Free Inquiry: The Haldane Principle and the Significance of Scientific Research
Alexander Bird and James Ladyman, University of Bristol

Introduction

What is the best way to organise science and how should it relate to the rest of society? Any answer to this question depends on a criterion of ‘best’? Best for the cognitive aims of science, the production of significant knowledge? Best for the production of economically and socially beneficial technology? Best by the lights of democratic engagement with important areas of social activity?

One might ask whether the question is even appropriate. Why should science and its relationship to society be managed at all? The question arises less naturally arise for religion, art, or sport. Perhaps, science should be allowed to develop its own relationship with the rest of society in an organic fashion, with minimal external management of that relationship. Yet science, it has long been recognised, is a significant social good. Society as a whole benefits from the promotion of knowledge, even when abstract and abstruse. Furthermore, science has the potential to do social and economic good — and harm. And in most countries, it is the recipient of large sums of public funding. So it is proper to ask how, in this case, the relationship should be managed.

On Regulation and the Vastness of Science

Science is now so vast that no single answer to the question of its management is plausible. Some medical science is clearly aimed at the production of drugs and therapies, and some engineering science would have no subject matter were it not for technology. It would be bizarre to suggest that these sciences should not be organised so as to maximise their social and economic benefits. Yet even within the most practical of sciences, the individual researcher may be aiming at knowledge and understanding for their own sake and their so doing may be necessary for the wider benefits of their work. The hole in the ozone layer above the South Pole was discovered in 1985 because data gathering (since the late 1950s) was an end in itself for British Antarctic Survey scientists.

Theoretical physicists and cosmologists arguably have only cognitive aims qua scientists because their theories are so remote from practical affairs. However, fundamental physics that once seemed completely esoteric gave us solid state and semiconductor electronics and nuclear power. It would not have such profound impact on our material culture if it had not been allowed to develop without having application as a goal. It is important to note therefore that managing science with the direct aim of maximising its social or economic benefits may not be the best way to maximise those ends, especially when it comes to theoretical science. In some domains democratic involvement may achieve exactly the opposite of the effect intended and make science less useful. Furthermore, democratic involvement may undermine the objectivity that often makes science socially and politically important.

Hence, one should not infer directly from the reasons that make the question a proper one — that science is socially important in various ways — that the answer, in a
liberal democracy, must be one that calls for some degree of democratic involvement in scientific decision making, in particular as concerns how funding for science is distributed, and which projects are given priority. There is no easy inference from the social importance of some activity to the rightness of democratic involvement in decision-making within or even concerning that activity. For example, the UK’s parliament is currently (March 2013) debating whether the press should be regulated by law (in addition to the more general provisions of the criminal and civil law).

The majority of those involved in the debate would have preferred a self-regulated press, and it is only the repeated failure of self-regulation and the enormity of some of the abuses by the press of its power, that have brought our democratically elected politicians even to consider democratic control over the press. This reluctance to exert control reflects the shared view that in a liberal democracy, democratic control over important elements of social activity is not always desirable. Other things being equal, many democrats feel that it is better to have a press free from democratic control. Indeed it is this self-denying feature of a liberal democracy that makes it a liberal democracy. It is widely recognised that this self-denying liberalism, as in the case of a free press, strengthens rather than weakens democracy. Illiberal democracies, those where the democratic winners take all, tend to lose their democracy also.

This self-denying feature is found elsewhere, even where arm’s-length democratic oversight is appropriate. For example, in the UK judges are neither elected, nor even appointed by elected politicians. They are appointed by an independent Judicial Appointments Commission (JAC), which is made up of a mix of judges, magistrates, lawyers, and lay members. While the JAC is very much concerned with important social issues surrounding judicial appointments (such as ethnic diversity), its independence from democratic control means that it is free to concentrate on the principal desiderata in a judge, viz. probity and legal understanding and judgment. (Similar principles apply to appointments in the civil service and in the armed forces.) In such cases, the structure of decision-making is ordered so that decisions can be made with a view principally to the effectiveness of the decisions (i.e. appointing the most competent individuals); indeed, in these cases remoteness of decision-making from democratic forces is regarded as desirable.

**Knowledge, Expertise and Decision Making**

We believe that the same considerations apply to the organization of science and its relationship to society as a whole. The aim of science is the production of important knowledge (Bird 2007). How should we decide which distribution of funds to potential projects will best achieve this aim? It is simply not possible to make such judgments without considerable scientific knowledge. Deciding which barristers would make good judges will require (among other things) examining the quality of the legal reasoning of the potential candidates, which can only be done fully effectively by those who themselves have good legal knowledge and judgment. Likewise, deciding which projects have good prospects of delivering valuable scientific knowledge requires having a strong background in the relevant science.

Simply understanding most project proposals in science will require a high degree of scientific literacy. While it might be possible to get over the gist of a project to a lay
person, that will rarely be enough to give a true comprehension of the project or its significance. Those who doubt this would be advised to read the abstracts in a leading scientific journal (e.g. *Journal of Physics C: Solid State Physics*). To understand the *significance* of a project is to understand its place in the recent and future development of a science, and that can only be judged by someone with a fairly extensive engagement with that science. Furthermore, to evaluate a project is to evaluate its *methodology*, which again will require scientific expertise. For example, a project proposes a sample size of 150 animal subjects. An evaluator will need to understand power calculations in order to check whether that enough to allow for statistically significant outcomes. How many of the animals might become sick during the experiment and thus not count toward the data? Does the experiment show appropriate understanding of the behaviours of the model animal? All such questions require scientific knowledge and experience.

If a person is able thoroughly to understand a project proposal and to assess its significance and methodological feasibility, then almost always such a person will be a practicing scientist from that field or a neighbouring one. There can be exceptions. Harry Collins and Robert Evans (2007) categorise expertise, and identify a level of expertise they call ‘interactional’. Someone with interactional expertise knows enough about a field to discuss ideas in the field with a scientist who works in it, but is not themselves a practitioner and does not have the level of expertise required to do original work in the field.

Collins himself has interactional expertise with respect to the experimental physics of gravitational waves (2004). Yet that came about only as the results of years of study of the gravitational wave community, including attending all their major conferences. Interactional expertise is difficult to acquire because it requires a great deal of investment of time and effort, in addition to the individual’s ‘day job’. That may be possible for a few fortunate individuals (such as Collins he is a sociologist of science), but for most others it will be well-nigh impossible. That said, it is possible and desirable for some outside the immediate field to acquire sufficient interactional expertise so that alongside the scientific experts and with the benefit of their views and advice, they are able to come to a reasonable view of the merits of a project, and in particular the relative merits of projects from different fields competing for the same funding. Such external interactional expertise is useful in balancing the particular interests of the expert scientists and adjudicating between competing interests. But such expertise is a supplement to the expertise of the scientists, not an alternative.

**The Haldane Principle and the Conduct of Research**

The idea that decisions about the selection of projects for funding should be made by expert scientists themselves, and at some distance from democratic control, is, in the UK, enshrined in the so-called Haldane principle. The Haldane principle was first articulated as such in 1963 by Quintin Hogg, Lord Hailsham, who had been Minister for Science and would later become Lord Chancellor:

> Ever since 1915 it has been considered axiomatic that responsibility for industrial research and development is better exercised in conjunction with
Hailsham was referring to the work of Richard Haldane. Viscount Haldane, who was twice Lord Chancellor, was a great Liberal and then Labour statesman and was responsible for many important reforms of the government and the military. One of those reforms, the outcome of the Haldane report of 1918, in effect set up the system of research councils that the UK has today. (The Medical Research Council predates Haldane’s report, having been founded in 1913. It was reorganised along the lines of Haldane’s report and received a royal charter in 1920. Hailsham’s ‘1915’ is a reference to the year in which the government’s Department of Scientific and Industrial Research was established.) Haldane not only promoted the research councils but also the use of research in informing policy-making in government departments. But the commissioning of the two kinds of research would be quite different. Research whose aim is to benefit the needs of government should be carried out or commissioned by the relevant government departments, not by the research councils. The structure and independence of the research councils would allow them to make decisions determined by the needs of science, not of government.

Because it was not laid down by Haldane himself, but expressed by Lord Hailsham forty-five years later, David Edgerton (2009) calls the Haldane Principle an invention. However, the fact that the principle did not have this name nor explicit form until Hailsham, does not mean that it did not exist, with something like this intent, in the minds of scientists, civil servants, and politicians. The fact that it is formulated and appealed to (or even rejected), as Edgerton points out, in moments of contention reflects not the fact that it did not exist at other moments, but the fact that did exist and was not being tested.

The Haldane principle has indeed been tested on several occasions. For the knowledge produced by science does also have the potential to benefit society, economically and in other ways. In moments of economic crisis especially, government ministers may feel frustrated that this link is perhaps not as effective as they believe it might be; they imagine that if public funds for science were directed more towards projects with clear potential for industrial exploitation, then the nation’s economic productivity would be markedly better. Hailsham’s remarks were directed at a Labour Party policy for science that would do precisely this.

While the left in British politics has on the whole tended to favour government direction of science toward national needs (exemplified by J. D. Bernal, 1944), it was a Conservative government that implemented (1973) a report by the Labour peer, Lord Rothschild, that called for the transfer of 25% of the science budget to government departments, which would then commission the research councils to carry out research that the departments considered to be in the national interest (Duffy 1986). A later Conservative government found the Rothschild reforms to be ineffective and reversed them. More recent governments (of both left and right) have made the economic and social ‘impact’ of research a significant determinant of the distribution of public funds, both to universities and to particular projects, while...
research councils have distributed an increasing proportion of their funding through themed programmes that likewise concern the application of research in economically and socially significant ways.

Many scientists and other academics, including the authors of this paper, are concerned that such developments in UK higher education funding policy constitute a dismantling of the Haldane principle. They do not do so in a direct way, by the direct involvement of elected officials in decision-making. Rather, the process is insidious, paying lip-service to the Haldane principle, while undermining it. It remains the case that researchers play the predominant role in deciding which research gets funded. Yet the criteria that they are required to apply and the programme structures through which they are funded are increasingly focussed not on the scientific significance of the research but on the promotion of broad governmental priorities. Furthermore, as we remarked above, there is a danger is that this interference with science will detract from rather than enhance its ability to meet government goals. If that proves to be the case the likelihood is that it will be mistakenly taken as evidence of insufficient external control.

A Democratic Approach

Kei Yoshida (2012) and Philip Kitcher (2001) promote a democratic approach to the organisation of science. This contrasts with the elitist approach that the Haldane principle embodies — the idea that the experts should set the priorities for research funding. Kitcher prefers the model of ‘enlightened democracy’, whereby ‘decisions are made by a group that receives tutoring from scientific experts and accepts input from all perspectives that are relatively widespread in society’ (2001: 133). Yoshida, on the other hand, sees no reason to reject what Kitcher calls ‘vulgar democracy’, where the group that decides is democratic and representative of the diverse interests in society, but is not tutored although it can receive advice from scientists. Kitcher fears the tyranny of the ignorant that vulgar democracy could permit; hence the importance of enlightenment — the democratic decision-makers are properly tutored. Yoshida thinks such fears are exaggerated, and that in seeking to avoid them, Kitcher in fact retreats to a form of elitism.

To an extent, Yoshida is right, since Kitcher’s enlightened democracy does require deference to experts at various points. Yoshida concentrates on the fact that the democratic decision-makers will depend on the experts’ assessments of the probabilities that projects will produce results that will promote the priorities laid down by the group. The point arises elsewhere also—and with greater severity. The decision-makers depend on the experts for their tutoring. What counts as success in this enlightenment project? Plausibly only the experts can decide whether an individual has acquired sufficient understanding to count as enlightened or tutored. And that assessment will depend in large part on the individual making the same judgments as the experts. That might work if there is an agreed distinction between areas where the experts’ judgment is relevant and determines correctness and those areas where the expert’s judgment is not relevant. But matters are not so clear cut. If, as many argue, science is imbued with values, then the distinction may be contested. Indeed that problem may arise with facts as much as with values. If a member of the democratically representative decision-making group should refuse to accept the truth
of evolution, despite intensive tutoring from the scientists, should that person be regarded as insufficiently tutored/enlightened to participate in decision-making? If yes—such a person should be excluded—then Kitcher’s model isn’t so democratic after all; if on the other hand such a person does remain in the decision-making group, then the tyranny of the ignorant does become a genuine possibility. A case such as this is surely not merely hypothetical.

Kitcher is right that a crucial question to ask is, what social good scientific research promotes. There is a general answer—the production of significant truth (or, perhaps, knowledge). The more difficult question concerns the notion of significance. Kitcher rejects various proposed accounts of objective, sempiternal significance, arguing that the best of them rely on a mistaken Unity of Science thesis. Instead he holds that significance is relative to context. Kitcher then argues that the idea of pure science, not concerned with the practical applications or consequences of research is a myth. Kitcher then concludes that significance is at least in part a matter of moral, social, and political context. The step from this to the demands of democratic control over what is deemed sufficiently significant to be worth pursuing (and so funding) seems a short one.

One can agree with Kitcher that significance is in an important sense contextual without accepting the further conclusions of his argument. Kitcher confuses two theses about the purity of science (a) there exists pure science whose significance is purely epistemic and not practical, and (b) ‘the absence of practical intent is enough to isolate a branch of inquiry from moral, social, or political critique’ (2001: 91). One can reject (b) without rejecting (a). In particular one can agree that because much scientific activity is potentially subject to such critique, it is proper to have mechanisms of constraint and scrutiny (such as transparency in research, ethics committees, and legislative frameworks). But it does not follow from this that the notion of significance to which those mechanisms apply is itself is laden with such public values.

A Kuhnian Approach

Thomas Kuhn (1962) offers us an alternative conception of the context-relative significance of scientific research. Kuhn’s model of science as puzzle-solving ensures that the scientific significance of a piece of research is relative to a paradigm. For it is the puzzle-solving tradition, determined by exemplars of good science, that generates the puzzles that scientists work on. Such puzzles might be analogues to paradigmatic puzzles; other puzzles might be matter of filling in gaps in the knowledge produced by the paradigm. For example, working within the Newtonian paradigm of cosmology, a puzzle of the first sort would be the determination of the orbit of a newly discovered planet or moon, to show how it fits with gravitational theory; a puzzle of the second sort would be the task of determining the value of the gravitational constant $G$. Changes in paradigms may lead to changes in scientific significance. If future theory tells us that $G$ is not a fundamental constant, but changes over time and/or in different parts of space, then determining its value with ever greater precision, will have a quite different significance. Likewise, measuring the atomic weights of the elements was an important task in nineteenth century chemistry, since these were thought to be absolute properties of the elements; this kind of
significance was transformed by the discovery of isotopes, showing that atomic weight reflects contingent matters such as relative abundance of the isotopes.

So Kuhn’s account tells us that significance is paradigm-relative. But significance on this view is still determined fully within science. It does not depend on external values. Some sociologists of science hold that the Kuhnian model does regard the development of science as subject to external, social influence. If that were correct, then part of the influence might be social determination of what counts as significant and worthwhile science. But Kuhn says very little to support such a view, and indeed he rejects the Strong Programme in the Sociology of Scientific Knowledge (Kuhn 1992). His approach is thoroughly internalistic—science develops under its own internal motor, governed by the general requirement to generate and solve scientific puzzles. It follows then that to direct scientific research by appeal to values and preferences coming from outside science is to interfere with the natural evolution of science.

The fact that the admixture of external influences would perturb science’s natural evolution does not itself invalidate the demand that there should be such influences, chosen democratically. But if Kuhn’s model is correct, then it does mean that science organised in such a way would not be science as we know it—it would be something more akin to technology. And let us say that science were to change, with different motivations and goals for research and maybe, as a result, different kinds of people engaging in research, then one might reasonably question such a science would be as effective at delivering even these external goals as a largely internally directed Kuhnian science.

A Thematic Approach

The approach of governments and their funding agencies, as exemplified by the UK’s funding councils, is often to expect funded science to fit broad themes, chosen for their economic and social relevance (e.g., for the UK’s Engineering and Physical Sciences Research Council: ‘Digital Economy’ and ‘Living with Environmental Change’). This makes it appear that scientists are still in control of the distribution of funding, yet it nonetheless directs funding away from projects that might be chosen on the basis of their internal scientific significance alone. This approach runs the risk of falling between two stools. On the one hand important scientific projects are not pursued. On the other hand, the breadth of the themes means that there is no guarantee that the research produced really will answer genuine needs or provide significant benefits outside science. Scientists will naturally squeeze, carve, and mould the projects they would like to pursue so that they fit within the theme. The result is research that is not good value for money either from the purely scientific perspective or in terms of its external social or economic ‘impact’.

In our view, both science itself and wider society would benefit from a clearer distinction between the funding of science on the basis of scientific imperatives, and the funding of science on the basis of the external use to which it can be put. The case for doing so is not undermined by the fact that there are where cases science may rationally be pursued for both kinds of reason. Priorities and decisions regarding science pursued for the first kind of reason should be made by organisations, such as
research councils, whose membership is predominantly drawn from among scientists. Non-scientists may be helpful also, not to bring values and preferences from outside science, rather in order to provide a neutral viewpoint in assisting in judging between competing claims from different areas of science. At the same time, it is true and desirable that government policy-making is informed by relevant science. That science should be funded (as it often is) by the department responsible for that policy area. That expenditure will be under indirect democratic control insofar as the government and its policies are also subject to democratic control.

**Conclusion**

A government department might also believe that it is desirable for socio-economic reasons that research is carried out in a particular area, without that necessarily feeding directly into policy.\(^1\) Since the benefits are external to science and relate to the remit of that department, then that research too should be funded by the department concerned, rather than from the research council budget. Funding that kind of research through government departments has the advantage that it will concentrate the minds of the departmental funders on whether that research really will have the socio-economic benefits claimed for it.

In the UK system as it is currently developing the motivation to ensure best value-for-money and the expertise to assess it for socio-economic benefit are not in the right place. The scientists sitting on research council committees making funding decisions will be better able to assess scientific value than socio-economic value. And insofar as government departments could make the latter assessments, they are not doing so, and even if they were to be involved, their motivation for making decisions informed by value-for-money would be lessened, since the budget is a research council budget not their own.

Our proposal, in effect a renovated application of the Haldane principal, would match the incentive structure to the intended aims of the different ways of evaluating science: the government and its departments, fairly close to democratic control, would fund and assess projects aiming at socio-economic benefits while research councils populated by scientists, at some remove from democratic control, would fund and assess projects aiming at advances in scientific knowledge.\(^2\)

**Contact details:** plajb@bristol.ac.uk; James.Ladyman@bristol.ac.uk

---
\(^1\) We ourselves think that the value of research that is neither motivated by scientific problems nor by precise external goals may frequently be insubstantial in terms of both internal scientific value and external social benefit.

\(^2\) This proposal has some superficial similarity to the Rothschild reforms. The problems with the latter were that (i) they involved a substantial (25%) transfer of funding away from the research councils to departments, (ii) the departments were not well equipped to assess the scientific merits of proposals for funding; and (iii) their contracts were with the research councils. We see no reason for there to be any transfer of funds away from the research councils. Government departments are rather better informed scientifically than they used to be, and in any case they too can call upon scientists to perform peer review on the scientific merits of projects. And finally, the contracts should be with universities and other research organisations and not with research councils, whose activities should be kept, as we propose, entirely separate.
References


