Timeliness in awarding research grants

Many of the numerous organizations that now support scientific research, mostly out of public funds, lay stress on the importance of timeliness in their choice of supported research topics, and furthermore go to immense trouble in order to ensure that only what they see as the very best research is supported. Thus, increasingly, selected themes, more-or-less precisely delineated, are announced as part of "calls for proposals"; submitted proposals are subject to a careful scrutiny, perhaps by four or five scientists, who are asked to write a written report, possibly accompanied by some kind of numerical grading. These reports are typically sent back to the proposers for comment (especially the correction of any factual errors in the reports); and finally the proposals, together with their reports and the proposers' responses to the reports, are ranked by a panel of peers. It is not uncommon for only a quarter or so of those proposals achieving the highest possible grading to be actually funded.[1]

The scheme has of course many flaws, and many critics, but nevertheless one notices that it has now been adopted in practically every country, even in recent years by new adopters, despite several decades of experience in countries such as Great Britain, which were early adopters, that made its drawbacks clear.

These drawbacks have been extensively elaborated upon elsewhere, and the purpose is not to repeat this elaboration here. The main criticism is simple, and essentially ostensive: most of the great discoveries of the late 19th and 20th centuries (X-rays, the electron, penicillin, the transistor—to say nothing of the great conceptual advances of the quantum and relativity) were made before the introduction of the "peer-reviewed research grant system" (PRRGS), as it is often called. In comparison, the output of the PRRGS has been comparatively meagre. The explanations usually offered are straightforward: a variety of reasons, ranging from Parkinsonian dysfunction of committees to voting paradoxes, contrive to make the output of the system (i.e. funded proposals) generally err on the side of conservative, incremental research. This result from such a system should actually occasion no surprise: once the grant has been awarded, the awarding body's preoccupation (that is then imposed upon the awardee as a condition of the award) is to ensure that what has been paid—indeed contracted—for is actually produced. The way of achieving this is to specify in as much detail as possible the actual course of the research, including specific so-called 'milestones', against which actual outputs can be ticked off even by the most scientifically ignorant functionary.

There is no need to expand upon the above, because it has been so extensively written about in many other places. It is mentioned now simply for the sake of completeness, and also because it is probably the most important reason for the failure of the system (as an instrument for promoting real advance of knowledge). This exposition will be confined to some features less prominent, but possibly no less important—they have simply received less attention.

One of them is rather straightforward. If the research is truly pioneering and original, the proposers are the only real experts. Any real contribution to knowledge involves an inductive leap, yet such leaps simply cannot be stated in a research proposal: they would come across as too vague and speculative. The panel, whose members in the specific field of the research proposed are by definition lesser experts, however eminent they may be in their own fields, can only consider deductive advances, which of course are not real advances to knowledge, hence essentially by definition no grant-funded research can actually lead to a contribution to new knowledge, except as an epiphenomenon. Such outcomes will probably not even be recorded as achievements of the project, since there will be no corresponding milestone, and hence no prepared box against which to tick off the achievement.

This defect is however not quite as bad in practice as it might appear to be, because new knowledge does nevertheless sometimes emerge as an epiphenomenon, despite—and this is yet another defect of the system—the erosion of the valuable thinking time of the researcher by the need to deal with the bureaucratic demands of grant-awarding bodies, such as submitting written reports, which those bodies would generally consider as being of greater importance than publishing papers in the scientific literature.

One of the most frustrating drawbacks of the system is the leisurely timetable by which it proceeds. The European Union (EU) is particularly guilty in this

[1] This difficulty is obviated by some organizations, for example the European Union's "Framework" programme, by allowing subdivisions of the grades and making strenuous efforts to avoid having more than one proposal sharing a grade, so that the proposals then essentially rank themselves, and it suffices then to provide finance for as many as funds allow, starting from the top of the ranked list.

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regard. For example, this month (September) some information about "Industry-Academia Pathways and Partnerships" (IAPP), part of the Marie-Curie package, was disseminated by the EU. The stated goal of this scheme was to promote the transfer of knowledge between academia and industry, and it works by financing the exchange of staff between an academic laboratory and a commercial firm. This could be very attractive, particularly for small companies wishing to rapidly adopt some new discovery relevant to their business that they have heard or read about. Yet proposals need only be submitted by the end of May 2008, after which time they will be evaluated, a process that will doubtless take several more months, and finally, possibly in the autumn of that year, the project would be able to start—almost 1 year after the initial announcement. Similar delays can be found in the workings of most of the national research grant-awarding bodies. Such a programme might be acceptable for the work of an individual researcher, who has worked for many years in his or her special theme, and can with reasonable accuracy anticipate future stages of the research several months ahead of their realization. But in actual fact, many of the research grants, especially those offered by the EU, require collaboration. How is the idea of such collaboration usually born? Two researchers happen to meet, typically quite informally, maybe at a coffee break in their institute, or at a conference, and suddenly hit upon an important new idea that is worth investigating. If the idea is indeed important, work should be started upon it immediately. This is actually possible in enlightened institutions, such as the National Institutes of Health in the USA, and was possible under the system of Academy of Sciences research institutes fostered in the Soviet Union and its satellites. But if the prerequisite for obtaining any additional resources needed is writing a grant proposal, the required detail of which will make that alone a task lasting typically one month, followed by submitting the proposal, possibly to meet a deadline that may only recur two or three times a year, and then waiting three or four more months before a decision can be made (during which interval the proposal is subjected to the "scrutiny of peer review" as detailed above), then a powerful damper is immediately placed upon exercising this kind of spontaneous, and most valuable, research agility.

There is little point in writing more about this. Most readers will doubtless agree. Even this criticism has probably been made somewhere else before. Yet still nothing is done to change the system. Achieving understanding of this state of affairs would itself be a suitable topic for research—but not one that is likely to be funded by one of the grant-awarding agencies!

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