

# THE CANCER COMPETITION?

Selected medical research could be funded through competitive prizes, suggests **Jeremy Shearmur**

## Introduction: a problem

As we get older we may suffer from a variety of diseases and, in the end, we die. We have, as a result, an interest in the discovery of cures for these diseases—out of self-interest, compassion, or in memory of the victims. Large sums of money may thus be raised for certain kinds of medical research.

Donors may become frustrated that not all that much seems to result from this expenditure. Clearly, in some cases it is a consequence of the scientific complexity involved. The more we discover about some problems, the more we discover that we had not the faintest idea, earlier on, as to what kind of issues were involved, and how difficult they might be. At the same time, those who give may also have a certain suspicion about what the money is being spent on. It is not that we suspect fraud—that it is not being used for scientific research. The issue is more whether the funds that we have given are really being applied to the kind of problem that concerned us, as donors.

There is every reason to believe that our suspicions here may be correct, but that there is a rational explanation for why this problem occurs. As Thomas Kuhn's *Structure of Scientific Revolutions*<sup>1</sup> and subsequent work on the sociology of science has brought out, those working in scientific research typically operate within paradigms or scientific research programmes. These are driven by broad explanatory ideas, or by other scientific

work which serves as a model. Each researcher thus typically has an agenda for work that he or she needs to undertake. This, however, seldom corresponds to the work that those who wish to put up charitable funding for medical research have in mind—there is, indeed, no reason why it should.

As a result, funds are typically offered for the pursuit of research on the basis of certain descriptions furnished by those who have responsibility for the funds. In the face of this, researchers then try to find ways in which what they legitimately wish to do could be re-described in terms of the fund-holders' criteria. The fund-holders, with a commitment to giving out their funds, make a choice among these applicants. The researchers then go off, and do whatever it was they were wishing to do anyway, while remembering to use appropriate language when communicating with their funders. (They are here behaving responsibly as scientists and researchers in the context of the disciplines within which they are working. Researchers, these days, must also typically attract funding from outside their academic institutions in order to be able to undertake most kinds of research.)

Those members of the public who put up the funding in the hope that it will lead to a cure for

**Jeremy Shearmur** is a Reader in Philosophy in the School of Humanities, Faculty of Arts, ANU. He is the author of *Hayek and After* and *The Political Thought of Karl Popper* and joint editor of *H. B. Acton, The Morals of Markets* and related essays. Endnotes are available at [www.policymagazine.com](http://www.policymagazine.com)

whatever concerned them—or at least progress towards this—are, if they get to understand how these things work and to reflect on them, likely to be concerned. The researchers, if pressed, will presumably say that their work should make a contribution to a wider body of knowledge which might, eventually, prove relevant to the development of a resolution to the problems underlying the disease in question.

A possible solution to these funding problems is that medical charities might consider not the support of research, but, instead, the offering of a large prize for the solution of a particular problem in basic research<sup>2</sup> relating to our understanding of the disease in question. (This suggestion concerns the promotion of key moves in the understanding of the character of the disease, not the development of treatments. The latter introduce other complications—not least because of regulatory and safety issues—which cannot be sensibly addressed here.) The character of the prize problem—or better, of successive problems as research progresses—would be set by the charity's specialist advisers. A prize would allow these advisers to set an agenda—based on their understanding of the current problem-situation relating to the study of the disease—which could contrast with the agenda that comes out of the purely scientific paradigms or research programmes within which the researchers are normally working.

The idea that prizes might be used for the funding of research is itself an old and distinguished one. In addition, proposals of in some ways similar arrangements have recently been suggested in respect to problems ranging from issues of public policy to attracting attention to the treatment of rare diseases.<sup>3</sup> In addition, such approaches have already shown their worth in practical terms, in a number of fields, including commercial ones.

### Rousseau and the Dijon Academy

One of the more striking works in political philosophy written in the 18th century was Jean-Jacques Rousseau's *Discourse on the Origin and Basis of Inequality Among Men*. Rousseau's analysis has also had a striking influence, up to our own time.

Rousseau himself was a strange figure, and almost a caricature of the 'romantic genius', the idea of which he in some ways presaged. However, it is

worth noting that Rousseau the proto-Romantic did not write his *Discourse* spontaneously. Rather, he did so in response to a prize that had been offered, in the autumn of 1753, by the Academy of Dijon. They offered a prize for an essay on the topic: 'What is the origin of inequality among men, and is it authorised by Natural Law?' This was not the first such prize for which Rousseau had competed. His *Discourse on the Arts and Sciences* came about as a result of his having seen by chance in 1749 an advertisement—in a newspaper that he took with him when going for a walk in the woods—for an earlier prize essay, again by the Academy of Dijon. That time, the topic was: 'Has the progress of the arts and sciences done more to corrupt or to purify morals?'. Rousseau seems not to have simply fitted his prize essay into a pre-existing research agenda of his own. For if we can trust what he says in his *Confessions*, the advertisement seems to have produced in him a sudden inspiration, and a dramatic insight that informed his subsequent writings on inequality and education.<sup>4</sup>

Of Rousseau's prize essays, it is clearly—for good or ill—the second which was the most significant. But it was Rousseau's first *Discourse* which won the Dijon prize, while his much more substantive *Origin of Inequality* didn't. This serves to illustrate part of the attractiveness of the prize structure.

To see this, let us look at this situation from the perspective of the Dijon Academy. Their interest was, presumably, in the fostering of scholarly discussion of an issue that seemed to them of importance. Rather than offering a grant, they approached the matter by offering a prize. From their perspective, this clearly had merits. First, it meant that people had to write on what the Academy was interested in, rather than finding ways of re-describing whatever it was that they wished to do in a manner that might receive grant funding from the Academy. Second, the Academy's prize was of sufficient value to attract work of some quality.<sup>5</sup> Third, there were obvious economic advantages to the Academy in proceeding in this way. For if the prize is attractive, then a considerable amount of work will be generated beyond that undertaken by the winner. After all, in Rousseau's case the *Origin of Inequality* didn't win the prize, but was clearly an interesting contribution to the Dijon Academy's concerns. The prize essay competition

also produced much more than Rousseau's essay: there was, after all, the essay that won the prize, and sufficient material was produced for a French scholar to have subsequently written a book on Rousseau's competitors.<sup>6</sup> Indeed, if a prize offered in such circumstances is significant, it could well be the case that much more funding is attracted to its pursuit than the overall value of the prize.<sup>7</sup> In some other settings, it has been argued that the results of such an effect may be problematic—for example, when, say, a few top-rated pop singers, sports players and so on gain astronomical rewards, this may have a distorting effect upon other economic activity, in the sense of diverting large numbers of no-hopers and of moderately good people into the field, with a consequent waste of resources.<sup>8</sup> But from the point of view of those concerned to get the

Indeed, if a prize offered in such circumstances is significant, it could well be the case that much more funding is attracted to its pursuit than the overall value of the prize.

best value out of charitable donations for medical research, it is not clear that this is a problem!

### Back to medical research

Charitable bodies concerned with the promotion of medical research might take a leaf out of the book of the Dijon academy. It is, thus, suggested that they might consider shifting a significant part of their funding from the sponsoring of research and the making of grants to research facilities, to the sponsoring of large prizes for the provision of a solution to specific problems.

What would be required is this.

First, they would need an active and able research committee which takes an overview as to just what it would be most useful to discover—what specific problems it would be useful to solve—based on a good knowledge of the current state of the relevant disciplines, and with an eye to making progress towards the understanding, treatment and cure of the disease in question. This, in itself, would seem to be a useful exercise. For it is not clear that, currently, there is a motivation for able

people to take an overview as to what is going on, with regard to the formulation of an agenda for prospective research on a specific disease. We live, increasingly, in an age of scientific specialisation.<sup>9</sup> But such specialisation favours researchers' solving of puzzles in their niche as opposed to taking an overview of an entire field of research. (One reason for this is that work undertaken from such a wider perspective is likely to be difficult to publish in a high-prestige refereed journal.)

Second, it would mean that the research committee can be creative in the identification of problems and their relative priority. This would make a welcome change to spending their time evaluating research proposals from people who really want to do something else, against their organisation's broad criteria for the support of research. It could be argued that this poses a risk: someone may have a genuinely new and worthwhile idea about a line of research that should be pursued that has not occurred to members of the committee. This is a genuine concern, but insofar as it is telling, it suggests an argument for not putting *all* of our research financing into prizes, and for the funding of basic research; an issue that I will address later in this article. In this particular context, however, any likely losses in the face of this problem are more than compensated for. The committee would not have to spend its time in trying to sort through all kinds of proposals which do not really have anything to do with the key issues generated by the task of addressing the medical condition in question, but which are, rather, products of the current system for the funding of research.

Why would they receive such applications under current circumstances? It is important for those currently undertaking research to attract funding, not least because their ability to attract future funding from government may depend on success in such endeavours. There are areas of scholarship, such as some parts of philosophy, in which extensive funding is not needed; but even those who work there require access to books and articles which, these days, can be expensive. The ready availability of information sources on the internet means that people discover that there is much more they need to have access to, if they are to make a contribution to the current state of scholarship. The situation here is particularly difficult for, say,

those economists whose work depends on their getting access to data sources that are currently prepared for commercial markets, and which are thus sometimes extremely expensive, and which it may be beyond the resources of some academic institutions to provide for researchers.<sup>10</sup>

As a result, various tools have been developed, giving information about potential sources of funding, which researchers can search on the basis of key words. This, however, means that a funding source will typically attract proposals from people who have come across them as a result of undertaking such a search.<sup>11</sup> The canny researcher will customise his or her research to the specific requirements of the funding source, once it has been identified. But there is no special reason why what they wish to do will track quite what the funders would like them to be doing.

Let me illustrate this from my personal experience. In previous employment, one of my tasks was to scrutinise applications for funding, generated on such a basis. It became obvious that our applicants included some highly intelligent people. They manifested their intelligence not only by the quality of their applications, but also by their ability to present themselves as the kind of people whom we wished to support. Of course, they might have been genuine; but it was difficult for us to discover this just from their applications. We could either risk misapplying money by making awards to people whom we would not have wished to support if we had known more about them. Or we could undertake additional expenditure (for example, to finance face-to-face meetings) to try to work out how genuine the applicants' concern was for the ideas in which they had expressed an interest.

Third, as I have suggested above, if the choice of the problems set up for the prize is made intelligently, it is likely to be the case that much more research will be undertaken relevant to the concerns of the charity than just the production of the work that wins the prize. A contemporary example here is furnished by the Ansari X Prize. A prize of US\$10 million was put up for a project relating to flight. Those responsible reported that 'Twenty six teams from seven countries signed up to compete for the \$10 million prize that leveraged more than \$100 million in private investment.'<sup>12</sup> At a more modest level, even the Dijon Academy

received twelve entries for its prize essay competition in 1754, including Rousseau's unsuccessful but ultimately highly influential piece.

### What is to be done?

This approach should be tried, selectively, by one or two medical charities. Indeed, they might undertake fund-raising specifically with such a goal in mind. They would require a top-flight advisory team to formulate the prize questions. There would seem no reason why those involved in this should not be remunerated, and in addition every reason why the

The canny researcher will customise his or her research to the specific requirements of the funding source...

charity should publish a record of the deliberations of these people, on a year-by-year basis. Not only would such discussion be important in terms of providing guidance to researchers; but it might also serve to expose for criticism tacit assumptions which are currently being made by leading people in the field. It would also be of real public interest to understand just how such people are thinking about these things, and thus to get an overview of how serious thought about these matters is taking place.

The whole process would appear to me to depend, for its effectiveness, on there being only a limited number of such prizes being on offer at any one time, and on the sums put up being quite large. (It would be an empirical matter to discover just what is needed for the process to be effective.) What is more, they would need to be sensibly organised—that is, with prizes being given, in succession, for what the key steps were taken to be, as one after another specific result was arrived at, rather than, say, a large sum being put up for 'a cure for cancer'. In practical terms, one might imagine that such an approach would work best in areas which are emotionally significant, of quite high profile, and where the people who suffer from them, or who are associated with people who suffer them, are likely to be relatively wealthy.<sup>13</sup> Accordingly, such topics as heart disease; lung, breast and prostate cancer; strokes, Alzheimer's disease and

Type 2 diabetes<sup>14</sup> would seem to be among the most obvious candidates. For these kill a lot of affluent people. (One might say: what about obesity as such? But here, while more and more people are obese, and they pay reasonable sums for things that are allegedly of use in helping them lose weight but which seem hopelessly ineffective, it is not obvious that obese people would make donations to such a cause. One might think, rather, that it is a field in which there are major commercial opportunities for those who could put together an effective mixture of medication and lifestyle modification,<sup>15</sup> and that because of the typical pattern of weight gain following on from weight loss, they should set out to sign people up for life.)

### What about government?

In conclusion, two final issues arise concerning the government.

The first issue concerns government and the funding of research. The government is not being urged to undertake the approach commended here. Its own efforts at directing Australia's research activities are embarrassingly poor. The categories under which academics are invited to apply for

The government is not being urged to undertake the approach commended here. Its own efforts at directing Australia's research activities are embarrassingly poor.

funding—for example, when applying for ARC 'discovery grants'—read for the most part like something made up by the proprietor of a local grocer's shop.<sup>16</sup> Not only are they a source of humiliation to researchers when—after the manner described in the first section of this paper—they try to re-describe what they are doing to fit in with such ideas. But insofar as they have an influence on what Australian researchers actually undertake, their emphasis on specific Australian problems and on practical applications, would seem to be the primrose path to mediocrity. I do not think that there are many things that government should do. But one of the few is the provision of adequate funding for pure basic research in the sciences, humanities and social sciences. (If this were done, it

could then be a matter for rational assessment—by these institutions or, better, by private investors working with them—as to whether or not it would be sensible to try to compete for a prize of the sort that I have here described.)

Accountability for the use of public funding is important and researchers should obviously be willing to explain what they are doing, to face public criticism and to suffer the consequences if they can't give a good account of themselves. But the idea that government should be in the business of choosing the topics for academic research almost beggars belief.

What, however, it might be asked am I doing in this article, if—my brief comment above apart—I am not suggesting what the *government* should do? What place, someone might ask, does an article such as this have in a journal of public policy? In my view it is here quite properly, as it is concerned with how a public policy issue should be addressed in a free society, and in that context with what form *private* initiatives should best take. Should government become as small in its scope as there are good reasons for its being, public policy problems will not disappear. We will still be faced by questions about how best to conduct our activities that have a public dimension to them, and these questions will require the exploration of alternatives and debate about their character. Accordingly, the need for neither the CIS nor *Policy* should be expected to disappear, even if every single policy that has been advocated in them to date (insofar as they do not conflict) were to be implemented.

The author would like to thank Dan Klein, Alex Tabarrok, Lawrence Cram, David Norton and Steve Fuller for leads to material relevant to my argument, or for criticism. None of these people, however, are in any way responsible for the paper as it stands.