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SCEPSIS SCIENTIFICA*

BY

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The habit of mind of any scientist is sceptical in so far as he is, or should be, unwilling to admit the truth of anything without proof. The antithesis is a habit of faith in beliefs, usually expressed in abstract ideas, that make so strong an appeal to the individual as to require no proof; to him they seem self-evident. The habit of faith is one shared by scientists, who must accept teachings and beliefs that they have not personally inquired into. There is this difference, that the latter know they can obtain verification of the facts if they turn to the original experiments or calculations. If after that they feel sceptical they will try a rigidly identical experiment themselves or will devise new ones to test the results previously given. Belief in abstractions, on the other hand, can only refer for confirmation to others, contemporary and past, who attest them with equal or greater vividness and force.

The definition of the scientific outlook just given might be commented upon by philosophers. They might somewhat cynically say that in history it was observable that individual scientists had seemed to be readier to accept the truth of personal researches than that of others. They might observe also that the answer to an experiment is always a special answer since it is conditioned by the nature of the experiment itself, and that its applicability in a chain of reasoning depends on the choice of that particular experiment out of all possible experiments. They would almost certainly object that the word "truth" was misused, for science has nothing to do with truth in the sense in which scholars for centuries have employed the word—"correctness" or "accuracy" would be better. The amended definition would then run that a sceptical habit of mind is proper for scientists with reference to their own as well as to other people's work, that it embraces unwillingness to accept without adequate proof the correctness of a certain type of observation, usually with limited aim; and it might be added that this proof should preferably be quantitative, should take the form of measurement.

Essential Irrationality of Science

The philosopher would perhaps go on to add his own sceptical reflections on the limitations of the scientific method and of the orthodox scientific mind. He would find it difficult to express himself better than A. N. Whitehead or to add anything to what that philosopher wrote in his classic *Science and the Modern World.* He there speaks of the essential irrationality of science, by which he means its pursuit of crude and brutal fact and imperative acceptance of fact irrespective of its having any recognizable meaning.

This charge of irrationality is at first a little shocking, but it is valid For science takes a special pride in the necessity for such acceptance and is particularly suspicious of first causes and purpose. It is simpler when the results of experiments or biological observations are in line with orthodox beliefs; when they are not, the observer must still record them even when they puzzle him, are even more "irrational" than he expected. Within its own framework science presents mental difficulties, for clearly the scientific outlook requires a discipline of mind not quite natural or intuitive. There is a possibility then that the discipline may prove to be too rigorous. We can find two very different examples of suggestions for minimizing the risks of personal bias. The first is inherent in the Baconian notion of the value of the collective brains of a scientific committee. It was perhaps a Lord Chancellor's idea of science, as William Harvey, his doctor, might have said of it, recalling what he had said of Bacon's philosophy. An entirely different and individual concept, that of ridding the observer of all preconceptions and emotional content, was that of François Magendie 200 years later.

Magendie claimed that he went into the animal laboratory with a head empty of any expectation of what he would discover. He had two hands, two eyes, and no brain, or, rather, no mind. It would be tedious to explore the labyrinths of Magendie's illusions; he could not possibly have been as ingenuous as he thought he was, a kind of Parsifal transplanted, the wise fool of the laboratory. None the less it is quite certain that Magendie's ideal is theoretically necessary if the observer is to maintain the discipline that science demands. Unless extreme care is taken the experimenter or inductive reasoner may find himself falling into the errors which Olmsted finds in Brown-Séquard's later work. He no longer asked, says Olmsted, "How will Nature act under certain conditions?" but, "What conditions can I set up so that I can demonstrate to others the way I think that Nature will act?" The classical dilemma of science emerges: the necessity for an idea, a preconception, a theory which is to be put to experimental test, and the consequent danger that the manner in which the experiment is conducted may prescribe a wanted, an emotionally desired, result.

It is not without interest to seek from contemporary sources further amplification of the necessity seen at the beginning of the scientific era for three things—freedom to submit anything whatever to sceptical criticism, the danger of the scientist being deceived in his own experiments, and recognition that the scientific method could not expect to solve all the problems that might be put to test. Let us listen to some of those contemporaries.

Joseph Glanvil

Nearly 300 years ago there was published a book bearing the title of this article. It was by Joseph Glanvil, who died too young; it contained a reprint of an essay that later became famous—"The Vanity of Dogmatizing or Confidence in Opinions," originally printed in 1661, the year in which

*The substance of an inaugural lecture delivered to Leeds University Medical Faculty.

Newton entered Trinity College as an undergraduate. It was also the year of publication of Robert Boyle's *Sceptical Chymist,* the book that laid the foundations of modern chemistry. Glanvil desired, apart from the pleasure that he no doubt derived from a manner of writing so felicitous that it made him notable even in an exceptionally gifted century, to attack the belief that the old ways of thinking were the best. The investigation of nature started to flower with what now looks like astonishing abruptness in the sixteenth and seventeenth centuries. It found itself immediately in conflict with rationality, with a whole congress of scholastic erudition hammered out by thinking, by meditation, by logic, and by reason from suppositions not subjected and often not amenable to test. The perfection of the logic, the impeccability of the argument, raised a superstructure so imposing that the fallacy of the premises that were its foundation was not perceived. So great had been the success of these intellectual exercises that the answers to all questions were already known, or so it seemed.

On the other hand, the discoveries and propositions of Copernicus, Gilbert, Galileo, Kepler, Harvey, and Robert Boyle had been arrived at differently; what was more, Descartes 30 years before had announced in the *Discours de la Méthode,* that momentous charter for independence and certainty in thinking, the overthrow of mediaeval learning as an active force. Here was a new age dawning, created by a new method. The new method was science, the object of which was to examine data and to report on them without prejudice and particularly without prejudice emanating from ancient and dogmatic teaching; for, as to that, "What hath it been," said Glanvil, "but a pretty toy in an Heiroglyphick; a very slender something in a Fable; or an old nothing in a disputation."

The battle in which Glanvil was engaged was won so decisively by science that in our own times the teachings of the mediaeval thinkers have become more the property and happy hunting-ground for the scholar, the humanist, or the epistemologist than for the scientist. None the less the educational background of the scientist was furnished by the universities and monasteries, which in late mediaeval times were the abodes of men of quite exceptional intellect. That they were engaged in studies which later times rejected should not obscure the fact of their quality. That quality was there to be deflected little by little, a process fortunately not to this day completed, into the new paths of science. Nearly all the greatest scientists gave some acknowledgment that the wonders of the natural world which they had done something to unfold increased their reverence for God. These scientists therein showed their deeply rooted unity with the body of thought from which they sprang, had corrected but not destroyed.

Francis Bacon

In our own generation the average scientist is not permitted to do more than thank, not God, but his parents for the happy consortium of chromosomes with which they have unwittingly endowed him, so great has become the gap between us and that monastic thinking in which scepticism was barely permitted. But was the mind the supremely reliable tool that scholars and savants had believed it to be? In England, Glanvil, like Descartes, had been antedated by Francis Bacon. His *Novum Organum,* outlining further the ingenious but impossible method for the advancement of learning already mentioned, had advocated the cutting of strings that bound his age too firmly to the past. He proclaimed his doubts on the speculative and reasoning powers of the human brain, belief in which had been the discovery and joy of the Greeks. Brilliant indeed had been their use of the new weapon, but it was clear that speculation could lead to absurdity.

Bacon mistrusted the powers of the mind to such a degree that in the *Novum Organum* he exclaimed, "And so we must add not wings but weights and leads to the intellect so as to hinder all leaping and flying." The wit of man, he said, when it worked on facts, on matter, was limited by its material and could indulge in no great excess. But when it came to pure thought—that was where the danger lay: "If it work upon itself as the spider worketh his web, then it is endless, and brings forth indeed cobwebs of learning, admirable for the fineness of thread and work but of no substance or profit" (Preface to *Advancement of Learning*).

Bacon gave further examples of the mind's weaknesses—it presented mixed qualities: "Facility to believe, impatience to doubt, temerity to answer, glory to know, doubt to contradict, end to gain, sloth to search . . . these and the like have been the things which have forbidden the happy match between the mind of man and the nature of things, and in place thereof have married it to vain notions and blind experiments" (*Praise of Knowledge*). Wilfred Trotter has, within our lifetimes, re-expounded the Baconian scepticism in language more suited to our present-day ears, with a compact brilliance and inventiveness that at least equals the original. Trotter's proposal that minds needed calibration to take account of inherent and acquired bias was as engaging as impossible. Nevertheless, the idea of necessary calibration remains a useful short statement that we need at all times to remember. Trotter's deduction that ideas which aroused emotion were the most dangerous because the most violently defended was extremely important.

Glanvil's period denied him the concept of emotionalism—it would have been called passion; but he put forward a very similar idea: "Do what we can," he said, "Prejudices will creep in and hinder our Intellectual Perfection: And although by this means we may get some comfortable allay to our distempers, yet can it not perfectly cure us of a disease that sticks as close to us as our natures." In many respects this was no more than an old theological doctrine, one befitting the rector of the Abbey Church at Bath, to whom all man's imperfections could be explained by the Temptation and Fall. Bacon subscribed to the same belief. This made some frailties inevitable, inescapable, but man's tendency to err about everything was not hopeless if he would stick to facts. Science was useful and as exemplified by the new Royal Society, of which Glanvil was a Fellow, might do much to correct some of man's mistakes.

World of Ideas

It is interesting to observe how quickly all philosophers came face to face with the problem of mind and inevitably with that of brain. Berkeley, with his proposition that nothing existed save as an idea in the mind, is a ready, if the extreme, example. His superb sentences carried almost complete conviction: "Some truths there are so near and obvious to the mind that a man need only open his eyes to see them. Such I take this important one to be—viz., that all the choir of Heaven and furniture of the earth, in a word, all those bodies that compose the mighty frame of the world, have not any subsistence without a mind; that their *being* is to be perceived or known." It seemed almost an anticlimax when David Hume dismissed Berkeley's statements with the laconic remark that they "admit of no answer and produce no conviction." The great Dr. Johnson misunderstood Berkeley when he kicked his foot violently against a stone and said, "I refute him thus!" for the Bishop would have retorted that the solidity encountered and the pain felt were still no other than ideas in the mind.

Attractive though the ideal theory was, man's instinctive belief in the permanence and reality of a world which tangibly and visibly exists whether he is there to see it or not has overtopped the doctrine of pure ideas. We believe that things still exist even in our absence because experience has taught us that this is so, even though the strictest philosophy must forbid us to assume it. A modern minatory example of the possible deception by the senses is given by astronomers, who tell us that the light that reaches us from many stars has been so long on the way that the star from which it comes may now be dead or destroyed. That has not yet been proved by example, and even if it were our people, rightly empirical, are content to wait until such an event affects their lives before they adjust their thinking to consequences from the distant empyrean.

Alexander Pope was more in touch with the first principles when he defined the proper study of mankind. For such close-range corrections within the reach of everybody are easily made. We recognize that the world may look the same or a little different to other people; we know from the cell layers in the retina and from the presence or not of a chiasma that it must look different, sometimes very different, to other animals, both in colour and in perspective. We admit that our senses may mislead us, that what in the distance we thought was a man is a bush, and so forth; but we have spent our lives from our cradles in correcting these errors and have developed a built-in theory of probability that minimizes mistakes. Man only rarely raises his eyes to the firmament, and when he does it with scientific intent he finds mathematics there. He can conclude that the Universe was constructed by a Supreme Mathematician, one who had pre-knowledge of the discoveries of Newton, Einstein, Planck, and Rutherford and then hid them in nature to give reason for a terrific university game of hunt-the-slipper. But we can be sure, with Eddington, that the mathematics was not there until man put it there.

Kant in one of his more intelligible passages rightly held that space and time are not inherent in the objects of our knowledge but are elements in the knowledge itself—i.e., we put them there by our methods of observation. None the less, it has to be admitted that there is something to measure and that mathematical accounts are almost the only ones so far available. Man looks at the Universe, and if the Universe looks back at him he does not know, nor, except in rare moments, does he greatly care. There have been those so impressed by the vastness of space that man has seemed to them an unimportant object. It cannot be shown that this deduction has had any recognizable effect on those thinkers' manner of living or their relation with their fellow men. It is a view to point an argument or to be subject matter for a reverie. Man remains the most important subject in the world; his own nature and that of the furniture of the earth provide him with urgent problems enough.

The Brain and the Mind

Philosophy has agreed with the doctors that whatever mankind studies he has only one tool to use, his mind. It is the instrument of progress. We have seen how fallible its qualities, especially when attempting to make a right use of knowledge, have been held to be, and that not only recently but for centuries. It would be easy by presenting passages from Descartes' and Hume's writings to show two great thinkers struggling with this problem, the use of knowledge, and after long meditation concluding that the only way in which they could advance was by rejecting everything that they had ever learned and beginning anew. This they tried, and it was not the solution they required. Prejudices, emotions, illogical successions of reasoning would creep in.

Could anything be learned from the structure and workings of the brain? To this old question we have no modern answer. It had early been a matter for wonder that a cold, grey, soft mass such as the brain could be the seat of those powerful mental qualities that can so delight and inform us—and so deceive us. In its gross morphology the brain is not difficult to understand, but it yielded grudging fruit to the early experimenters. It has required the two centuries after 1740 to formulate the nervous impulse from the mediaeval concept of the animal spirits. And except that we admit now that we have no reason to believe that mental activities are carried out by processes very different from the impulses in the peripheral nerves and spinal cord, we still do not know how they produce the aggregate of mental processes that we call mind. That was Sherrington's conclusion. In that failure we greatly disappoint the philosophers, who imagine that we know more than we do, and, what is worse, build on that belief.

[*Passages on growth of knowledge of nervous impulse have been omitted.*]

The advance of basic knowledge of the brain's structure and its working could be made to apppear as a swiftly moving panorama of events that were in reality long and laborious in the enacting. It could be made to read like a smooth tale of success, a scientific rise from log cabin to White House. We know that what really happened is what happens now, that one man here possesses a piece of information, a man there another. It may be a long or a short while before it is realized that they fit together to make something else. But on the matter of mind it may seem strange that the great philosophers who have communed and speculated about man, his meaning and his purpose, should have known so little of the intimate mechanisms of integration in the brain.

The great innovators in psychology—Freud, Jung, and Adler—have shown no more interest in structure than Plato. It is actually of less moment than might be imagined; for the definition of correct method, of what we can hope to know, how we know it, what are our lets and hindrance, and what is beyond knowing, requires next to no neurological knowledge. It was no detriment, for example, to David Hume, who, supposedly the arch sceptic, comes nearest to the scientist's ideal of a philosopher in his axiom that given a cause we cannot foretell what the result will be unless we have previous experience of that cause acting in rigidly identical circumstances. This is pure science. The difficulty is that causes do not arise in isolation but, too often for our composure, derive from events further back again. Generalized, Hume's doctrine is expressed by saying that we know nothing except by experience—again the basis of pure science. Distrust of dogma, of far-reaching planning based on a concept, is a characteristic of British political and philosophical thinking, as Bertrand Russell has recently so brilliantly demonstrated

Application of the Scientific Method

What is the use, we say, of a logician's theory neatly pursued when experience has taught us that so many adventitious variables will force their way in. Better far not to attempt to see too far ahead; better to correct the bias as each variable appears. That, too, is the method of science. Are we to infer that speculation is never permissible in science? By no means. The difficulty is to get the dosage right. Science, at conceptional level, is as speculative as art; every good piece of research begins as an idea coming unsought into a mind. "All the thinking in the world," said Goethe, commenting on someone's remark that thinking is so difficult, "does not bring us to thought; we must be right by nature, so that good thoughts may come before us like free children of God, and cry, 'Here we are!'" We know not how this is, but in its highest form the inspiration is the same as that which visits poets and artists. The difference lies in the method applied to the idea.

Science is more fertile than art in suggesting trains of thought with one thing arising out of another. It does not do this smoothly without pause nor in a straight line, for not only may it seem more profitable and enlightening to swerve into a side-chain but something may be discovered that casts doubts not so much on the main truth of the argument as on its being the whole truth and sends us back to examine the beginnings anew. This happened, of course, classically with Newton, and it is unlikely that Einstein's correction will stand permanently unaltered. Hence all scientists must harbour, as most of them do, a grain of scepticism in their composition. To be too great a sceptic is not a sign of greatness, for it is easier and less laborious to doubt than to discover truth. Reflection suggests that the only sciences which have succeeded in producing immediate conviction and durable results are mathematics and those into which mathematics enters to a very great degree. The biological sciences are more difficult because of their incalculable variables. But we advance none the less, step by step.

When we look back we can be impressed by the ignorance that was our forefathers'. The mathematicians of Elizabethan times were totally ignorant of the quantum theory, of nuclear physics, of wave mechanics. That is scarcely 400 years ago. What will they say of us 400 years from now! We are aware of vague discomforts in our minds about so many things. So must they have been. Until we can get our uneasinesses to the point of crisp formulation they admit of no answer, for we cannot investigate them. Let us not therefore laugh too loud at our fathers lest posterity overhears us! Their ignorances were of things that they could not have known; they lacked the interlocking discoveries and precision instruments that little by little advance learning. This is implicit in all that I have said of the nervous impulse and the cellular nature of the nervous system and the body. It is true of chemistry, physics, and astronomy.

It was nearly 300 years before instruments of sufficient delicacy were available to confirm the parallax of the fixed stars which the Copernican theory of the earth's motion demanded. It would be absurd to imagine that progress in instrumentation is a process which has now ceased, that further improvements will not disclose our present ignorances. We can be very sure that there are a dozen things under our noses which we misconceive or do not even see. The greatest source of error lies in our having explanations that satisfy us or with which we make do because we can see no other. That was where our forerunners came to grief, though I deprecate such a manner of expressing it. It is unquestionably where we ourselves shall be found to have erred. Our greatest ignorances must by definition be those of which we are unaware. Wilfred Trotter speaks of our natural resistance to new truths, our battle to throw them off at once because they irritate like an acid or sting. That seems to be particularly so when we are satisfied with what we already know of the subject. When our minds are vacant of explanation we seem to be extraordinarily gullible, ready to accept any theory, however nonsensical.

The Emotions in Science

Whatever means science theoretically should use, the scientist is a man more imaginative than Bacon would allow or Magendie would admit. He does require a presupposition which it is his intention to investigate. He is in fact a good deal more rational, more emotional, in a word more human, than argument can hold him to be. Hence his scepticism must be wilful. There have been important scientists who have appeared to maintain a scrupulous exactness in their researches but who have displayed emotion, bias, prejudice, and temper in their pronouncements on other subjects, notably on politics. It is difficult to believe that none of these qualities are to be found in their scientific work. Everything that a scientist does must have a personal flavour and be subject to those vagaries which personality implies. The intellectual cold purity of scientific work, something that is like a flame without heat, is a supposition without possibility of realization. Orderly scepticism is a discipline, not an intuitive possession; the proper name for the intuitive variety is prejudice.

There are other large fields of knowledge besides science: there is knowledge to be gained from literature, history; there are the arts; there is philosophy that embraces all kinds of knowledge. In these others the emotional side of men's nature is permitted a freer rein, and in the arts its fullest development is positively demanded. Men cannot lead contented lives unless they store their minds with goods bearing different kinds of trade-mark. There are many who believe that scientific certainty is not the only kind, who would agree with Descartes that they could recognize some ideas as so clear and so distinct that they brought instantaneous conviction and were immediately acceptable as truths. But unless they can be demonstrated, can be shown to be indestructible on attack, they must remain truths only for the individual who holds them. This seems a depressing conclusion, however well it may explain man's permanent liability to disagree with his fellows.

Conclusion

However, it is, as Glanvil would have said, "more perpendicular to our discourse" to conclude that our task in keeping emotions in control in science is difficult, since they are so permissible in much else that occupies our thoughts, colour our lives, and are at all times ineradicable. The rules that we live by have been made by experience as curbs on unfettered emotional behaviours. The rules of science have a shorter history, but are in the main of the same kind narrowed by a sharper focus to a different end. We have seen that better knowledge of the brain gives us no hope for lenses that will automatically correct the astigmatism of our minds. Let us then live our lives according to rules of historical experience, and in our scientific thinking let them be tempered, but with our actions not paralysed, by scepticism.

THE BACTERICIDAL ACTION OF STREPTOMYCIN

BY

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Streptomycin, an antibiotic derived from *Actinomyces griseus*, and discovered by Schatz, Bugie, and Waksman (1944), owes its therapeutic value to action on bacteria which are insensitive to penicillin, notably *Myco. tuberculosis* and many species of Gram-negative bacilli. The original description credits it with "strong bactericidal properties" without giving evidence for this statement. Several subsequent authors, including Hegarty, Thiele, and Verwey (1945), Hamre, Rake, and Donovick (1946), Strauss (1947), and Smith and Waksman (1947), have shown that low concentrations added to susceptible bacteria in a nutrient medium cause a slow fall in the viable count; it has also been observed that in a non-nutrient medium there is no such effect unless a considerably greater concentration is used. The only published example of a test employing a high concentration in a nutrient medium is an experiment by Helmholz (1945), who inoculated the urine of a patient under treatment with streptomycin and containing 1,330 units (micrograms) per ml. with various bacteria, and found that they were all killed within one hour. Since concentrations of this order can easily be attained in the urine, and local treatment can produce similar conditions elsewhere, it is clearly of interest to know more about their effect on bacteria.

In so far as the action of a chemotherapeutic agent is bactericidal, that action must be influenced by the various factors such as concentration, temperature, medium, and