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Are scientists really stifling science?

SCIENTISTS are stifling science; Professor John Taylor of King's College, London put this proposition to an audience at the Royal Institution in a televised "Controversy" programme this week. Within the necessary confines of a televised debate it is all too easy to judge the matter won or lost on style rather than content; Professor Taylor raises an important issue and it merits careful consideration within the scientific communty.

There is, of course, a very specific circumstance surrounding Professor Taylor's unease. For the past two years he has been attempting to come to terms with a variety of phenomena that are usually put into the paranormal category. His motive has, he insists, consistently been to explain these phenomena by means of conventional scientific concepts. History is on his side in this respect; many phenomena which in the past were regarded as bizarre or supernatural have yielded to scientific analysis. History, as his critics continue to urge (see Professor Hammerton's letter on page 640), is not on his side in offering too much encouragement that professional scientists can outsmart professional deceivers, if deceivers some be.

The issue at hand, however, goes far beyond metal bending. Scientists are in an unusually favoured position in policing their profession, and so they have a special responsibility to keep these policing mechanisms up-to-date, fair and seen-to-be-fair. Governments devote vast sums of money annually to the practice of science, and even when the science is obviously directed at solving pressing problems, the scientist is still the ultimate arbiter of what he does and doesn't do. Indeed through a variety of mechanisms from refereeing for journals to sitting on grant-giving bodies, scientists are arbiters of what others are allowed to publicise and spend money on. Do scientists warrant the trust that the public, unwittingly, puts in them to keep their house in order and forward-looking?

The greatest guarantee that the uncommon or unusual idea will get some sort of hearing is pluralism. If the sources of money are not controlled by just one committee, if the means of publication are not all in the hands of one person, if education is not according to some unified syllabus, then the prospect of any one group of people being able to exert an unhealthy influence is vastly diminished. And those who cannot get satisfaction through normal channels can often be remarkably effective in unusual ways. Research does not have to have been sponsored by the Science Research Council to be distinguished, nor does it have to be published in *Nature* to command wide attention. Indeed some of those who feel they are unjustly ignored might be surprised how well known and extensively thought about their work is; simply because no-one relishes the thought of being known to posterity as the person who rejected a seminal idea. Likewise, as Professor Ball's letter (page 640) shows, experimental help does not necessarily come from a grant-giving body.

No doubt all of this sounds like smug self-satisfaction; the real test for the openness of the system, it will be said, is bound to be in the case histories rather than in assertions of fairness. The problem with case histories, however, is that conclusions have to be drawn from limited evidence and that there is often considerable difficulty in understanding the spirit of the time. Wegener's advocacy of continental drift in the early years of this century is often quoted as an example of an immensely important idea on which the establishment turned its back, thereby retarding the earth sciences by fifty years. But Wegener's ideas were well known and widely discussed; continental drift was the subject of at least one conference in the 1920s. Causes of drift were discussed extensively in the literature. What was missing was the compelling evidence that Wegener could not provide and that only an expenditure of large sums of money on marine research in the 1960s could. That example should warn us that certain fields can fail to flourish, even when confronted with an apparently blinding insight.

It should also warn that the case history is not an easy way of demonstrating inbuilt bias amongst scientists unless it is possible fully to appreciate the environment of facts, observations and speculations within which they lived at that time.

Science is ultimately about ideas moving around inside people. It does not reside in textbooks or in large pieces of equipment. Although it is possible for individuals to deny to other individuals the access to hardware or the literature through one particular channel, it is possible for no-one to control all channels, and it is certainly impossible to suppress ideas and people. This may be less true in other societies or in other professions, but surely the Western scientific community has had a relatively good record, and it is time that we stood up and said so. □