Research sponsorship

One of the most difficult issues currently affecting scientific research is the influence of research sponsors [1]. In recent years this has been brought to a head by a number of notorious cases in the biomedical research field [2], and this has led to many journals, especially medical ones, introducing strict rules for the declaration of conflicts of interest [3]. As a matter of fact, awareness of such potential conflicts has been gradually increasing for the last 50 years or so, as illustrated, for example, by interesting data collected by Gingras and Gosselin [4]. This seems to be no doubt about the reality of bias brought about by sponsorship. For example, Stelfox et al. studied a large number, about 70, of articles dealing with calcium channel blockers (used to treat high blood pressure): 96% of the authors of articles evaluating a drug favourably had financial links with a manufacturer of the drug; 60% of neutral articles had such links; 37% of unfavourable articles had them [5].

It is somewhat doubtful whether the formal obligation to declare financial interests (or their absence) has any more effect on authors' actions than, for example, the statutory health warnings printed on cigarette packets have on discouraging smokers. It is in any case impracticable for the editors of a journal to police these declarations. The Macmillan journal *Nature* has, in effect, adopted a policy of self-policing (rather like the "self-ticket-inspection" policy on many Swiss trains) by offering as a definition of declarable interest: "Any undeclared competing financial interests that could embarrass you were they to become publicly known after your work was published." Anyone with a significant interest could, I imagine, take good care to keep it secret if they so desired. It is indeed quite likely that the current trend to require these declarations represents a disservice to the overall work of scientists (of which dissemination is only a part). I cannot help thinking of an anecdote related by George Mikes: during a visit to Switzerland his wife wished to iron some clothes and, in a shop selling domestic electrical appliances, asked whether it was possible to rent an iron. It turned out that the shop only sold them, but the shopkeeper kindly offered to lend one gratis. Mrs Mikes was delighted and offered to leave a deposit, or at least her name and address—at which point her husband hastily led her out of the shop because it was clear from the look on the shopkeeper's face that she was beginning to suspect some criminal intent—these were obviously people to whom the *possibility* that an iron might be stolen had occured [6].

Somewhat reassuringly, there is growing debate and discussion about the matter. C.L. Berry has recently delivered an illuminating lecture, subsequently published [7], particularly examining the circumstance that knowledge production is typically context-dependent. In other words, the facts are one thing; their interpretation (according to which real knowledge is produced) is another. In notorious case, involving the firm Monsanto and the drug bovine somatotrophin, the firm not only claimed the rights over the raw data emerging from the study it had sponsored, but also over the analysis of the data, even if already in the public domain [8].

While sponsor influence (leading to bias; i.e., a departure from objectivity and, hence, from the scientific method) appears to be most prevalent in the medical and veterinary fields, it is by no means confined to them; obviously it covers all areas of scientific research, although, perhaps because of the emotional aspects of medicine, other areas are far less in the spotlight. Besides that, biomedical research constitutes a very significant proportion of the total research effort. It is highly distressing that the fairly recent Lambert Report [9], which specifically reviews business–university collaboration in the UK, makes no mention of the deleterious impacts of sponsorship. There is no escaping the fundamental bottom line, that "he who pays the piper calls the tune". Denman has expressed this in lapidary fashion: "Truth, as one saw it [in academic life], was outspoken and expectant of contradiction, confrontation, rebuttal, denunciation and criticism. Words were not trimmed nor ideas double-thought. Straight flung speech was never considered impolite. The professional world, on the contrary, appeared to confuse politeness with deference. The shopkeeper's code, the customer is always right, was the aphorism to work by. Should the principal or client wish to think that black is white, don't disillusion him—you might lose a fee! What the French call *prévenance* held precedence over a hammered out truth" [10]. Once academic independence is lost, there is no bulwark against an insidious erosion of values. Brown quotes the following [11]: "The moment of truth arrived for me [Peter Desbarats, former Dean of Journalism at the University of Western Ontario] in 1995, when Rogers Communications granted my request for $1 million to endow a chair of information studies, for which I was extremely grateful. When journalists asked me to comment on the subsequent Rogers takeover of McLean Hunter, all I could do was draw their attention to the donation. They understood right away that I had been, to

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express it crudely, bought. This had nothing to do with Rogers. I had begged for the money. It was given with no strings attached. It will serve a useful purpose. But unavoidably, I gave up something in return. No one should ever pretend, least of all university presidents, that this experience, multiplied many times and repeated over the years, doesn't damage universities in the long run".

Several writers have commented on the changing modes of research (i.e., knowledge production). Gibbons et al. refer to "Mode 1" (traditional "academic") and "Mode 2" (sponsored) [12]; Ziman to non- (or pre-) instrumental and instrumental [13]. Of course, the root problem is that research costs money. Even the researcher who needs only pencil and paper for his work needs shelter and sustenance. Historically, one notices the following progression: Mode I (e.g., Socrates, Cavendish, Joule, Rayleigh) is the private individual carrying out research at his own expense; sometimes (but all too rarely) the work is supported by private philanthropists in a more institutional fashion (e.g., Davy, Faraday and others at the Royal Institution in London). Mode II is the University that makes money from tuition (in the extreme case, research is only possible during the vacations). Mode III is represented by government institutes. In some countries (e.g., Great Britain, Switzerland) this was never of great importance except in specialized domains that often combined danger with strategic importance (e.g., atomic weapons at Aldermaston or rocket propulsion at Westcott); in others (e.g., France, Germany, Italy, the USSR) this was and in some cases still is the principal mode (CNRS, Max Planck institutes, CNR, Academy of Sciences institutes). Mode IV is research carried out by commercial industry, in some cases (e.g., at the former AT&T Bell Laboratories) in a manner comparable to, and with much better facilities than, traditional academic (Mode II) research (Spier [14] gives a good description of industrial research in contrast to academic). Note that there is some mixing between modes (e.g., nowadays the government heavily subsidizes most UK universities, even though they are private foundations; in continental Europe, universities are anyway mostly state institutions).

As well as their rôle in gathering funds from the general economic activity of the country through various taxes (income, corporation, sales etc.) and disbursing it to fund activities (e.g., research) that are considered to be beneficial for the country but not necessarily supported privately, there is also a consideration that, at least in the past, governments are considered to be guarantors of integrity, much as, for example, private telephone companies were nationalized in order to avoid the misuse of subscribers' conversations; a similar consideration underpins the provision of government postal services. Nevertheless, governments have their own political, if not commercial interests and these can potentially be influenced by lobbying. Indeed, the research and technical development programmes financed by the European Union are notoriously susceptible to such influence. Given that tendencially more and more public research funding comes in the form of a tightly controlled contracts to carry out specific pieces of research (usually through the medium of grants made by research councils, national science foundations etc.) there is clearly the possibility of a similar distortion of the traditional values of disinterestedness, impartiality and openness that is associated with commercially sponsored research. Some academics, such as Ziman [13] and Berdahl (quoted by Brown [11]), strongly assert that traditional academic research is effectively dead and cannot be revived. This effete hand-wringing has little to commend it. It is justified by the seemingly modern view that one cannot "turn the clock back", but actually abandonment of academic research ideals would turn the clock back much further to the era of myth and superstition. Perhaps we should abandon the term "academic research", which is anyway rather ill-defined. Nowadays it seems to have a somewhat pejorative connotation, probably because of the excessive production of what is, by any objective criterion, useless knowledge (with neither the grandeur of discovery nor the potential for improving man's estate). Incidentally, much of the output of research grant-funded work appears to be similarly useless (e.g., judging by the monthly digests from the European Union [15]), despite the fact that this mode of operating was expressly introduced to weed out useless activity. Rather, we should refer to research carried out in accordance with the scientific method (the objectivity of which requires the traditional values of disinterestedness, impartiality and openness). In this regard there is indeed no difference between "basic" and "applied" research, as A.M. Prokhorov has asserted. The contrast is between this scientific (objective) research and "research" in which some of the conditions are relaxed and the results of which are, as a result, unreliable [16].

J.D. Bernal has emphasized the huge returns (especially to society as a whole) from investment in research [17]. Undoubtedly the application of the scientific method led to the discovery of certain drugs, which appeared to be beneficial to patients, and which spawned a huge industry to manufacture and sell these drugs. Further research resulted in the subsequent discovery of deleterious effects of those drugs, at which point it would have been consequential to immediately withdraw them from medical practice. The fact that this is often not done, or strongly resisted, by what have

meanwhile become immense commercial interests, can be seen, metaphorically speaking, as a result of "friction" or "inertia" that prevents the overall system from evolving in a more beneficial fashion. If something is to be done, it should be directed towards the diminution of that friction. The complexity of today's situation, in which the interests of science and industry are sometimes seemingly inextricably entangled, is well illustrated by the fact that the provision of social services (e.g., pensions) to the staff of scientific research institutes is likely to depend on the financial well being of companies marketing drugs on the basis of unreliable knowledge. Ultimately, it is perhaps impossible ever to circumvent the maxim that "one cannot do research of real integrity if one has to depend on sponsors"; one can perhaps only hope to alleviate some of the worst effects of dependency. It is a corollary that the "funded project" so beloved of university administrators is a barbed hook, the extraction of which is likely to turn winning such a project into a Pyrrhic victory. The quantum computer springs to mind as an analogy: its reliable operation requires isolation from its environment. Independence and integrity can only be achieved if the funding is wholly decoupled from the research. But, given that some degree of entanglement seems to be unavoidable, it is surely worth exploring whether some kind of "error correction" can be applied to ensure that the output under such circumstances is objectively acceptable.

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References and notes

1. Spier, R.E. Ethics and the funding of research and development at universities. *Sci. Engng Ethics* **4** (1998) 375–384. See also the inadequate rebuttal by S.P. Nichols and C.M. Skooglund, *ibid*. **4** (1998) 385–390.
2. Healy, D. In the grip of the python: conflicts at the university–industry interface: a way forward? *Sci. Engng Ethics* **9** (2003) 59–71.
3. For example, the British Medical Journal asks authors to disclose three types of information: (1) Associations with commercial entities that provided support for the work reported in the submitted manuscript (the timeframe for disclosure in this section of the form is the lifespan of the work being reported); (2) Associations with commercial entities that could be viewed as having an interest in the general area of the submitted manuscript (in the 36 months before submission of the manuscript); (3) Non-financial associations that may be relevant or seen as relevant to the submitted manuscript.
4. Gingras, Y. and Gosselin, P.-M. The emergence and evolution of the expression "conflict of interests" in *Science*: a historical overview, 1880—2006. *Sci. Engng Ethics* **14** (2008) 337–343, especially Figure 1.
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13. Ziman, J.M. Non-instrumental roles of science. *Sci. Engng Ethics* **9** (2003) 17–27.
14. Spier, R.E. Ethical aspects of the university–industry interface. *Sci. Engng Ethics* **1** (1995) 151–162.
15. research*eu Results Supplement. A monthly publication of Unité CORDIS, Office des publications de l'Union européenne, Luxembourg.
16. Here we should include illnesses invented expressly to provide a market for a drug, as described by Brown [11], as well as the kind of distortion described by Healy [2].
17. Bernal, J.D. *The Social Function of Science*. London: Routledge and Kegan Paul (1939).

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