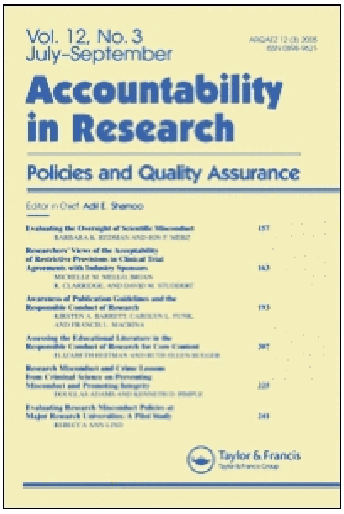

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Cost of the NSERC Science Grant Peer Review System Exceeds the Cost of Giving Every Qualified Researcher a Baseline Grant

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COST OF THE NSERC SCIENCE GRANT PEER REVIEW SYSTEM EXCEEDS THE COST OF GIVING EVERY QUALIFIED RESEARCHER A BASELINE GRANT

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Using Natural Science and Engineering Research Council Canada (NSERC) statistics, we show that the $40,000 (Canadian) cost of preparation for a grant application and rejection by peer review in 2007 exceeded that of giving every qualified investigator a direct baseline discovery grant of $30,000 (average grant). This means the Canadian Federal Government could institute direct grants for 100% of qualified applicants for the same money. We anticipate that the net result would be more and better research since more research would be conducted at the critical idea or discovery stage. Control of quality is assured through university hiring, promotion and tenure proceedings, journal reviews of submitted work, and the patent process, whose collective scrutiny far exceeds that of grant peer review. The greater efficiency in use of grant funds and increased innovation with baseline funding would provide a means of achieving the goals of the recent Canadian Value for Money and Accountability Review. We suggest that developing countries could leapfrog ahead by adopting from the start science grant systems that encourage innovation.

Keywords: *Discovery Grants Program, excellence, NSERC, peer review, positive feedback*

Introduction

Science in Canada is generally funded via the peer review system, in which unelected committees (NSERC, 2006, 2007a), consisting mostly of the people who are also in competition for the agency's available funds (Meikle et al., 1992; Meikle et al., 1995), judge the

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scientific merit and fix the budget of proposals submitted by other scientists. While certain categories of conflict of interest are avoided, historically the result has been, at least in the case of the former Medical Research Council of Canada (MRC), that 100% of committee members are funded, while a lower fraction (20% for MRC) of noncommittee applicants received funding (Poulin and Gordon, 2001). Other agencies, including Natural Science and Engineering Research Council Canada (NSERC), need to be studied in this regard. In the case of MRC, this situation may have been a systemic effect caused by a positive feedback loop (Milsum, 1968), in which committee members were selected using the primary criterion that they received MRC grants (Poulin and Gordon, 2001).

The Canadian Federal Government keeps at an arms length, "management distance" (Mitchell, 2006), from this process and granting agencies assiduously defend the sanctity of the peer review system and their ability to govern themselves. However, control by peer review makes no sense for the allocation of scarce resources in any environment conducive of innovation (Drucker, 1973; Nicholson et al., 2008). Sometimes, minor tinkering is done by the introduction of small grants intended to fund high-risk research (i.e., research looked at askance by peers), but in practice the peer review controls are the same in such programs, so that many novel ideas remain unfunded.

As Federal agencies have a substantial monopoly in funding, there is no alternative system with which to compare peer review, and the agencies are not prone to experiment significantly with "what works." Thus the present system of control through peer review has yet to be tested and, as such, is not itself the result of good science. This situation reduces critics of the granting system to "what if" thought experiments, which are never enacted. It also silences most critics of the system because they fear alienating the very peers who would be judging their grant applications. A private experiment that set up an alternative to peer review unfortunately did not include a control group (Braben, 2004).

Why Alternative Grant Funding Systems?

Why should the Canadian Federal Government, or any other government, be interested in alternative grant funding systems?

The answer lies in the possibility that peer review is wasteful of taxpayers' money. Proving this is difficult, for a number of reasons, especially because the current system muddles along and justifies itself by a string of scientific advances for which it takes credit, though there is of course no mention of the breakthroughs that didn't happen. Nevertheless, there are many hints that the grant funding system we have fails us on many counts:

1. Because of the stiff competition for grant funds, requiring development of a skill called *grantsmanship* (Lauffer, 1977; Glassford et al., 1980; Smith and McLean, 1988; Birenbaum, 1993; Laznicka, 1993), many scientists drop out unnecessarily. While they are but stereotypes, the personalities of the reclusive scholar and the outgoing salesperson are near opposites, and many good scientists do not compete well in this environment. A component of job stress in academia is "pressure . . . to obtain external funding" (Catano et al., 2007). If good talent is discouraged by an ineffective grant process, this is a considerable waste of potentially productive lives, and waste of Canadian taxpayers' money since post secondary training of scientists, especially at the graduate level, is heavily funded by the Federal Government.
2. Knowledge of this cutthroat competition in academia trickles down, and students choose other careers (OECD, 2007; Tibbetts, 2007). This worries demographers and programs are developed to encourage children to consider science as a career (Brainard, 2007; Robinson and Lewthwaite, 2007). Job prospects and working conditions are hardly mentioned. Unlike competitive sports, where also few make it to the top, science does not have minor leagues, neighborhood clubs, coaching opportunities, or weekend jousts with friends. One is either in or out, and except for a small cadre of science reporters, few can find fulfillment in science as a spectator sport. Those who fail to become scientists just do other things.
3. Highly trained technicians working for scientists have jobs only as long as grants last, and their salaries must often be pieced together from a few grant sources, each of which is inadequate. Thus there is a high turnover. The result is a constant emphasis on training of "high quality personnel" (HQP) (NSERC, 2007a),

for whom few steady jobs exist, and those technicians who become highly trained often leave academia.

4. Because tight grant systems and an individual scientist's unpredictable and fluctuating funding make it difficult to offer any kind of stability or job security to technicians, many university scientists are forced to use graduate students as cheap labor, demanding long hours, years of work, and responsibilities not directly related to their studies to keep laboratories running. For example, a Master's student stipend of $16,500 (Canadian) per year for a de facto 60 hour work week or more, amounts to less than $5.30/hr, below minimum wage, for a period of 2–3 years, frequently after 1–2 years of pre-Master's studies. Getting a Ph.D. is now 2–4 years typical additional duration, with about $5,000 larger stipend. These low wages, for a post-undergraduate period that has grown (Elgar, 2003) up to 9 years, occur while many graduate students are also starting families, and impact on their children and spouse. This wastes years of productivity for those who remain in the system by forcing them to be apprentices for long periods rather than beginning their own careers as scientists. Replacing highly paid and skilled technicians with graduate students also reduces productivity of the laboratory because each student must be trained from scratch. Being a postdoctoral student just prolongs the apprenticeship period even more, sometimes indefinitely (Smith, 1997; Vella et al., 2004).
5. The shortage of scientists and shortage of Canadians willing to become science graduate students under these conditions has international repercussions, enhancing the brain drain both from developing countries and lesser endowed developed countries (Kapur, 2005) to fill empty graduate student placements.
6. Each scientist tends to follow their own initiatives or do their own thing regarding research, so grant competitions are really about competing worthiness of distinct goals, rather than between people trying to attain the same specific goal (as in sports or business). Thus when a grant application is not funded, the expertise that drove it, the questions it addresses, and the possible breakthroughs it offers are often lost.
7. Peer review discourages innovation, for a simple reason. While constructive criticism is necessary at some point, it is not appropriate when a scientist is embarking on something entirely new.

In this circumstance, a scientist's own confidence in success is or should be low if they are honest with themselves. If they can't convince themselves of the certainty of early success, why should they be expected to convince their peers? The history of science is replete with example after example of major breakthroughs occurring in the face of the harshest peer reviews, see Pasteur's fermentation theory (Gordon, 1999), continental drift/plate tectonics (Hughes, 1994), bacterial cause of ulcers (Gozlan, 2005), etc. These are undoubtedly the tip of an iceberg of other nascent breakthroughs that simply do not see the light of day. The ability of a scientist to prevail despite the resistance of peers is not a measure of the worth of the ideas, just of tenacity and alternative opportunities. Lacking these, ideas die, and society loses.

8. The temptation to fake results when one's career is at stake is high. While fraud in science comes to public attention once in a while (Lewin, 1989; Root-Bernstein, 1989; Garfield and Welljams-Dorof, 1990; Catano and Turk, 2007), this must be considered but the tip of another, darker iceberg (Munro, 2008; Titus et al., 2008). Under the current system, degrees of shading the truth become acceptable as a way to get an edge on the competition. Examples are: inflating one's students' evaluations to give them a better chance of getting fellowships, rejecting data that doesn't fit preconceptions as statistical outliers, publishing the same material many times or minor variants to increase one's publication list, using money from a grant for other research, especially the generally unfundable "preliminary research," skimming percentages of grants by university administrators as overheads, tailoring one's results to the expectations of the sponsor (especially if commercial), and assigning graduate students jobs that should be done by technicians or administrative staff, not because the student really needs to know how to do the job but because the salary of the technician or administrator was not funded. This mode of behavior is inimical to searching for truth (Gordon, 1993). Fraud also occurs because failure is not an option, yet research is inherently risky, and failure needs to be expected and tolerated. In some cases, such as the "magic bullet" approach to cancer (Galanski and Keppler, 2007), success is only hoped for after an expected long string of failures.

9. The kind of science done under grant competitions is short term. As in politics, the horizon for funding is at most 5 years, whereas major unsolved scientific problems usually require decades or even a lifetime of commitment. The latter is recognized in the institution of academic tenure but no money backs it up, except for salary (at least in Canada).
10. Teaching in universities suffers, because advancement for academic scientists (promotion and the attainment of tenure) is predicated mostly on their ability to get grants, despite lip service paid to the "research, teaching, and service" balance. Some grants deliberately pay salary so that scientists can opt out of teaching, depriving all but a few of their own students of access to them. Whether these factors negate the advantages of a combined research and teaching environment (Pouris, 2007; AUCC, 2008) is open for discussion.
11. Because so much emphasis is placed on success in getting grants, de facto universities have abrogated their responsibilities in promotion and tenure of scientists to the grant agencies, whose criteria do not include performance in classroom teaching or public service.
12. Women scientists are often forced by the competition and societal expectations to choose between having a family and having a science career. Those who choose family first and attempt to re-enter the fray later find themselves at severe disadvantages in landing tenure-track jobs, attaining competitive salary rates, having success in getting grants in competition with those who already hold grants, and in terms of pensions. Their expertise is, therefore, lost to society.
13. In general, the peers who do the reviewing, called "referees" or "reviewers," are not paid for their services. At best, they are compensated for their travel expenses. While scientists are reviewing others' work, they are not producing their own. There is another more insidious effect. The reviewing of others' proposals is regarded as an honor, but with hundreds of proposals to review each year, there is a large degree of reviewer burnout (Office of Inspector General, 2006). The result is that the science that is approved is judged by a relatively small self-sacrificing group who seek nonmonetary rewards, such as access to the halls of political power in science. However, this

access generally, albeit indirectly, also guarantees their own grant funding (Poulin and Gordon, 2001), recreating what used to be called the "old boys club."

14. The paperwork needed to write a grant application has increased substantially, from a single page when the system started mid-twentieth century, to massive tomes that take months to prepare. Recently, reviewer burnout has trimmed this back a bit, but the detail examined has been expanded to include: students' grades, the career successes of one's former students, one's role on each publication, self-evaluation of the impact of one's work, etc., making each application a monumental effort of self-promotion.
15. Letters of intent have changed from short notices allowing agencies to convene appropriate peer review committees, to major applications for the right to apply for a grant, nearly doubling the researchers' efforts. Rejection can also occur beforehand, in house, through internal review, as universities often only permit grant applications to go out that local committees think will compete well and look better, since each university's grant success rate is published. The number of layers of review, and the effort devoted to applications, has thus expanded to typically include: a) informal review by one's friends, b) internal review and assistance by the Research Office of one's university, c) preparation of a letter of intent including social relevance, d) the grant application itself, and e) an "Inter-agency Funding Mechanism" for interdisciplinary proposals (NSERC, 2007b).
16. Many scientists give up, especially after attaining tenure. They are then salaried, but without the resources to do their own scientific work. Universities provide only small amounts that may be used for research ($1,400/yr at the University of Manitoba, for example). In other words, while "teaching, research, and service" are expected of professors, research is not paid for by universities, nor, therefore, by transfer of payments between the Federal Government and the Provinces. The fraction of funds spent on salaries that is designated for research could be regarded as wasted if it were not that some faculty members are so self-motivated and dedicated that they subsidize their research through their own private resources. Universities and the Federal Government formally recognize this situation

via the mechanism of tax-deductible self-funded grants from one's salary.

17. In the past, universities provided secretarial and shop services whose costs have been shifted onto scientists and their grants. Even postage stamps and phone charges now have to come out of grant funding. University funding cutbacks have reduced or eliminated deans' slush funds that in the past could be used to provide bridge funding, i.e., funding of novel ideas in their early stages. We thus now have a hidden cost, namely, the delay between the inception of an idea and finding the cash to test it (if ever).
18. There are conflicts between the grant system and the patent system. Science thrives on open discourse, as each scientist's ideas inspire new ideas for others. This is what scientific conferences are mostly about. When scientific ideas become more intellectual property than public good, mouths become closed, and scientific advancement slows. As the presentation of patentable ideas in grant applications creates opportunities for theft of those ideas, they are usually held back. Students are prevented from publishing and also lose out when evaluated against their publishing peers.
19. Universities tend to value their scientists in proportion to the amount of money they bring in. This can be seen just by reading the dollar headlines in university public relations newspapers and press releases. This undermines those scientists who are working at the frontiers of their fields, which are generally lonely unfunded territories. It also reduces the perceived value of scholars in nonscientific fields.
20. Even well-funded scientists can fear doing something new, because they will be seen as doing something other than what they were funded to do, or because they will be judged by their peers as inexperienced or incompetent in a new field they wish to enter. This tends to lock scientists into narrow paths that they themselves have well worn, reducing the adventure, innovation, and scope of their discovery. The cure is often touted to be interdisciplinary work, but without funded interdisciplinarians who can cross many boundaries, real communication and innovation between scientists in narrow disciplines is impeded.
21. The noise in the reviewing process is so high that "wrong" decisions on who to fund are made with unacceptable

frequency (Cole and Stephen, 1981; Cole et al., 1981; Humphreys et al., 1982; Hodgson, 1997; Mayo et al., 2006). In other words, the variance in reviewing decisions is so high that ranking of proposals is highly random and arbitrary. This matters because the sharp cutoff system for who gets funded arbitrarily causes drastic consequences to careers.

The peer review system in science may have been responsible, by stifling research creativity, for the decline of global per capita growth per annum from 2.8% during the "Golden Age" (1951 to 1974) to half of that subsequently (Braben, 2004).

One might conclude that what progress in science occurs, occurs in spite of, rather than because of, the grant peer review system. The net result is that the Federal Government, and its taxpayers, receive far less bang for the buck than they otherwise would, a point reflected in the "Value for Money and Accountability Review" (Mitchell, 2006). In the international competition for novel products and markets, and for environmentally sustainable development, for which science is the driving engine, the country that does not examine peer review critically may be putting itself at a severe disadvantage.

Origin of Peer Review of Grants

The peer review system for grants probably grew out of the peer review system for publications (Benos et al., 2007). In the latter, a scientist submits a paper to a specific journal, whose editor chooses a few people to read the manuscript and judge if it should be rejected, accepted as is, or accepted with modification, usually within a few months. While this dialogue is usually anonymous and, therefore, subject to the interjection of irrelevant criteria and some amount of idea stealing (as are grant applications), there is one major escape valve: there are many journals to choose from in each field, and one can even start a new journal with a few colleagues if frustrated with one's treatment or with what is available. Journals based in academic societies or commercial publishers often compete with one another, frequently soliciting submissions, especially nowadays by e-mail. However, grant agencies are much fewer in number than the more than 100,000 scientific journals (de Solla Price, 1975) by orders of magnitude, closer to monopoly status, and most have annual deadlines, so that failure to receive funding can have repercussions for years, including interrupted work, loss of students and technicians, and driving a scientist out of the system.

An important distinction between journals and grants is that journal peer review at most delays the reporting of innovation while grant peer review suppresses innovation itself.

Peer review of grant applications is sometimes compared to democracy: "Many forms of Government have been tried and will be tried in this world of sin and woe. No one pretends that democracy is perfect or all-wise. Indeed, it has been said that democracy is the worst form of government except all those other forms that have been tried from time to time" (Churchill, 1947). But it is not democratic. There is no voters list, no elected representatives, no issues voted on, and no referenda. While scientists obtain academic degrees like the professions, unlike the professions, scientists and the institutions that train them are subject to no outside certification process. Most scientists fear challenging the grant peer review system, worried that this will affect their ability to get grants: don't bite the hand that feeds you. Getting a grant has become more important to many scientists than seeking new knowledge.

Alternatives

Two major models for alternatives to the present peer reviewed grant competition system for funding scientific research have been proposed:

1. Sliding scale funding, in which on renewal each grant goes up or down by a bounded factor from its present funding level (Forsdyke, 1989, 2000; Berezin et al., 2001). Since the peer reviewers do not have the option of cutting funding to zero, this buffers each scientist against them. (We will not consider this model further, since its administrative costs would be similar to the current system.)
2. Baseline funding, in which each researcher gets the same amount per year (Gordon, 1993; Poulin and Gordon, 2001). This protects each scientist from peer review. It is open to the objection that some scientists will do nothing useful with the money, and so it needs some modest checks and balances,

such as requiring a public accounting of what research was accomplished. It also underfunds expensive research.

These are not monolithic proposals. For example, in one scenario we recommended:

1. 40% of an agency's budget be distributed evenly as baseline funding;
2. 50% be available for regular peer reviewed competition, for projects requiring more funds than baseline;
3. 10% be available for industry collaborations (Poulin and Gordon, 2001).

This is a "mixed portfolio" of high risk, medium risk, and low risk investments. And, after all, it is investment in science that the Federal Government is doing in the annual budgets for granting agencies (Gordon and Poulin, 1999; Poulin and Gordon, 2001). The present emphasis on commercialization of university research (Hawkins et al., 2006; Langford et al., 2006) ignores the fact that all research starts from an unfunded and (by present criteria) usually unfundable idea. The mixed portfolio approach alleviates the objection "that the best researchers are left relatively underfunded in order that other good researchers can receive at least modest funding" (Mitchell, 2006). (That objection is also fallacious, in assuming that we can distinguish "best" from "good" before doling out the cash.)

NSERC Bucks the Trend

The above proposals for alternative funding schemes were made at a time that most grant agencies reported decreasing "success rate" (Tornow, 2007), i.e., fraction of grant applications that received any money at all, declining to as low as 16% (Table 1). Canada's NSERC (see Natural Sciences and Engineering Research Council Canada, http://www.nserc.ca/) has bucked this trend, retaining at least a 70% success rate, though historically the rate has been as high as 83% (Table 2).

NSERC has been under pressure from its external reviewers out of a concern to be "consistent with international standards of quality . . . . Are best researchers supported at an internationally

TABLE 1 Grant application success rates

Agency	Success rate	Year	Average annual grant	Source
NSERC	70.2	2007	$30,000	(NSERC, 2007a)
CIHR	16.3	2005	$109,700	(CIHR, 2007)
SHHRC	33.1	2008	$28,245	(Taylor and Yasmeen, 2008)
US NIH (R01)	16.3	2006	$359,030	(NIH, 2006a,b)
US NSF	20	2005	$140,000	(Tornow, 2006, 2007)
UK MRC	25	2006		(Tornow, 2007)

Note: NSERC = Natural Sciences & Engineering Research Council, Canada; CIHR = Canadian Institutes of Health Research; SSHRC = Social Sciences and Humanities Research Council, Canada; US NIH (R01) = United States National Institutes of Health Research Project Grant Program; US NSF = United States National Science Foundation; UK MRC = United Kingdom Medical Research Council.

TABLE 2 NSERC discovery grant trends (NSERC, 2005, 2007a)

Year	No. of applications	Success rate	No. funded	Average grant/yr
1995	2557	68.5%	1752	$28,704
1996	3043	76.4	2326	$28,386
1997	2777	76.3	2119	$28,308
1998	2685	74.1	1990	$32,345
1999	2645	71.5	1892	$32,267
2000	2963	71.2	2111	$32,080
2001	3089	79.7	2461	$37,578
2002	2907	83.4	2424	$37,489
2003	2970	79.7	2367	$31,904
2004	3014	75.3	2270	$32,210
2005	3201	74.9	2398	$32,054
2006	3258	72.9	2375	$30,811
2007	3592	70.2	2522	$29,443

competitive level?" (Mitchell, 2006). The pressure is to conform to the norm of much lower success rates, and NSERC "is convening an International Review Committee" to examine these issues (Marchildon, 2007; NSERC, 2007a). Table 2 indicates that the NSERC trend is downwards. Fortunately, the subsequent review concluded that because ". . . the top 10%" of grant recipients "comprised nearly 21% of total funds", NSERC ". . . has generally supported Canada's best researchers at an internationally competitive level" (Nicholson et al., 2008). Of course, this statement also assumes that the best funded are the best scientists. The "proof" of this is that "the top one percent of Discovery Grant Program (DGP) grant holders has total funding from *all* sources that averages almost $350,000 annually." In other words, the same assumption as needs to be proven was made: money measures worth.

Excellence

The word "excellence" has come to permeate Canadian grant agencies, to the point where it has become a meaningless incantation (Fig. 1). Excellence is defined ultimately as success in getting grants. Excellent scientists, with excellent students, are chosen by excellent peer reviewers, and, when money is tight, only the most excellent of the excellent get funded. Grant agencies produce lists of "excellent scientific journals," which are the journals that their excellent peer review panel members publish in. The historical fact that much seminal work had to be published first in less popular journals questions this line of thought. Much of what is

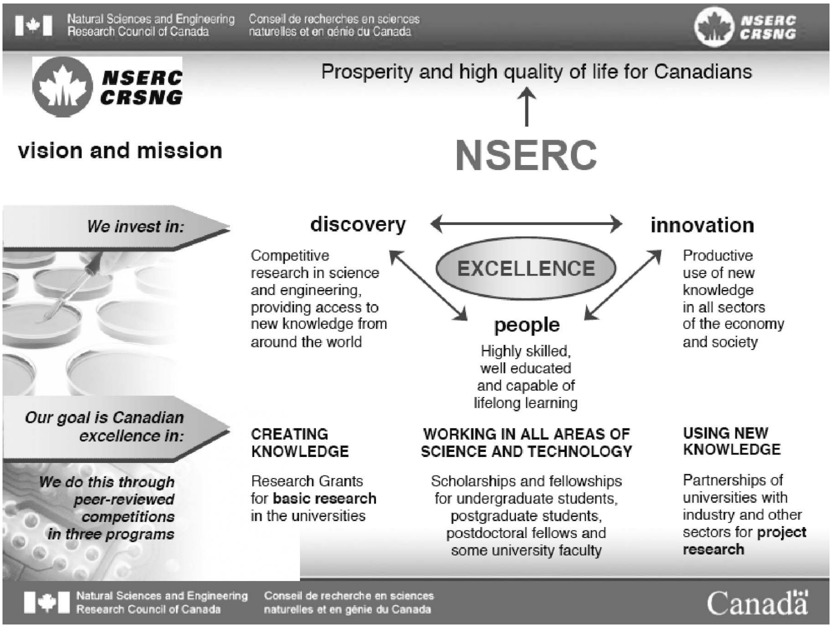

FIGURE 1 "Excellence" as a presumed center for NSERC activities. (NSERC, 2007a).

deemed excellent is merely popular or the latest fad, while many deep unsolved problems go unfunded.

An important example of the failure to recognize real excellence is the discovery of a chemical reaction that creates spatial and temporal patterns (Belousov, 1958), whose publication was almost completely suppressed by peer review (Winfree, 1984). The theory of these reaction-diffusion chemical systems was developed by another science outlier (Turing, 1952; Hodges, 1983; Leavitt, 2006), and both theory and experiment have since generated a vast literature.

There is nothing deliberate in all of this; no conspiracy theory applies. The crescendo of excellence verbiage is a result of a positive feedback system, like grade inflation in college courses. No individuals are responsible. By the same token, no individual within the system can stop the process. However, the Federal Government could.

There may be an analogy between this culture of "excellence" and the one dimensional nature of the extremely competitive Cambridge University Tripos mathematics exam that developed in the 1800s "a new system of assessing and ranking academic ability . . .":

> "The coaches and examiners had clearly been successful in establishing what was widely regarded as an objective scale of intellectual merit. Neither Galton nor any of his similarly disaffected contemporaries challenged the fairness of the Tripos examination, but they learned, by painful experience, that this was a system that celebrated a handful of winners—especially the 'upper ten' wranglers—while remaining largely indifferent to the fate of the rest" (Warwick, 1998).

Mathematics, like science and medicine, is a multifaceted, almost fractal web of ideas and relationships, with people following its many paths into the unknown. A one-dimensional ranking of these scholars, with sharp and severe funding and opportunity cutoffs, leaves many of these universes unexplored.

The journal system, being vast and multifaceted, like knowledge itself, creates many opportunities for publication, and in its migration to the Internet, is becoming an even greater leveler, because it creates equal exposure for popular and obscure journals. In a sense, there is now but one journal, the scientific literature, recorded and accessible via the World Wide Web. Experiments in open publishing are being made, including journals where ideas may be published initially unreviewed, but anyone may append public discussions to each article. The honing and refinement of ideas permitted by this open exchange is far more likely to bring out true excellence than closed peer review committees passing judgment on individuals, and increases democracy in science (Pharaon, 2007). Facile statements, such as "Suffice it to say that . . . the best researchers are . . . the most productive . . ." and "peer review is the key mechanism for ensuring research excellence" are made despite the observation in the same document that: "The challenge here—long recognized by practicing scientists and performance measurement experts alike—is that few direct and quantifiable measures of the larger and longer-term results of research are available" (Mitchell, 2006). Indeed, history ultimately judges the real value of each scientist's work, not peer review.

Costs

For the Faculty of Medicine at the University of Manitoba, if no one had to write grant proposals, the productivity of its researchers might rise 50% (Gordon, 1996), because an increased number of qualified researchers would be involved, and they would have more time to conduct actual research.

The costs of the peer review system in NSERC will now be approximated, and compared to costs of 100% funding of grant applications, assuming the same total budget so that this does not become a plea to Government for more money for scientists (warranted as that may be). Costs are based on the following parameters:

1. Average salary of an academic scientist (excluding medical/ dental, "All ranks combined," weighted by numbers): $89,806 per year in 2003–2004 (Statistics Canada, 2004), rounded to $90,000 per year.
2. Workweek based on Canadian university official norms: 35 hours of 'formal work' for 50 weeks per year.
3. Average annual cost of research: $30,000 per year, per NSERC 2007 granting practice (NSERC, 2007a).

Thus time spent is valued at $51.32 per hour, rounded to $50/hr. For students and technicians, we take a mean cost of their time as $20 per hour, based on a weighted mean salary of $35,000/yr. Peer review committees are assumed to have 13 academic members

(NSERC, 2007a). NSERC had 25 Grant Selection Committees in 2007. While no statistics are kept, and the practices of these Grant Selection Committees are heterogeneous, up to 200 hours each, or more, per year are spent by their members reviewing the dominant Discovery Grant applications (NSERC staff, personal communication), brought down by us to 100 hours each, so as to be cautious about overestimation. Administrative costs are taken as the fraction of the Discovery Grant budget ($316,200,000 in 2007/8, which includes multiyear grants) to the total budget ($920,000,000) times NSERC's total administrative budget of $43,000,000 (Table 3). In order to not "mix oranges and apples," the analysis and conclusions will not include or be applied to the other programs run by NSERC.

Elimination of the present peer review system would result in savings that could be applied to fund additional research projects. Stated another way, the cost of the present peer review system is about rejecting 30% of applications, since successful applications would be treated the same way under both systems (i.e., peer review vs. no peer review at the idea or discovery stage). The success rate for NSERC applicants for Discovery Grants in 2007

TABLE 3 Estimated costs of discovery grant peer review at NSERC, 2007 (NSERC, 2007a). The number of people is based on table 2. Hours and salaries are estimated in the text. The number of hours per person is based on our personal experience and personal communications from NSERC staff. We assume two students or technicians helping per grant application, and two internal reviewers.

Item	No. of hours per person	Cost/hr	Cost per person	No. of people	Item cost
Professors writing applications	120	$50	$6000	3592	$21,552,000
Students and technicians time assisting with applications	40	$20	$800	7184	$5,747,200
Internal review	2	$50	$100	7184	$718,400
Committee review	100	$50	$5000	325	$1,625,000
Administrative costs					$14,778,913
Total					$44,421,513

was 70.2%. Thus the number of people not funded was (1 – 70.2/ 100 × 3592 = 1070. From Table 3, the cost of preparing and rejecting applications was $44,421,513/1070 = $41,515 each, rounded down to $40,000 per grant rejected. While one may tinker with the estimates used here, it seems a robust result that the cost of rejecting a Discovery Grant application ($40,000) equals or exceeds the cost of giving it ($30,000).

This result, namely, cost of rejection by peer review exceeding acceptance without peer review, does not automatically apply to other grant agencies with lower "success" rates. "In short, the strength of this argument depends on high funding rates and small grant sizes" (reviewer). Our argument is that high funding rates are indeed justified because of the 221 reasons given above. The "mixed portfolio" approach to overall funding counters the implication that we are proposing that all grants should be small. We are arguing that the distribution should peak at many small grants and fewer large ones, so that all eligible scientists have a chance of seeing their ideas, if they pan out, through the embryonic stages. Equal baseline grants to all eligible applicants, plus competition for a second pot of money, accomplishes this distribution. The low success rate of other grant agencies may be doing society a disservice.

Without peer review of baseline research funding, we would not have the current scrambling of NSERC to rearrange its committees to keep up with emergent fields that current committees suppress (Sedra, 2008).

Observations and Advice

We observe that it would be cheaper for the Federal Government to send each professor of science or engineering a direct grant for $30,000 per year, and leave judgment of who is qualified to do the research to the universities (through hiring, promotion, and tenure), than to distribute the money budgeted by NSERC for Discovery Grants through this agency. The rejections are costing over $40,000 each, plus the denigration and loss of scientific careers, and ultimately loss of new recruits to science, compared to $30,000 each for a direct baseline grant. Eligibility rules are already in place, which in effect would place a cap on expected expenditures on baseline funding:

"... [Y]ou must hold or have a firm offer of an academic appointment at an eligible Canadian university [in the List of Eligible Institutions (NSERC, 2008c)] at the time of application and during tenure of the award .... You must engage in research in the natural sciences or engineering" (NSERC, 2008b).

If more people who are eligible applied, this would either require an increase in the total budget or a decrease in the amount per grant. But either way the country would gain in scientific productivity since more new ideas and discoveries would be pursued. We may get a rough estimate of the number of eligible NSERC applicants by combining the number of faculty members in mathematics, the physical sciences, engineering and applied sciences, agriculture and biological sciences, which came to approximately 9,000 in 2006 in Canada (AUCC, 2007). Of these 9,000 potential grant applicants, only 3,258 applied to NSERC in 2006 (Table 2). If all of them were given baseline grants of $30,000 per year, the cost would be $270 million per year, or 11% of the Federal research budget (AUCC, 2008). As the present NSERC budget for Discovery Grants is $318.4 million (NSERC, 2008a), not counting special programs, baseline grants for all is an achievable goal, and in fact could even be raised to $35,000 per year.

We suggest that the best policy is to support all of the eligible faculty members, who ask for funds, with baseline grants. All of these people have passed hurdles of hiring, promotion, and tenure hearings. We claim that there are no effective criteria that will distinguish who will produce important and lasting research. Even the author of one elitist experiment in funding admitted that:

"Outstanding ideas do not grow on trees, and at any one time, very few scientists would expect to be working on one. That would be true most of the time even for the best scientists at the most prestigious institutes anywhere. Nevertheless, any competent and determined researcher might expect to have an inspirational idea at least once during the course of a lifetime" (Braben, 2004).

That is the negative view. The positive view is that every scientist, given freedom from peer review at the conception stages of his or her ideas, may make a lasting contribution to humanity. If they are trained and employed to do so, why create impediments? We just waste our investment and trust in them. Furthermore, we have no tools to predict which of them will actually produce the breakthroughs. The mixed portfolio approach permits us to take advantage of the breakthroughs when they occur.

Controls on appropriateness of expenditures would be achieved through standard university accounting procedures and a return to the one page summary of research proposed, with another one page summary of results, together with links to data, analysis, patents, and publications for those who are interested in the details. These could be publicly available and required on a Web site, as a condition of continued funding. This is more to keep a record of what was done than a means of control per se. The far greater control of quality of researchers is assured through university hiring, promotion, and tenure proceedings, whose scrutiny far exceeds that of peer review. The greater efficiency in use of grant funds and increased innovation with baseline funding could achieve the goals of the present Value for Money and Accountability Review (Mitchell, 2006).

Though there are those who may object, we reject tinkering with a research system that is as flawed in concept as peer review at the initial idea stage. Either review ideas, as with the present system, or accept that talented, well-trained, and vetted researchers are best able to decide their own research priorities, at least at the idea/discovery stage. The question is really about centralization and control vs. decentralization and democracy, a question dwelt at some length in classic management literature (Drucker, 1973). We do not suggest scrapping the peer review system, but to reserve it for later stages of research, and use NSERC as a test case for scrapping peer review at the idea or discovery stage. This seems a good test toward much greater innovation at little or no cost, since only NSERC is close (at 70%) to what we see as the ideal: all (100%) research ideas by qualified researchers receive baseline funding.

The advantages of baseline grants are as follows:

1. In no other profession than science is one given a salary and space and asked to beg outsiders (who are not customers) for the means to do one's work. This anomaly makes being a scientist unattractive to young people.
2. Administrative costs can be reduced or eliminated. Most universities already give token amounts of research funds to their scientists, so these internal accounts need only be supplemented, using administrative structures already in place. NSERC could

retain the power to decide who qualifies to receive funding, which would keep the distribution of funds immune to local university politics and "taxation."
3. Controls on expenditures, to make sure they are appropriate, are already in place through the purchasing departments of universities.
4. If we trust scientists, who are carefully selected already through the hurdles of hiring, promotion, and tenure, they will attain a degree of independence of thought from their peers that could lead to much greater innovation. Quality control is exercised in the products of their work, through the peer review system of scientific journals, and through the patent submission and evaluation system if the work reaches commercial value.
5. If the baseline grants are allowed to carry over from one fiscal year to the next, and scientists are allowed to pool their funds, ambitious projects could be carried off with the same degree of independence. "This freedom allows the researcher to respond to events—events that of course will arise unpredictably in a genuinely exploratory initiative—and [doubles] . . . the relative value of the funds" (Braben, 2004).
6. Patentable ideas can be kept secret until patent protection is obtained.

Similar principles apply to both science and business, including: 1) being fair to all parties; 2) equitably offering opportunities and rewards; 3) making decisions that protect legitimate human rights; and 4) looking to long term legacy rather than popularity. There are unexpected bonuses that often accompany doing the right things. Inequity may be only the tip of another iceberg of the ethical inefficacy of the present peer review system, which, like bad business, may be accused of considering only short term gains. One result of short term thinking is that " . . . ethical misconduct has become a major concern in business today" (Ferrell et al., 2008). Ethical failures reported by employees occurred most often among senior managers (with a quite astounding 77% alleged misconduct rate) as compared with a high 67% and 56% rate among middle and first line managers, respectively (Ethics Resource Center, 2006). In science, the misconduct rate for ". . . 155,000 researchers suggests there could be, minimally, 2,325 possible research misconduct observations in a year" (Titus et al., 2008). If we distribute this over, say, 30 year careers, this comes to a guesstimated misconduct rate amongst scientists of 45%.

Ethical failures are almost always serious, since proper, acceptable behavior is needed for any culture of trust. For example, ethical failures in business have resulted in lower investor loyalty and trust, lower customer satisfaction, lower employee commitment and trust (Ferrell et al., 2008), and lower productivity. This lends credence to the hypothesis of a major negative impact of the science peer review system on the global economy (Braben, 2004).

Grant Systems for Developing Countries

As developing countries make their way from poverty to higher levels of prosperity, they find themselves wanting to partake in the world culture of science (Goonatilake, 1984; Varmus, 2008). At some point, initially for addressing practical, local, urgent problems, they begin funding scientific research within their borders. They look to the granting systems adopted by the countries leading in scientific research, and start to emulate them. Yet those systems are trapped in their own dysfunctional behaviors, and perhaps had best not be copied. We would suggest, in fact, that developing countries could leapfrog ahead by adopting systems from the start that encourage innovation.

Giving Up Control

The central problem is for scientists to let go control of other scientists, or for this control to be wrested from them by the Federal Government. Scientists have to allow their peers to do their own thing and give up the control and judgment of peer review of grants. It is counterproductive and does not accomplish the aims for which it is designed. Perhaps these "recommendations go counter to too many vested interests in the system as it has evolved" (Klaus E. Rieckhoff, personal communication). But there is already enough control and accountability in the system through hiring, promotion, and tenure hearings, and university financial accounting systems, that every professor is subject to. Granting agencies are superfluous middlemen for ordinary grants such as NSERC's Discovery Grants. These attempts to guide the uncontrollable nature of discovery in science end up suppressing discovery and innovation. Many advances come from lone scientists off in the intellectual frontier, who will not be appreciated or understood by peers until they have a chance to test and prove or disprove their ideas. That takes distributing the available money equitably, a willingness to take the risks this entails, and an understanding that the benefits of unfettered research far exceed those risks.

But the system of peer review of grants is too entrenched for the scientists to disband it themselves. Thus the Federal Government, which supplies the funds, has to consider stepping in and altering the system itself. In doing so, the Federal Government must face and overcome its own Catch 22:

> "Being generalists [Government politicians] rely on 'experts' for advice (e.g., when there is a SARS epidemic). But experts are largely defined by peer-review as currently practiced. If the peer-review system is defective, then the advice given to government is likely to be defective. Worse still, the peer-review winners are, more often than not, themselves highly skilled in politics. So they will tend to tell the politicians what they want to hear, rather than the truth!" (Donald R. Forsdyke, personal communication).

A similar problem occurs in the relationship of business to venture capital, analogous to that of scientists to grant agencies:

(a) ". . . 'start-ups' are effectively denied access to the primary institutional sources of equity capital" (Fletcher, 1992).
(b) "A Darwinian selection from a broadly seeded culture would do better at encouraging diversity but it leaves out the 'need' to support vested interests and the large group of dependents the current system keeps in bread and peanut butter" (Robert Bender, personal communication).
(c) "The freedom from government micromanagement that John Polanyi (Polanyi, 2007) seeks for scientists is the same freedom long sought by entrepreneurs, investors, and traders. Why don't well-intentioned scientific and business communities, and government make common cause on this issue? Such business and scientist leaders should be well received in Ottawa by conservatives and liberals alike: all who value freedom of enterprise, be it scientific or economic" (Manning, 2007).
(d) Elimination of peer review at the idea/discovery stage of the innovation process would eliminate the need for 'counterexample' such as ". . . the U.S. DARPA program (that) uses a modified

peer review that specifically seeks out ideas that 'won't work' (and, in fact may be impossible—quotations added for clarity). That program resulted in many successes, including what we now know as the internet" (reviewer). This seems to make our point exactly. Elimination of peer review at the early stages means that we need not introduce yet another review, this time by counter-reviewers to reverse the decision of the first reviewers.

In summary, we recommend that fair minded and practical government, business, and science agents work together to conduct a 'natural experiment' to test our hypothesis: innovation will be increased by the elimination of peer review at the idea/discovery stage. Here the NSERC Discovery Grants program could be the test case, with the past experience with this program used as the control case (Yin, 1989). There should be little objection on scientific or business grounds, since costs are comparable and the risks are no greater and most likely less. We anticipate that the elimination of the shadow of peer review at idea formulation stages, together with the advantages of fiscal carryover rights and freedom to combine funds with colleagues, would permit Canadian scientists to create and take advantage of many new opportunities and greatly increase their innovation.

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