

Scientists with Half-closed Minds

IAN STEVENSON

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A surprising number are scared to death of new ideas. They have attacked major discoveries without even glancing at the evidence. And their distrust of unconventional experiments may now be hampering scientific progress.

A Dutchman living in the East Indies once tried to tell a native of Java that in his country the water sometimes becomes so hard you can walk on it. The Javan was immediately convulsed with laughter, and the Dutchman' could make no progress with his explanation.

We find this an amusing story, but it would be even funnier if it did not really refer to us all. Ordinarily our reaction to new ideas does not harm us or others. But when we make the discovery of new facts and new concepts our business, then incredulity can prove costly. When humans become scientists they continue to experience some of the less rational qualities of being human. And with this part of them they can get in each other's way, and in the way of progress.

Pierre Gassendi, for example, made notable contributions to seventeenth century physics. He devised the first atomic theory of matter since Democritus, and his works strongly influenced Newton. Yet when in 1627 someone reported the fall of a meteorite in Provence, Gassendi explained it as due to some unidentified volcanic eruption. This attitude toward meteorites was shared by nearly all astronomers and many other leading scientists for the next century and a half.

Some insisted that the stones had been picked up somewhere and carried by the wind; others accused those who claimed to have seen the stones fall of lying. In the late eighteenth century the great Antoine Lavoisier, himself a radical innovator in chemistry, rejected accounts of meteorites as the products of malobservation. Stones could not fallout of the sky, he declared, because none were there. Finally, in April 1803, a shower of small meteorites on L'Aigle, France, persuaded the astronomers to change their attitudes.

In the same way the first reports of hypnotism-or mesmerism, as it was called in the eighteenth and early nineteenth century-evoked many denials that the reported phenomena had ever occurred. In London, Dr. John Elliotson was driven from the chair of medicine at University College for endorsing and promoting the study of hypnotism. The early accounts of surgical operations conducted under hypnosis encountered extraordinarily irrational opposition. Dr. James Esdaile reported from India in the 1840s the successful completion of over a thousand operations (one-third of them major operations) with the patients hypnotized and a death rate of only 6 per cent during or after the operations. Although this occurred before asepsis when almost 30 per cent of other surgeons' patients died, Esdaile had great difficulty in getting his work even published, much less accepted. His scientific critics alleged that he had bribed his patients to sham insensibility. According to one account "it was because they were hardened impostors that they let their legs be

cut off and large tumors be cut out without showing any sign even of discomfort." In their opposition to hypnotism many of the most creative scientists of the period forgot the rules of their own calling. Lord Kelvin announced that "one-half of hypnotism is imposture and the rest bad observation."

Similar prejudices met Harvey's discovery of the circulation of the blood, Pasteur's work on microbes, and Semmelweis' discovery that physicians themselves spread the infection of puerperal or childbed fever from one mother to another. To the list of scientists who have suffered from the incredulity of their colleagues we can add Darwin, the several discoverers of anesthesia, and Freud.

Early in the nineteenth century a tragic example of resistance delayed the introduction of a life-saving medical treatment. An English physician, O'Shaughnessy, discovered evidence that patients with cholera died not of the infection directly, but of the depletion of salt and water carried off in the diarrhea. Another physician, Dr. Thomas Latta of Leith, boldly acted on these observations and snatched from the grave a number of patients desperately ill with cholera to whom he gave infusions of salt and water. He reported his almost miraculous success; a few other physicians tried and confirmed the value of his treatment; but still not enough interest could be aroused to promote the treatment further. Almost one hundred years later, twentieth-century physicians rediscovered it.

Contempt Prior to Examination

A common and astonishing feature of the opposition to scientific advance is the certainty with which it is offered. For the moment, and sometimes for years, the doubter forgets that he could be wrong. At the first demonstration of Edison's phonograph before the Paris Academy of Sciences, all the scientists present declared that it was impossible to reproduce the human voice by means of a metal disc. One man proposed to throttle the demonstrator. "Wretch!" said he. "Do you suppose that we are fools to be duped by a ventriloquist?"

Resistance to the new can reach into the highest places. We owe to Francis Bacon much of the foundation of scientific method. He said: "We have set it down as a law to ourselves that we have to examine things to the bottom; and not to receive upon credit or reject upon improbabilities, until these have passed a due examination." Yet Bacon could not believe that the Earth goes around the Sun. Galileo, who could not persuade fellow astronomers to look into his telescope, could not himself accept Kepler's evidence that the planets move in ellipses. Nor could he believe that witches suffered from mental illness, a view beginning to gain acceptance in his day.

Professor P. G. Tait, a contemporary and colleague of Lord Kelvin, made contributions to physics hardly less important than those of Kelvin. But when the news of the discovery of the telephone reached him, he said, "It is all humbug, for such a discovery is impossible." Another interesting conversation occurred between Sir William Hamilton and Sir George Airy, justly celebrated mathematicians of the nineteenth century. Hamilton had just published his discovery of quaternions and was explaining it to Airy. Airy said, "I cannot see it at all." Hamilton replied, "I have been investigating the matter for many months and I am certain of its truth." "Oh," rejoined Airy, "I have been thinking it over the last two or three minutes and there is nothing to it."

Many great ideas have, to be sure, won rather easy acceptance. Einstein had his difficulties, but they did not include stupid hostility from fellow scientists. Still such hostility should not occur at all among scientists. For it was science that once fought religion for freedom of inquiry and belief. In its original victories -- and some of its more recent ones too -- science defeated attempts to censor ideas. The principle of expanding knowledge replaced that of closed revelation. What had seemed to be a body of established facts was challenged and succeeded by a new body of facts based on observation rather than on reason and authority. But in the process, a confusion arose between science and that body of newly discovered facts.

As science has progressed, more and more facts have become established with reasonable certainty -- with enough certainty unfortunately to stimulate the illusion that science is chiefly a body of knowledge. The current body of scientific knowledge differs markedly from that of the seventeenth century, and the comparison shows the transience of our concepts. Yet we frequently overlook this and identify science with current knowledge. Those who forget that science is fundamentally a method and not a collection of facts will righteously challenge new concepts which seem to question old facts.

Organized scientific activity as we know it goes back less than five hundred years. And during this time it has occupied the interest and attention of only a few people. I am not referring to the millions it has affected, but to the few thinkers who have affected the millions. These people had first to struggle with themselves to believe that things could be other than they appeared to be. When someone asked Einstein how he came to discover relativity, he replied: "By challenging an axiom."

To accomplish anything worthwhile in science (and in nearly everything else), one has first to persuade oneself that things may be different from what they seem. This is the most difficult step to take and we should not be surprised if those who have walked furthest have frequently slipped. A scientist is -- perhaps fortunately -- only capable of scientific thought for a small portion of his time. At other times he usually allows his wishes, fears, and habits to shape his convictions. The wish not to believe can influence as strongly as the wish to believe. Most of us most of the time practice Paley's recipe for obstruction:

"There is a principle, proof against all argument, a bar against all progress . . . which if persisted in cannot but keep the mind in everlasting ignorance -- and that is, contempt prior to examination."

Scientists may also become seduced by their own attainments and acquire the conviction that success in one matter makes them authorities in all. James Clerk Maxwell's genius achieved an advance in the theory of electromagnetism from which came radio, television, and radar. His imagination shattered previously impenetrable theoretical barriers. Yet today he would surely blush crimson to read what he said to the British Association in 1879: "Atoms are the foundation stones of the material universe, unbroken and unworn. They continue to this day as they were created, perfect in number and measure and weight."

Pasteur struggled as much as any important scientist against the uninformed opposition of orthodoxy. After he attained recognition and at the height of his fame, he addressed a distinguished

group of scientists and gratuitously included in his speech an announcement that scientific methods would never be used successfully in the study of the emotions. Yet already living at the time of his speech were the two persons who later established the scientific study of the emotions -- Ivan Pavlov and Walter B. Cannon.

Like lesser human beings, scientists have a proprietary affection for their own contributions. Having given the best of their lives, as many have, to new observations and concepts, they may defend these as devotedly as those who give their lives to material possessions. And this kind of psychological investment can carry the investor into the most ridiculous positions. About fifty years ago, for instance, a curious exchange took place between the great anthropologist Malinowski and Dr. Ernest Jones, one of Freud's most devout followers and his biographer.

Jones subscribed wholeheartedly to Freud's statement about the universality of little boys' attachment to their mothers, which he called the Oedipus complex. This occurred often enough in nineteenth-century Vienna, and Freud declared it an invariable feature of human development. When Malinowski studied the Trobriand Islanders in the South Pacific he found that their children were reared by their mothers and uncles and had little or no contact with their biological fathers. The domestic relations and psychological development of the Trobrianders differed considerably from those reported by Freud for Viennese families.

Malinowski published his observations, but they did not convince Jones. From his office in London he insisted that Freud was right and urbanely reprimanded Malinowski for faulty observations. To this Malinowski patiently replied that he was compelled to accept the evidence of his own senses rather than statements made by those who had never visited the Trobriand Islands.

The tendency to erect "systems" -- which are then marketed as a whole -- affects particularly the less mature sciences of medicine and psychology. In these subjects we have had a succession of intellectual edifices originally made available only in their entirety. It is as if one cannot rent a room or even a suite in a new building, but must lease the whole or not enter. Starting with a substantial contribution to medicine the authors of such systems expand their theories to include ambitious explanations of matters far beyond the original validated observations. And after the first pioneer, later and usually lesser contributors to the system add further accretions of mingled fact and theory. Consequently systems of this kind -- like homeopathy, phrenology, psychoanalysis, and conditioned reflexology (the last dominant for years in Russia) -- eventually contain almost inextricable mixtures of sense and nonsense. They capture fervid adherents, and it may take a generation or several for those who preserve some objectivity to succeed in salvaging the best in them while discarding the dross.

Many such systems repeat the same story almost tediously. A few brilliant observations encounter fierce opposition from entrenched authorities. Despite this the new ideas slowly acquire adherents. Gradually opposition to much of the original propositions crumbles. But in the meantime the avant-garde of the enlightened have stiffened their doctrines into a sectarian orthodoxy. Instead of befriending further advances, they frequently attack and deride them. Certainly not all early adherents to a new discovery do this, but those who do not often find that loyalty to a group requires loyalty to a set of ideas which conflicts with dispassionate examination of later ideas and observations.

Harmful Incredulity

Rigid systems and their fanatical devotees have driven many scientists into the camp of the too incredulous. The querulous "schools" of psychiatry have by their own extravagance delayed the acceptance of the best in psychiatry by other physicians and laymen. However, physicians of all kinds are particularly guilty of failing to keep up with advances in their own specialty. This comes about because medicine is, to be frank, a trade as well as a science. Most medical students go into the practice of medicine, not research, and we all know worthy physicians who devotedly practice the medicine taught them twenty-five years ago, apparently uninfluenced by the events of intervening years. Yet these same men conscientiously trade in their old automobiles for new ones every two or three years.

Theoretically, physicians should have no more difficulty than, say, chemists or physicists in changing their habits to accommodate new advances. But to accomplish this, medical schools must change their principles in selecting students and try, first, to attract flexible minds into medicine, and, second, to avoid doing anything that will harden these minds against new ideas. Happily, medical educators have already recognized the need for this. When medical science moved slowly a man could write the same prescriptions for thirty or more years and still not fall far behind the times. The increasing pace of medical discovery has made such physicians not only foolish, but positively harmful. Whitehead's comment that "knowledge keeps like fish" applies to medicine as much as any subject.

However, research scientists, too, are bound by harmful incredulity, although it is harder to determine to exactly what extent. In some ways scientists today have more protection against uninformed authoritarian opposition than their predecessors. For one thing there are more scientists and they are constantly testing each other's work so that confirmation, revision, or rejection of new observations and concepts can come rather rapidly. Communications between scientists have improved, and many journals now spread new data and new theories quickly across the world. Thus many scientists and not merely a handful judge the work of a fellow scientist.

On the other hand, the vastness of our scientific activity tells us nothing about the number of genuinely open minds occupied with it. A few years ago, Dr. Lucien Warner surveyed a number of psychologists on extrasensory perception. He asked what they thought about the existence of extrasensory perception and how they had reached their conclusions. All who replied had convictions, but less than 20 per cent said they had studied the original reports of the work on this subject. Seventeen per cent had reached their opinions on the basis of hearsay. Twenty per cent had made up their minds entirely on a priori grounds.

One can only respect the candor of persons who have registered themselves as scientists and yet make public declaration of the fact that they can decide on a matter of extraordinary importance without examining the relevant published work. Perhaps parapsychology provides a special case and scientists do not feel so free to make up their minds on other matters. Certainly the implacable opposition parapsychology encounters among some scientists illustrates again the relationship between the heat of antagonism and the possible threat to established convictions from the new data

or ideas. For the data of parapsychology portend, I believe, a conceptual revolution which will make the Copernican revolution seem trivial in comparison.

We may tell ourselves that this incredulity has no effect on creative achievement but I personally do not believe it. I am convinced that deep conservatism strongly influences the approach of many scientists to new ideas. I have tested this frequently by throwing out into a group of them some new idea, especially one whose acceptance would fracture favorite concepts. Almost invariably they attack it like a school of piranhas. By the time it reaches the bottom of the discussion they have stripped off its flesh.

My friends are not ordinarily destructive people. They do not injure people, only ideas. And I think this behavior has to do with a mistaken concept of the role of scientist. Certainly the role includes skepticism and tough-mindedness, but these alone are not enough. The best part of science derives from the imagination and creativity which contribute to it no less than to the arts. A scientist should examine an idea as an artist might look at a delicately enameled vase-in many different lights and positions so as to bring out all its beauty and value.

Scientists frequently pride themselves on not being gullible. Sometimes they do not seem to realize that they cannot be incredulous about new ideas without at the same time being excessively credulous about old ones. Between the merits of accepting too much and not enough of what is new there is perhaps little to choose, but surely that little favors a receptivity to the new since we already know so little.

I believe our conservatism has infected the financial support of scientists. Although a lot of money flows toward scientific research we do not know how much runs in well-cut gorges and how much can irrigate new ground. But the system of project grants for research is a symptom. Nearly all the funds poured into research by foundations and the federal government reach scientists *after* they have submitted a project to a committee. Since a scientist must gain the approval of the committee for his project, he may not resist the temptation to design his project along the lines most likely to harmonize with the convictions of the committee. The committee in turn must account to a board of trustees or to Congress or the public for the success of the research it has supported. Who can blame the members if they behave like bankers and venture their money more readily on "good risks" than on "wild ideas"?

Once he has his money, the scientist feels committed to the project he has outlined. If he makes some interesting but unexpected discovery or observation, he cannot easily abandon his main object to pursue a new line. Nearly every year he must submit an account of progress to the committee. I have heard a number of scientists tell, half laughing, half crying, how they adjusted their applications or reports, or, worse still, adjusted their scientific projects, to the real or apparent expectations of a granting committee.

It matters little that often the scientist's fears are unjustified or exaggerated. Certainly most scientific members of committees evaluating projects consciously wish to give the working scientists the greatest possible freedom. Still possession of the power to make decisions can eventually persuade anyone that he also has the proper knowledge to do so. The fault, I think, lies

in the system, but wherever the fault, I believe that our scientists and the tellers of their money can easily become mutually involved in timid projects which always succeed but never advance.

It is difficult to pin down instances of the withering effect of incredulity on budding ideas. Prejudice can rationalize itself as caution or be easily disguised by other appearances. A surveying committee may conceal from the applicant, and even from itself, the real reasons for turning down a request for funds. Yet there are grounds for believing that research in psychiatry in this country has become excessively influenced by the theories of psychoanalysis. I know of two first-rate investigators who have had great difficulty in obtaining support for their projects because (so the available evidence strongly suggests) their ideas run counter to psychoanalytic concepts. One eminent psychiatrist, much experienced in such matters, told me in discussing one of these cases that it is now extremely difficult to obtain support for psychiatric research projects which are not psychoanalytical in orientation. (He was referring to psychological and psychotherapeutic projects, not biochemical or neurophysiological ones.)

Another leading American psychiatrist recently published a vigorous protest in one of our professional journals against the centralization of psychiatric research and its control by committees which permit a few persons to swing enormous funds toward a few favorite or fashionable themes of research. The one-sidedness of our approach evokes both horror and amusement in our European colleagues who have managed to preserve a better balance in planning psychiatric research. This state of affairs does not reflect adversely on the merits of psychoanalysis, only on those who insist that its assumptions must be the point of departure for all psychiatric research.

Freedom to Act Foolish

Defenders of grants for project research claim that they permit scientists to get funds long before they are sufficiently well known to receive support for themselves. This supposes that we can support scientists directly only when they have already become well known -- certainly a most unsatisfactory criterion of worth and one more likely to lead to a search for publicity than for truth.

A second symptom of harmful conservatism is the figures published by the *National Science Foundation* on the distribution of funds for research. In the years between 1940 and 1954 -- a period in which sums allocated for research skyrocketed -- the funds available for basic research (*i.e.*, research not bound to any immediate application) increased ten times. But in this same period the *percentage* of funds allocated for basic, as opposed to applied, research decreased by half.

Moreover, applied research has become increasingly important in the universities which have traditionally remained free to support new ideas and their testing. Recently, in order to maintain themselves against rising costs (or so they rationalize, perhaps), universities have accepted more and more contracts for applied research. According to a report prepared by Dr. Vannevar Bush in 1945, basic research received 70 per cent of all the funds devoted to research by universities before World War II. This contrasts sadly with a recent estimate derived from the report of the *National Science Foundation* that basic research now accounts for only 35 per cent of universities' research funds.

One remedy would be to give more money directly to scientists for themselves, rather than for special projects. The federal government has already begun this on a small scale, although we apparently lag far behind the Russians. Such a system would have its weaknesses in this country, as it undoubtedly has in Russia. Its mistakes would be more obvious and perhaps more wasteful than those of the present system. But if we had more failures, we might also have more new knowledge. Certainly we will have no new knowledge at all unless we continue to foster ideas which shake present beliefs. Prophets have warned us. John Dewey told us "every great advance of science has issued from a new audacity of imagination." And Whitehead wrote that "every great idea sounds like nonsense when first propounded."

During the planning of the Rockefeller Institute for Medical Research, someone asked Dr. Simon Flexner, who was one of the chief architects of modern medicine: "Are you going to allow your men to make fools of themselves at your Institute?" As it turned out the Rockefeller Institute made many more discoveries than fools, but the freedom to make a fool surely contributed to its extraordinary success. Scientists at the Rockefeller Institute were given full support to pursue their own work in their own ways. Unfortunately, this system had almost no imitators (except in Russia) and even the Rockefeller Institute departed in later years from its original principle. Today we badly need not only new institutes of the kind it was, but new freedom to pursue strange ideas. And scientists themselves must encourage each other to think brazenly and experiment boldly.

When I read about the now-primitive treatments practiced by our predecessors in medicine a hundred years ago, I cannot refrain from smiling at some of their fatuous remedies. My smile includes a little pity for them because they knew so little and some pleasure for us because we have come so far. Then I hope that a hundred years from now, some medical descendant will read our books with similar pleasure for similar reasons. If he does, this supposes that we in our time have remained humble about our knowledge and receptive to the new ideas which will furnish the justification for his pity. May it not be said of us: "No man having drunk old wine, straightway desireth new; for he saith 'The old is better'."