Science policy: three recent idols, and a goddess

W.J.M. Levelt
Max-Planck-Institute for Psycholinguistics, Nijmegen

Science and science policy

Human perceptual systems accomplish feats of veridicality. To be sure, there are occasional illusions and misperceptions, carefully registered and analysed in research institutes such as IPO, but the astonishing fact is that, under a wide range of conditions, our senses yield truthful representations of objects and events in an ever-changing environment. There is immediate categorisation of what is fixed, constant, permanent, essential, and this knowledge provides veridical guidance for often equally immediate decisions and actions. This, with language, is one of evolution's most precious gifts to mankind.

But evolution has been less generous to the mind's eye, in particular as regards our ability to discern scientific truths immediately in the ever-changing flood of potentially relevant data. In fact, that ability is nonexistent. Approaching truth in science is a slow and highly unpredictable process, one which is beset by illusions and misconceptions. Only time is a filter comparable to the senses: After the fads, the rhetoric, the personal and public interests have died away, it becomes increasingly clear what progress has been made in terms of lasting contributions to science. Twenty-five years is a relatively narrow band-width for this filter. Still, when it is applied to IPO, one detects the contours of some major theoretical developments which are generally viewed as 'classics' (Some of these highlights are discussed in '25 jaar IPO', Eindhoven: IPO, 1982).

The disquieting (for some) but instructive aspect of this is that none of these developments could have been predicted 25 years ago. The mind's eye is completely blind to future knowledge. If one did, moreover, try to trace back how a particular scientific insight came into existence, one would find a bewildering gamma of idiosyncratic accidentals, none of which in itself could have been known <u>in advance</u> to play a crucial role in the process.

This makes research management a difficult, or -depending on one's perspective- an easy job: One cannot do much about steering the process. There are no tested and proved strategies. No planning, however intelligent, can guarantee success.

This state of affairs, though nothing new in science, is becoming increasingly disquieting to science policy makers. Public pressure is building up to 'exert control' over the advancement of knowledge, to make the process less erratic and more predictable. The worst, and most ridiculous version of such 'control' is to assign scientists the task of proving a pre-established scientific 'truth', such as a racist, Marxist, or feminist ideology. Leaving such excesses out of consideration, however, one can also detect more subtle ways in which governments and funding agencies try to influence the course and the degree of success of scientific discovery. There can be no doubt that some of these efforts are exercised with the best of intentions: The promotion of science is still widely recognised as a maxim of our culture. Still, the form such promotion takes is at times impractical, at other times based on false assumptions. Some of these assumptions acquire the status of idols, worshipped equally by science policy makers, administrators and scientists. But at least the latter should speak the truth, even when this leads to disharmony with public opinion or to jeopardising the flow of funds. Moreover, it may well be that the worshipping of idols will in the long run boomerang and harm science itself. In the following I want to mention three idols whose worshipping I would not recommend to scientists.

Three idols

The research proposal idol

Only research which is cast in a neat research proposal, outlining theory, methods, expected findings, and timing, can be expected to yield significant results. There is no doubt that the writing of research proposals can perform useful functions in the promotion of science. It forces the scientist to relate his or her ideas to whatever is around in the literature, and it gives the indispensible scientific forum a chance to interfere even during the conception of a research project. Also, it makes the scientist 'funds-conscious': In the best case, he or she will consider whether the expected scientific gain is reasonable related to the financial requirements of the project. All this I grant; still, the general claim is patently false. Its falsity follows from the above-mentioned in-principle unpredictability of future knowledge. If one always required a scientist to predict his scientific results, or to predict the direction of these results, or even only to outline the problems, and if one at the same time required him to stay within the limits of these predictions or outlines, this would be a death-blow to scientific progress. A good research project will, as a rule, yield unexpected results, and progress in science is best served by allowing the scientist to follow these leads, i.e. to define a new problem and to steer in a different direction. Happily enough, funding agencies are often aware of this and do not bother too much about mismatches between the proposal and the actual work carried out. But then one wonders whether the present research proposal cult, which is growing out of all bounds, is not really a liturgy rendering homage to an idol, and only serving the public illusion that the scientific progress can be 'controlled'.

The interdisciplinarity idol

Interdisciplinary research is better than monodisciplinary research. Dissatisfaction with scientific progress <u>within</u> certain disciplines, and general dislike of an 'ivory tower mentality' may be at the root of strong public pressure towards interdisciplinary research. If a scientist is only put into a situation where he is forced to consider problems, theories, and methods other than the traditional ones of his own field, new vistas of scientific progress will automatically emerge. To be sure, the recent history of science has witnessed the growth of highly successful interdisciplinary fields, such as biochemistry, biophysics, and psycholinguistics. But in my view, this has nothing to do with interdisciplinary <u>per se</u>. The viability of an interdisciplinary field depends on whether or not it cuts nature at its joints, i.e. whether the systems and processes studied are sufficiently autonomous and specific to warrant research in their own right. Interdisciplinary research may equally well lead away from such 'islands of nature' as towards them. There is an additional confusion here, which should be carefully distinguished. It is popular these days to say that research should be 'problem-oriented' (another idol), and that 'problems' usually defy traditional boundaries between disciplines. The comprehensive study of traffic problems, minority problems, or rehabilitation problems, for instance, obviously requires expertise from different disciplines simultaneoulsy. Therefore, 'problem-oriented' research requires interdisciplinarity. Though this is obviously true, the starting-point is less convincing, i.e. that real science should be 'problem-oriented' in the suggested sense. I will turn to this issue when discussing the next and last idol. Here it suffices to say that striving for interdisciplinarity as such amounts to trying to exert control over science by sheer magic.

The relevance idol

The promotion of science is best served by giving priority to the study of urgent problems in our society (there then follows an unbiassed listing of these problems). Though not dead, this idol has lost much of its revolutionary appeal over the last ten or fifteen years; its falseness is too apparent. I will not repeat the arguments here, but rather consider some offspring of the idol which are still alive and kicking, and which find considerable support among scientists themselves. The keywords are 'problem-oriented' and 'applied' science. As for 'problem-oriented' research, its meaning is dependent on what is taken to be a 'problem'. Usually, it takes the form of 'an urgent problem in our society', which brings us back to the relevance idol. At the other extreme a 'problem' can be anything arising from science itself, such as the chemical structure of DNA, universal properties of syntax, or the recognition of words. In that case, 'problem-oriented science' is just a faddish way of saying 'science'. Between these two extremes lies a third use of the term: A problem is any issue external to science which draws attention. This may or may not be a 'socially relevant' issue, a practical issue, an aesthetic one, etc. 'Problem-oriented research' is, then, the scientific analysis of such an issue. This sense of the term is, as far as I can see, indistinguishable from what is usually called 'applied research'. So let us limit the discussion to the question of whether applied research should be a privileged way to promote science. That there is a general move these days away from basic and towards applied research is a given, and I have always taken this as the unhappy result of funds drying up and scientists wanting to stay alive. This is regrettable, but not insincere. What disturbs me is to hear scientists proclaim that applied research is so exceptionally good for science. The Dutch Psychonomic Society, for example, is organising a conference on metatheoretical aspects of psychonomic research - a laudable initiative. However, a major part of this conference is dedicated to applied research. Why? Is one really presupposing that applied research has some intrinsic theoretical role to play in the scientific analysis of mental processes? This would be utterly off the mark. It is sheer luck when an applied problem reveals the existence of an hitherto unknown principle of mental (or, for that matter, of biological or physical) organisation. Almost any readily observable phenomenon or problem is the resultant of complex interactions. This fact does not preclude their scientific analysis, but this is not the most straightforward way to discover the laws of nature. The latter requires abstraction from interactions, irrelevant variables, and the like. Louis Pasteur must have had this in mind when he remarked that there are no applied sciences, only applications of science. It is true that applied research can sensitise a scientist's mind to potentially important variables. But here applied research is on a par with occasional observations, talking to colleagues, having dreams, reading books, etc. They are among the ingredients from which the highly associative creative process in science draws; but there is no special status here for applied research. It would be untruthful to proclaim this, and it will in the end hinder the advancement of science if politicians capitalise on such proclamations.

Just a last point here: I am a staunch supporter of applied research. There is a host of problems in our complex society which cannot be solved without applying the best of our scientific tools and methods. This should be done, and it should be done well. But one should not confuse it with science.

A goddess: the freedom of science

Science is free in the sense that it is disinterested. It approaches truth whatever the consequences. An unpopular, dangerous, or socially irrelevant truth is just as valuable in science as a popular, useful, or relevant truth. The only thing that counts is the internal dynamics of the inquiry. In last instance, this freedom has to be realised in the individual scientist's mind. This is not a luxury, but rather a responsibility, one which is becoming increasingly hard to live up to under growing public pressure and with a falling economic tide. Freedom of science is, like democracy, not a self-evident permanent characteristic of our society. It is vulnerable, and it needs continuous defence both within the scientific community itself and before the general public.

At the same time, the disinterestedness of the inquiry can in no way serve as an excuse for the scientist to refrain from signalling potential abuses of his results. In fact, public arguments against the freedom of science have often addressed scientists' neglect of this duty. But one should not throw out the baby with the bathwater.

Bow can science policy promote the advancement of knowledge? There is, first, the domain of applied research. Governments can promote the study of urgent societal problems, they can define desired results, technical developments and the like. Second, science policy can consist of setting priorities for fundamental research, stimulating one discipline or subdiscipline rather more than others. The heart of the matter here, however, is to create a maximum of freedom of whatever science is to be promoted. Every move to exert control over the course of the inquiry itself is doomed to be counterproductive.

An especially effective way of realising these boundary conditions for fundamental research is the establishment of research institutes with longterm funding and independent internal definition of the research program. This is, to a good approximation, the structure of IPO, and it has been put to good use.