

Hindrances to the progress of research

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The fundamental ideal of science

We live at a time when the most fundamental ideal of scientific enquiry is being called into question, and indeed explicitly rejected. This cannot fail to have a profound effect on research in all fields, not only the medical. But the effect of the dominant ideological climate may be particularly distinct in relation to medicine since this concerns the nature of human beings and the extent of their dependence on the society around them; these are matters that carry a particularly strong emotional loading from the viewpoint of the prevailing orthodoxy, and this loading sets up stringent requirements for what shall and shall not be done.

But first let us consider what may be the fundamental ideal of science. The Duke of Kent, in his 1981 presidential address to the British Society for the Advancement of Science, asserted, 'I say without any equivocation that I consider it the scientist's first and imperative duty to expand the boundaries of knowledge.' Similarly, Hans Eysenck stated, 'Personally, I would take my stand with Thomas Jefferson: "There is no truth existing which I fear, or would wish unknown to the whole world".'¹

Both these assertions were made in the awareness of, and in explicit opposition to, a climate of opinion in which they are no longer widely accepted. It is old-fashioned and naive to talk of an external truth or reality toward an understanding of which the human race is advancing by successive approximations. There is no criterion of reality other than social agreement. Reverence is due only to what is socially desirable.

A central, maybe the central, determinant of contemporary attitudes in all fields of intellectual activity is the modern drive toward eliminating any sense of tension between socially agreed-upon opinion and external reality. The tension is removed by denial, more or less explicit, that there is any such thing as external reality or that it has any right to numinous status if there is. Why does it matter what is true? What is important is what is good. There is even a school of thought in the modern philosophy of science that teaches explicitly that it is impossible to arrive at objectivity in scientific observations; all observations are made in the context of the received ideology of their time and cannot be separated from it.

So we find ourselves in a situation in which one ideal of science is being, with increasing explicitness, replaced by another. The old-fashioned ideal conceived of science as striving to establish the truth, whatever it might turn out to be, whether at variance or not with what human beings would expect or prefer it to be. The new ideal conceives of science as subservient to the requirements of social desirability. This view of the matter depends on the idea that the outcomes of research can be foreseen, the social consequences of it predicted, and a definite opinion formed whether these consequences are desirable.

The disinterested pursuit of knowledge

In fact, immensely useful, practical consequences have often arisen from the disinterested pursuit of knowledge. The Curies studied radium without foreseeing its medical applications; Sir Alexander Fleming discovered the antibiotic properties of penicillin by chance. It is impossible for the consequences of an increment in human knowledge to be accurately foreseen, even by those most directly concerned with it. Twenty years before the first use of atomic power, Einstein and Rutherford expressed their opinion that no practical harnessing of atomic power would ever be possible.

In general, it is certainly possible to argue that the ostensible modern goal of beneficial effects on society as a whole is more likely to be achieved, and to be achieved more effectively, by an adherence to the old-fashioned principles that knowledge is good in itself and that the extension and dissemination of knowledge of all facts without distinction is intrinsically desirable. Nevertheless, it is a somewhat weak position to defend a principle by demonstrating that it may be defended in terms of another principle, as if admitting that the latter is the really important one, and the former can only be justified in terms of it. As Eysenck observes, 'According to the scientific ethos, scientists should fearlessly speak the truth; in theory, truth is the supreme god to whom the scientist bows. The position now is departing rather rapidly from this belief'.² He quotes Carl Sagan as saying, 'In a time of trouble, the tendency of society is to constrict the range of accepted ideas. But just the opposite – diversity, heresy – is what is needed if problems are to be solved.'

The qualification ‘in a time of trouble’ is unnecessary. Any society has a strong tendency to foster and favour only activities and intellectual productions that support the received ideology of the time, and the notion that individualistic heretics are good for anything is never likely to be applied with much energy. In a society in which the financing of research is largely, indeed almost exclusively, undertaken by the state or by collective entities which are answerable to the prevailing orthodoxy, there will be little opportunity for heresy to take effect.

Practical pressure and moral pressure

The modern ideology produces two kinds of pressure, one practical and one moral. The practical one is that there is a constant transfer of freedom of action (or financial power) from individuals to the state. Individuals are heavily taxed, and their ability to pass on by inheritance even such accumulations as they are able to build up in a heavily taxed lifetime is itself subject to heavy taxation. In addition, there is taxation by inflation, and state control over the supply and value of money held in the hands of individuals. This is confiscation as surely as would be open levies on the assets every citizen, but its effects are indirect. The cost makes it less and less likely that any individual or group of individuals can carry out independent research on an adequate scale; the freedom to set up research establishments and to do independent work has thus effectively been confiscated and transferred to the state.

The moral pressure of the modern ideology is simply towards doing what reinforces it. The fact that the beliefs that actually make up the modern ideology are largely implicit, although all-pervasive, makes it more, and not less, dangerous. You will gain social reward and approval by doing research that has results other people will approve of; you will not gain it by doing research that calls into question some important, even if implicit, belief. Even if this were not supported by the financial censorship already described, it would be a powerful force.

These pressures are at work within medicine. The fact that, on a certain level, much can be achieved by the application of well-established medical knowledge in relatively underdeveloped parts of the world may help to distract attention from areas of neglect in more innovative fields of research. Much that is obviously useful can be achieved by applying to

very large populations simple pieces of knowledge resulting from what was once pioneering research. Because of this, it is easy to lose sight of the fact that it is new advances in knowledge, the significance of which cannot be assessed in advance, that may have the greatest effect on the potentialities open to the human race.

How ‘useful’ is research?

Actually, the consequences of the present trends appear to be somewhat different from what is usually supposed. A very small fraction of research work done in universities is ‘useful’ in any sense, and the standards of it are quite possibly declining, for two reasons. One of these is that what serves to advance a scientist’s career is the number of papers he has published, and scientists are thus under pressure to maximize this number with little regard for their content or quality; and the second is that papers that produce socially acceptable results are likely to meet with more social reward than those that do not, regardless of their technical qualities as pieces of work.

The expectation that things will be done well and effectively if they are done by large numbers of people acting together with a minimum of independence depends on somewhat uncynical assumptions about human motivation. If people are put into positions of social authority, their motivation is unquestionable; they are there to do good. If people are socially authorized scientists they are there to do science, and they are supposed to be additive: several scientists will do more science than one scientist. A statistician once remarked to me, attempting to reconcile me to the tedium of discussing a piece of work with a committee, ‘Discussion is always a good thing. Many heads are sure to be better than one.’

In fact, the state may be disposing of colossal funds and resources for research, and deploying millions of people, but it does not follow that what is being done is necessarily advancing knowledge at a greater rate than would be achieved by even a small number of individuals who had some peculiarity of motivation that made them wish to find things out, and who also happened to dispose of financial resources that, while infinitesimal compared with the totality of those wielded by the state, were still large in relation to the capital which it is at all easy for a single individual to acquire in modern circumstances. Nor does it follow that a committee

consisting of a dozen people with an average IQ of 150 will wield an effective IQ of 1800.

What, in fact, are the motives of professional, state-supported scientists and members of directing committees likely to be, and are such people likely to interact constructively or destructively? It is an easy guess that they will be predominantly interested in their own social advancement; they will want to make decisions that will impress other people as the right kind of decisions, and they will want to do or see done the kind of research that other people will reward with higher degrees and similar marks of social favor. If young scientists are too strongly motivated in any other way – by intellectual curiosity, say, or by a desire to seek out fundamental paradoxes in the nature of things – they may well find themselves unable to stay the educational course that leads to life as a socially accredited and salaried research worker.

Some years ago a course of lectures on scientific research was given in Oxford, intended to provide information and preparation for those who might be considering proceeding to do research in the form of a higher degree. As reported to me at the time, the general tenor of these lectures was as follows: ‘Young people have an idea that when they start doing scientific research they will be breaking new ground and dealing with issues of burning interest. This is not so; they have to realize that research is not like this. What people do in the course of working for a D.Phil. is of practically no interest to anybody. The average number of people who read a scientific doctoral thesis, other than the author’s relatives and supervisor, is estimated to be 1.8.’

But even if the greater part of modern research really is uninteresting, in every possible sense, a very great deal of it is being done. As already mentioned, what advances someone’s career in social terms is the production of papers. Broad and Wade have observed, ‘The preoccupation with publications has resulted in a veritable ocean of journals and papers. Today, there are at least 6,000 journals in medicine alone. An additional reason for the number of journals is the tremendous increase in the ranks of scientists themselves. It has been estimated that 90 percent of all scientists who ever lived are alive today.’³

Estimates have been made of what fraction of the research being done is useful, at least in the sense that it is referred to in papers by other scientists. This is not a very high standard of usefulness, and, of course, work that is of poor quality but is ideologically attractive may well be cited frequently; it, then, will qualify as contributing to progress on this criterion. However, even estimates of this kind show that only a tiny fraction of the research papers produced have any influence on the work of other scientists and can thus be regarded as contributing to progress. According to Broad and Wade, ‘The available evidence indicates that the great majority of research responsible for the advances of science is produced by a small number of scientists. This small elite depends overwhelmingly on the research of other members of the elite, not on that of the wider majority. The pace of scientific advance would not obviously be slowed if this majority did not exist. It might even be enhanced if pursued by a leaner and fitter community of researchers. Perhaps there are too many scientists. Perhaps basic scientific research would be more appropriately supported by private patrons, as economist Milton Friedman has suggested, instead of by the government’.⁴

What is being sacrificed?

One line of defence that might well be adopted by a proponent of the modern orthodoxy would be to inquire earnestly what scientific or medical research one thought was being neglected, and to require a statement of exactly what beneficial developments might be forthcoming if things were done differently. But, it is essentially the case that what is being neglected is invisible; all that can be done is to point out the presence of a very strong ideology in a position of dominance. From the requirements of the ideology one can, perhaps, indicate certain areas in which it is unlikely that research of a progressive nature will be done, but it is possible only to adumbrate vaguely some of the potentialities that might begin to emerge if it were.

The modern ideology is certainly operative within medicine – including particularly strong ideas on the nature of human beings and in what relationship they should be to society, and these ideas undoubtedly have their effect on the way medicine regards those to whom it is ministering.

It may appear that little is lost by the non-pursuit of research in some of the neglected areas; the findings, if any, could surely not be of great fundamental significance. But it is characteristic of research that one cannot be sure how interesting or significant the findings may be until one has made them, and any ideological restraint upon the extension of knowledge is a serious matter.

In conclusion, let me point out another consequence of a dominant orthodoxy which may also be overlooked. It is that it inhibits research even if the orthodox opinion is actually correct. Only research that may be expected to support it in the crudest and most obvious way is likely to be encouraged; areas that could lead to heresy must be ignored. Now it might sometimes be that research in 'heretical' areas leads to an expansion of knowledge and that once it is obtained, it is observed to be compatible with the desired view of the matter after all. But, in general, there is little tendency for researchers to risk being placed under pressure to refine or develop the ideas accepted as correct, and areas of weakness, incoherence, or paradox are passed over in a discreet silence, rather than regarded as promising fields for enlightening investigation.

References

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2. *ibid.*
3. William Broad and Nicholas Wade, *Betrayers of the Truth* (New York: Simon and Schuster, 1982), p.53.
4. *ibid.*, pp.222-223.

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