CHAPTER 5

Research Funding: trends and challenges

*Leszek Borysiewicz**

INTRODUCTION

Research — the generation or collection of knowledge — is of the greatest importance. It can affect individual lives, society at large and even the fate of our planet. Uncountable sums of money are spent, and usually well spent, on moving forward our understanding of academic disciplines. Researchers access these funds in a variety of ways and account for their use, similarly, in a variety of ways. As each individual researcher knows painfully well, obtaining funding is a competitive activity — many more grants are sought than are awarded. And yet the effectiveness and efficiency of the various methods of allocating research funding are not well understood. What one might call "research about research" is thin on the ground. There is little agreement even on the appropriate methodologies to use to track either efficiency or effectiveness, and although the great majority of funds are dispensed to scientists by scientists (the arts, humanities and social sciences requiring less equipment and fewer consumables), it is in the social sciences that the necessary methodologies are to be found. Scientific funding boards, by implication, are not the best placed to rate their own success.

The principal thesis of this paper is that, in a context of poor data, trends in research funding methods and objectives need tracking. These trends are shaped by different funders, not necessarily acting with regard to each other, and so the possibility arises that by pulling the trend line up and down different axes, gaps can open up in provision.

The humanities have typically chosen to present their case for funding according to arguments of beauty and value, including (recently) economic value. Science has argued for funding on the basis of utility: and so it is entirely

reasonable that funders should particularly ask scientists to account for their success in those terms, and demonstrate the impact of their research. That there is an inherently long delay between funding a research project and observing the impact of the funding is generally understood — but a funder will naturally want to know that the research proposed is meaningful. As Gordon Graham, Professor of Philosophy and the Arts at Princeton Theological Seminary, writes, knowledge is not always valuable. "There is a fact of the matter as to how many people listed in a telephone directory between, say, pages 171 and 294 have surnames beginning with the same letter as the street in which they live, and quite some time could be spent ascertaining this fact. But the knowledge we would come to possess … would be quite worthless". (Graham, 2008, p. 88). A researcher proposing such a project for funding would have to do better than to argue "it may prove useful in some way, eventually". In this extreme example, a funder would have no difficulty concluding that any value in the research would be too small and too distant; in other cases (most, indeed) careful judgment is needed to weigh the scale, likelihood and imminence of a potential benefit.

Funders of research often have multiple options on where to place their investments: research institutes, R&D divisions of companies, or universities. Universities are a unique sort of organization and can make a strong case, based on that uniqueness, to attract research investment.

WHAT CHARACTERIZES GLOBAL UNIVERSITIES?

Leading, research-led universities are characterized by three commitments:

Excellence in both education and research. The best research-led universities are also committed to teaching, in a variety of modes from intensive supervisions to large-scale lectures, often using innovative technology, at both undergraduate and graduate level. We place heavy bets that enough of our faculty members (hired principally for their research excellence) will also have a taste and aptitude for teaching — bets which are hedged by the great variety of modes of teaching we employ, and bets which at institutional level pay off: it is unusual to find an excellent research-led university whose teaching is assessed poorly. The essence of a university in the 20th and 21st centuries has been the unity of teaching and research. Although universities have local, national and international responsibilities to admit talented students and to teach them to the highest degree of excellence, it is by our research performance that we stand or fall, and that our global reputations are made.

Disciplinary breadth. Universities are characterized by a broad span of disciplines, from the arts to the physical and often the medical sciences. The best universities actively find ways to encourage the productive cross-fertilization of ideas between disciplines, helping the creative process of determining

research directions, and also providing new applications, by employing the innovations of one discipline in another. Cambridge is fortunate to have inherited from medieval times a College system which achieves this mix superbly. Other institutions have consciously evolved other strategies to obtain a similar result.

Relevance to society. Both our teaching and research efforts are relevant to the societies which we serve. If ever there was an age which contrasted ivory tower universities with "the real world", that age is over. Serving society, disinterestedly, is at the core of what we do. Many universities capture that purpose in their formal mission statements — Cambridge's mission statement for example is "to contribute to society through the pursuit of education, learning, and research at the highest international levels of excellence". Though national and local missions remain important, in the 21st century, society is construed globally.

Universities are the only providers of research in which all these benefits are unified in one institution.

LEAGUE TABLES

Measures of education, research and contribution to society are used (often indirectly) in league tables — which, although artificial and tendentious, are of course enormously influential. Their simplicity is seductive (University A immediately appears "better than" University B because A scored 82.3, whereas B only scored 82.1), and their proper interpretation requires, but doesn't often receive, some sophisticated analysis.

Positions in institutional league tables are almost absurdly sensitive: my university, currently at the top of U.K. league tables, could easily drop several places simply by sneezing — or, as frequently happens, by small adjustments in the weightings given to various factors by the creators of the league tables. Nothing substantive about the quality of our education or research would have changed, but external perception certainly would change.

What are funders to do with the information that they think league tables are giving them? Industrial funders of research often identify partner universities by their strengths specific to the industry in question, and government agencies funding research typically make funding decisions on the merits of the particular grant application before them. In each case, the institution's overall position in league tables is less relevant than excellence in more specific areas. This allows for the emergence of "pockets of excellence": high-performing research teams and centres within an otherwise average institution. Such "pockets" have three possible fates — most die away when the key researchers move or retire, but more productively a "pocket of excellence" might move wholesale to another institution — or the home institution might

succeed, during a brief window of opportunity, in creating new critical mass by combining and supporting them, and thus contribute to the whole institution's movement up the quality scale. A funding system based on institutional league tables would squander that opportunity.

In the worst cases, governments can use league tables to direct short-term research funding to favoured institutions, particularly in countries where funding decisions are not robustly separated from the priorities of the government of the day — making it almost impossible for research groups in lower-ranked institutions ever to progress. Although governments have a legitimate interest in asking the research community to solve particular problems of practical public policy (for example in understanding patterns of criminal offending), the decision of which research groups receive that commission is best made by the community of researchers themselves. At that level of granularity, governments cannot, and should not, pick winners.

HOW DO FUNDERS CHANNEL RESOURCES TO RESEARCH?

Since we have ruled out governmental whim as an effective means of putting funds in the hands of individual research groups, how is that decision best made?

At its best, the relationship between funder and researcher is a continuing dialogue, tailored to individual talents, interests and objectives. Government research funders have an obligation to part with their money; philanthropic and industrial funders often do not, and the difference can shape relationships. In practice, most large funders run competitions of one sort or another, and funding models are designed often in *pointilliste* detail in the hope of obtaining an increasingly closely-defined outcome.

Impact versus Excellence

In the U.K. at least, a veneer of "impact" now colours pretty much every sort of research (e.g. from the Research Councils U.K. website, "Excellent research with impact is central to Research Council activities" [RCUK, 2013]). In considering the impact of research, the U.K.'s Research Excellence Framework also requires 2* minimum quality ("very good") in the underpinning research. The equivalent exercise in Australia makes no such requirement, the underlying logic being that quality of research need not be a pre-requisite for impact. Do research contributions that are effective in meeting practical challenges also need to be academically excellent?

Peer review

Research proposals are usually vetted by others in the field who are not compromised by being in direct competition for the same funds. This process pro-

duces a self-evaluating community of scholars and helps ensure excellence and independence. As an evaluation tool, peer review is used in over 90% of formal funding allocations — but here particularly the evidence base for effectiveness and efficiency is lacking. RAND Europe, a widely-respected research consultancy, evaluated 13 frequent criticisms made of peer review, and found sufficient evidence in studies (i.e. "research about research") to conclude that three of those criticisms were valid; one was not valid; and the remaining nine were "unclear" — in other words, that there was insufficient data. (Guthrie *et al.*, 2013)

- The three "valid" **criticisms** — those for which there was sufficient evidence — are interesting.
- **High cost**. Although research assessment is inherently bureaucratic, peer review is particularly so. The cost is principally measured in the time required, and is exacerbated by the opportunity cost: universities want their best researchers to be researching, not reviewing. The Wellcome Trust — a global U.K.-based charitable foundation which funds biomedical research in several ways including responsive-mode grants — found that fewer than 50% of those approached contribute a review (and the Trust has since introduced a peer-review college, which enjoys a higher review rate. Members join the college on the understanding that they will not be approached for more than six reviews in a year). Anecdotally, the more successful and renowned the reviewer, the less likely they are to contribute a review — though again, data is lacking.
- **Unreliability**, evidenced by wide variety of ratings given by different reviewers. There is a question as to how effective peer review is at discriminating between several research projects which are all at an international level of excellence: U.K. Research Councils routinely grade a much higher proportion of research as A* (meaning internationally excellent), than they are able to fund — so need tools to discriminate — but it is arguable that though peer review is good at defining whether a piece of research is internationally excellent, it can't readily distinguish at a more granular level than that.
- **Lack of transparency**, in the common case of reviews being provided anonymously.

The principal conclusion of the RAND review however was that the great majority of the criticisms — whether they proved to be valid or not — were anecdotal, and had little firm evidence behind them. There are few ethnographic studies, and no studies of how gender balance on a panel might affect the outcome; conversely, there is evidence that the time of day when applications are considered does have an effect. The general conclusion was that peer

review, though still the best mechanism for assessing academic merit, is itself a rather unscientific process: it is carried out by fallible human beings.

Typology

It is possible to sketch a rough typology of funding models and reasons for their variety, and the paragraphs below attempt this.

Investigator-led, responsive-mode grants

In this mode, an individual investigator (or, in a few cases, several such investigators acting as a consortium, perhaps across more than one institution) submits a project funding proposal in response to an open competition. The idea for the topic and scope of the proposed research comes from the mind of the researcher, and is most likely of all the possible modes to warrant the description of "blue-skies" research: inherently risky experiments which may or may not work. (Society at large may or may not be supportive of this risk, where it derives from taxpayer investments.) The proposal is peer-reviewed, and awards made on the basis of the review. This is a well-understood method, whose benefits include providing a gathered field of competing bids.

Funders are encountering problems with this model which they find difficult to address, and other models, considered below, are gaining ground. In the U.K., the three-year project grant, for which a tenured researcher makes a case through a grant application, was once the norm, but is now much more restricted. Reasons for its decline include:

- Demand far exceeds supply. The U.K.'s six Research Councils are charged with the allocation of public funds to research across the arts, humanities, sciences and social sciences. The median success rate they reported in 2010-11 for responsive-mode standard research grants was 22.2%. The highest success rate was 33% (at the Engineering and Physical Sciences Research Council); the lowest 15% (at the Medical Research Council).
- Bias against younger researchers. The average time in years between appointment as a Principal Investigator and the award of a first research grant is increasing. Early-career researchers do not have as high as success rate as established investigators. Further, the National Institutes of Health in the U.S. reported that most investigators were now in their 40s before they succeeded in obtaining their first award (37 in 1980, compared to 42 in 2008). (National Institutes of Health, 2008, p. 53).
- Administrative costs to the funding body. It is much more cost-effective for funding bodies to administer one £30m grant than 30 £1m grants.

Grand Challenge model

In response to weaknesses in the responsive mode, and in order to marshal the resources of the research community, several large funders now favour what is called the "Grand Challenge" model. This is a spectrum: the challenge can be more or less closely defined. The U.S. Defense Advanced Research Projects Agency, DARPA, uses the (bureaucracy-light) "prize" model; its "Grand Challenges" are open competitions, with teams constructing driverless vehicles (and more recently humanoid robots) which compete against each other. This approach has a distinguished history: in the 18th century the British Parliament established a generous financial prize, administered by the Board of Longitude, to stimulate innovation to solve a specific problem: the measurement of longitude at sea, vital for the increased maritime trade of the period (Cambridge Digital Library, 2013). In this format, there may be prizes for the "top" few places, but it is entirely possible for a competitor to incur significant expense with no reward.

The Bill and Melinda Gates Foundation led the way in popularizing a different sort of "grand challenge" model which identifies an ambitious target — the eradication of malaria, for example — and funds large teams to contribute to meeting that challenge. Multi-disciplinarity is well catered for in this model — as is multi-institutional research, since the concept is that the very best researchers from around the world bring their minds to bear on a single problem, but from different angles. This version seeks to combine the virtues of top-down and bottom-up methods.

Depending on the point along the spectrum of broad to narrow, downsides to this model include:

- **Risk of homogeneity**. Universities and institutes all want a slice of these very large pies, and so configure themselves to meet the best-known challenges: meaning that they all end up focusing on the same problems.
- **Risk to the pipeline**. This model tends to produce thematic "centres" in universities (Energy Centres, Institutes for Food Security, etc.) which attract talented researchers (and, particularly, researchers who talk a good talk) — potentially depriving the discipline-based faculties and departments of funds and people to develop and retain core skills upon which successful research relies. The depth of understanding created in the latter sorts of department is critical to the pipeline that will enable the thematic centres to solve the grand challenges.
- **False impression of the tractability of the problem.** Awarders can be ill-informed about the "researchability" of a topic. Some challenges are not particularly sensitive to the number of dollars thrown at them, and can be susceptible to fashions. An example is the U.K.'s fixation

with superconductivity in the late 1990s. The central assumption was that superconductive materials would allow highly efficient overhead cables in the electricity grid, at potentially transformative cost savings. "Proof of concept" existed, and funding was narrowly directed at research teams who were challenged to create the ideal material. Expensive centres sprang up in U.K. universities, and it was considered only a matter of time before the key breakthrough was made; it never was, and the funding eventually ceased.

If the challenge is sufficiently broad — as in the Gates Foundation's mission to eradicate malaria — then to the individual investigator, bidding for funds, it will be almost indistinguishable from "blue-skies" research. Crucially, the key idea for which funds are sought is the researcher's. To a researcher, "explain how your idea contributes to this public good" is much more attractive than "solve this specific [and potentially insoluble] problem".

Awards to support individuals, rather than projects

Especially in the sciences, investigators building a serious program of research will have several research projects in their lab. Since many more grant proposals are made than are funded, investigators are constantly writing (often fruitless) grant applications. To break out of this pattern, the research community has petitioned funding bodies to make large, bold investments in stellar individuals, so that this generation of Einsteins does not spend their time and energy on writing grant proposals. (As noted above, the peer review process is not good at supporting true paradigm-changing research, so it is entirely possible that today's Einsteins will have their grants rejected.)

It is also recognized that in responsive-mode grants, investigators tend not to perform the experiments which they originally set out (and applied for funds) to perform. The funders know that, even if their funding scheme intends to support ideas, what they are actually doing is investing in people, whom they can trust even if they divert from their original and intended path.

The Howard Hughes Medical Institute in the U.S. has had great success with the "people not ideas" approach. The 330 current HHMI Investigators include 164 members of the National Academy of Sciences and 15 Nobel Laureates (Howard Hughes Medical Institute, 2013).

Despite obvious successes, this approach like the others has its downsides:

- **Pressures other than excellence**. Even if the lion's share of award-worthy individuals are in one department or one institution or one country, the unhelpful signals sent by allocating resources accordingly are often too unpalatable for the funding body, which may impose — probably without articulating it — a quota.

- **The gap in the middle.** Several significant funding schemes target young researchers (including for example the E.U.'s Marie Curie Fellowships). Several others reward senior, established professors. Fellowships for those in mid-career are rare in comparison, especially in the sciences: many post-doctoral scientists find themselves too senior (and expensive) to be employed on another contract, but too junior to be appointed to an established position.
- **The vicious spiral**. Investing in individuals rather than responsive-mode grants takes out of circulation a large sum that would have gone into thematic research — introducing the possibility of a vicious spiral whereby researchers can't show the track record of competitive project funding necessary to qualify for fellowship awards, precisely because the funding bodies are focusing resources on such awards and not on project funding. Data to support this theoretical possibility is not available, but Wellcome Trust evidence does show that while the total amount spent by the Trust is the same, the number of grants awarded is decreasing, while the size and length of grants are increasing. Competition, therefore, is higher.
- **Two-tier research.** The approach also picks "winners" at an early age, risks creating a demoralizing two-tier system — those with individual funding and those without — and potentially leaves very able researchers without the means to set up their research group.

European structures

The development of the European Research Area (ERA) and the increasing importance of European funding to research-intensive universities have significantly challenged our thinking. The overall budget for Horizon 2020, the E.U.'s eighth framework program for science and innovation, is 70.2 billion euro (US$92 billion), built on three pillars:

- Excellent science, delivered primarily through the European Research Council
- Industrial leadership
- Societal challenges

There are many positives in this approach, but it is the case that many of the themes have been decided "top-down", with limited input from the community of European research-led universities. I believe that there are echoes of this trend in other parts of the world.

GENERAL TRENDS

The trends identified in the above typology are away from shorter grants towards longer; away from individual applicants towards collaborative work; away from single-discipline focus towards multi-disciplinary breadth; and away from blue-skies, investigator-led speculative approaches towards centrally-defined themes to which investigators are expected to respond. The effect of any one of these trends would be small — but the net effect of the combination may be to damage the generation of genuinely new knowledge.

Tackling global grand challenges is laudable and is indeed among our core duties, but doing so relies on what Donald Stokes, sometime Dean of the Woodrow Wilson School at Princeton, has called "basic research with considerations of use": the sort of work Pasteur did, which Stokes contrasts both with the pure curiosity of Niels Bohr, and — critically — with the applied focus of Thomas Edison. The combined trends in research funding appear greatly to favour our Edisons at the expense of our Pasteurs. As with all else, moderation is key: it is valuable for some of our researchers to be looking at this year's grand challenge, as long as they are not all doing so (Stokes, 1997).

This package of trends brings with it a shortening of time horizons. Every proposal now needs to demonstrate a measurable short-term impact. "Strategic themes" are identified, sometimes under political (fiscal) influence, as those responding to a perceived *current* challenge. The risk to the pipeline of research is obvious; and the risk to institutional and individual autonomy is obvious too. There is an associated risk to universities: it is much easier for politicians to control the inputs and outputs of short-term research if it is performed in government-funded research institutes.

SOME RECOMMENDATIONS, SOME CONCLUSIONS AND SOME QUESTIONS

The assertion at the beginning of this paper — that the large sums spent on research are usually well spent — does not rely on a mass of trustworthy and verifiable data, but on anecdote and experience. Nevertheless, it is an assertion which the research community overwhelmingly believes to be true. The inefficiencies in the system, particularly around peer review, result chiefly from the need to design out the worst flaws of caprice and bias. The need to track trends, and to make corrections where gaps in provision emerge, is nonetheless clear.

It is imperative that universities retain their depth and continue to supply fundamental research of the first quality. As a system, research funding bodies must always keep funds available for individuals (not just large collaborations) and for basic research (not just applied). It is difficult though to know how much is enough. Responsive-mode grant-giving (or at least, enough of it)

should be genuinely un-earmarked, and open to speculative bright ideas. Responsive-mode programs renounce a good measure of their usefulness if they are hijacked by fashion, and by the temptation to pick winners.

The diversity of funding models is valuable, and the trends identified in section 4 above eventually risk damaging diversity, by tending towards homogeneity. Agencies should maintain separation of roles. National public-funded bodies (e.g. in the U.S., the National Science Foundation, the National Endowment for the Humanities; in the U.K., the six Research Councils) lend themselves to responsive-mode, investigator-led basic research: supporting *ideas*. In Europe, the European Research Council can play the complementary role of supporting excellent *individuals*.

The systems-oriented changes outlined above will have a tremendous impact on research-intensive universities. It leaves them with challenges, which include:

- Ensuring that a university structure which is still largely based in discipline-based units can deliver multi-disciplinary solutions
- Combining grand-challenge approaches with investigator-led research, preserving the distinct benefits of both
- Avoiding the institutional instability that can result from increasing support for star individuals, coupled with increased mobility of researchers and increasing requirements for costly infrastructure
- Promoting strategic research partnerships, with academia and with the private sector, domestically and across national borders, in the changing research environment illustrated above.

These new shifts and tensions in research funding carry enormous implications, with risks and opportunities in equal measure, both for funders and performers of research — but also for the wider world. We have a responsibility to get it right.

***** *The author gratefully acknowledges the assistance of Matthew Moss of the University of Cambridge in helping to write this contribution; and of Dr Steven Wooding and colleagues, at RAND Europe, and Dr Liz Allen of the Wellcome Trust.*

REFERENCES

Cambridge Digital Library (2013). Board of Longitude. http://cudl.lib.cam.ac.uk/collections/longitude.

Graham, Gordon (2008). *Universities: The Recovery of an Idea*, (Societas). 2nd Edition. Imprint Academic.

Guthrie, S., Guerin, B., Wu, H., Ismail. S. & Wooding, S. (2013). "Alternatives to Peer Review in Research Project Funding: 2013 Update." Santa Monica, CA: RAND Corporation, 2013. http://www.rand.org/pubs/research_reports/RR139.

Howard Hughes Medical Institute (2013). Investigator Program. http://www.hhmi.org/programs/biomedical-research/investigator-program.

National Institutes of Health (NIH) (2008). 2007-2008 Peer Review Self-Study: Final Draft. Washington, D.C. National Institutes of Health (NIH), 29 February. http://enhancing-peer-review.nih.gov/meetings/NIHPeerReviewReportFINALDRAFT.pdf

RCUK (2013). "Why do RCUK consider demonstrating and maximising the impact of research to be significant?" http://www.rcuk.ac.uk/kei/impacts/Pages/maximisingimpact.aspx.

Stokes, D. E. (1997). *Pasteur's Quadrant: Basic Science and Technological Innovation*. Brookings Institution Press.