiefs of Staff, the Institute for American Strategy in effect took over the responsibility of training reserve officers on active duty, even though the