

How to Organize Science Funding: The New Canadian Institutes of Health Research (CIHR), an Opportunity to Increase Innovation

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Why are Bell, 3M and the intramural program at NIH (US National Institutes of Health) so successful at inspiring innovation? How does MRC (Medical Research Council of Canada) compare and conflict with innovation in these examples? Will the existing and proposed structure and culture of CIHR (replacing MRC) produce relatively little innovation from the outset? Our examination leads to recommendations for an alternative funding system and organizational structure for CIHR: minimal structure with baseline funding at the initial, idea stage (40% of budget); more formal structure and competitive funding at the feasibility stage (50%); and matching industrial grants for the commercialization stage (10%). Our alternative CIHR budget would permit baseline grants of approximately \$20,000/year for each of 10,000 qualified medical investigators, 8,000 more than presently funded.

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INTRODUCTION

Science and innovation are generally considered to be engines of a nation's economic and social development (e.g., Drucker, 1986; Cole, 1995). One response to this awareness is to throw money at the scientific community without giving much thought to the fragility of the links between research, resources, structure and the hoped-for economic and social advances.

Another response is to restructure, in recognition of the very large differences in effectiveness that exist between different organizations within the same industry (Rumelt, 1991). Ghemawat (1999) estimates that the differences in performance are larger within industries than they are between industries, by a ratio of two to one. The implication is that top-performing organizations are somehow structured differently than poorer performing ones. The idea of restructuring is to bring the latter into line.

The Medical Research Council of Canada (MRC: Bliss, 2000; Friesen, 2000; Romano & Li, 2000) is an organization that aspires to do more by restructuring under a new banner, the Canadian Institutes of Health Research (CIHR). Changing the structure of an organization is costly but the costs of retaining the wrong structure can be even higher (Mintzberg, Ahlstrand & Lampel, 1998). In this assessment, we review the main features of the structure of a few benchmark organizations and compare these structural features to those of the MRC to appraise the changes in structure for the new CIHR organization that might improve scientific innovation by Canadian medical scientists.

Approach, Purpose and Research Questions

Our approach to analysis of the CIHR situation takes a public policy perspective and includes organizational and innovation theory. Our purpose is to constructively suggest improvements that are expected to promote the innovation process. This requires that we address the following questions:

1. Is the CIHR organization more likely than the previous MRC to lead to more discoveries and innovations as it is envisioned? In other words, are all worthy new ideas and innovations likely to be encouraged, and if not, what kind of vision is required of CIHR to make this so?

2. What key factors of organization structure drive optimum idea generation and innovation, and why? Are the key factors likely to be given priority at CIHR, and if not, what kind of structure and systems are required?

Unique Opportunity

CIHR is presently unstructured as it comes into existence as a result of Canadian Federal Government legislation (launched June 7, 2000), which could have mandated its structure (Gordon & Poulin, 1999), but did not. This study of the CIHR offers the unique opportunity to rethink the structure and funding systems at the outset and so help improve medical research and innovations for Canadians and Canada's trading partners of the future. This is easier said than done because it depends on the world-view and the motivations of the architects of the CIHR.

THE IMPORTANCE OF STRUCTURE TO INNOVATION

Structure is about people: the roles and responsibilities of decision-makers to manage the complex processes of human activity in the organization. In the context of innovation, structure is particularly about encouraging, supporting and assessing ideas and innovations. Designing such a structure poses a challenge to administrators of research organizations: somehow it must realistically engage the human will and spirit (Gellerman, 1963).

Systems of Rationing and Vision

Closely related to structure of the organization are systems that are developed to ration resources, typically through budget or funding mechanisms (Alam & Poulin, 1996). This is not to imply that some system of rationing of is not necessary in complex fields such as health care and research, as pointed out by Globerman & Vining (1996). But how this rationing system reinforces or otherwise relates to the structure of the organization will significantly impact on research outcomes. For example, a budget system that mis-allocates scarce resources can do much to defeat the efforts of dedicated research scientists and innovators.

The difference between failure and success often hinges on the validity of the vision of the organization and the philosophy behind the vision (Poulin, Mills & Spiller, 1998). Vision should be about encouraging and inspiring people by developing objectives that reflect an attractive future (Dennis, 1989). Not all organizations attract their people. One reason is

that top administrators tend to set up organizations to reflect their views and philosophy on how to accomplish given objectives.

INNOVATION WITHIN EFFECTIVE ORGANIZATIONS

Innovation is the application of scientific knowledge, toward a final commercial purpose (commercialization). The paradox of innovation is that it often requires “periodic shifts” in thinking from improvements on previous discoveries to radically new discoveries (Madique & Hayes, 1984, p. 18). Innovation theorists have known about this duality of incremental improvement and revolutionary discovery for some time but few organizations have structured themselves to take it into account (Burgelman, 1984; Price, 1996). Edwin Land, one of the most commercially successful and creative minds of this century (second only to Thomas Edison in patents), stated that scientific applications are the result of curious people, “searching for the causes of things” (McElheny, 1998, p. 16). He describes the process as “daring theories and daring adventures and colorful experiments, a feeling for life as seen through science” (McElheny, 1998, p. 19). We define innovation as *a daring and creative scientific search for, and application of, new knowledge for the benefit of others.*

We provide three examples of innovative organizations to demonstrate common features of structure and funding processes across widely different research contexts. The transferability of lessons will be valid to the extent that commonalities of content and process across the organizations transcend their situations or “contexts” (DeKluyver, 2000, p. 3). We first review Bell Labs.

Bell Labs

Bell Telephone Laboratories (Bell Labs: <http://www.bell-labs.com/>) of New Jersey was established in 1925. In 1985, Bell Labs won the inaugural U.S. Government's National Medal of Technology for the invention of the transistor. The transistor is typical of the breakthrough-research that Bell Labs has been able to develop and bring to the point of commercial reality. Diebold (1990) reported on a speech made before the US House of Representatives by Congressman Dean A. Gallo of New Jersey in 1987 who stated, “The transistor's switching power provided the technology for the computer revolution.... Today, it would be hard to imagine what life would be like without this small but extremely powerful piece of technology” (p. 2). How this Nobel Prize-winning feat was accomplished

at Bell Labs provides insight about a structure that is remarkable in its support of the discovery and feasibility stages of innovation. Diebold's report is also so insightful of the innovation process that it is worthy of extended quotation:

Russel Ohl, a chemist turned radio engineer, was attempting to improve the methods of detecting very-short-wave radio signals. He had discovered that bringing a fine metal wire into point contact with a crystal was the improvement he was seeking, and experimentation convinced him that a silicon crystal suited his ends better than any of the other materials he had tested. It took him a very few minutes to determine that the warming beam of his work light was passing, inadvertently, through the blades of the fan on his workbench. An electrical current was being produced when the light fell on the silicon and, in turn, being drastically reduced when the blades of the fan diminished the light. Neither Ohl nor any of his associates had ever seen anything comparable or had any idea what caused it. Following standard procedure at Bell, Ohl took his accidental discovery to his immediate superior, who passed it to his superior, and so on up the line... (p.3-5).

A decade-long process of scientific curiosity and persistence followed before the first primitive transistor was successfully demonstrated. Over this period, the phenomenon was pursued with interest by a growing team of practical and theoretical scientists whose talents complemented one another (Diebold, 1990). There were no guarantees of success since supporting technologies were needed to make the project feasible. But the best prospect of success at Bell labs was to assume that the project could come together and then to do everything possible to ensure that this would be so. Effective organizations have appropriate structures and support systems while ineffective ones do not (Poulin, Mills and Spiller, 1998; Bennis, 1989).

At least five lessons can be learned from the transistor innovation at Bell Labs. These are:

1. Ideas and innovation require scientists who are curious and creative.
2. The process of innovation appears chaotic, especially in the early stages.
3. Scientists require motivation and support to overcome many challenges.

4. People within the structure must be supportive and forgiving of mistakes.
5. The funding system must accommodate the non-linear process of innovation.

The second example of innovation is the 3M Corporation. 3M is exemplary in demonstrating how the difficult idea and feasibility stages can be successfully traversed, through the design and development of an appropriate structure and culture.

The 3M Corporation

The Minnesota Mining and Manufacturing Corporation (3M: <http://www.mmm.com/>) is arguably the leader among large-scale, research-oriented manufacturers. Not only is 3M especially effective at managing the interfaces between the idea, feasibility and commercialization stages of innovation, 3M also stands out among large-scale manufacturers with an average of 21% return on invested capital over the past three decades, compared to the 13% average of other manufacturers (O'Reilly, 1997).

Successful innovation at 3M is due to its "research driven style", that is its structure and culture (Nicholson, 1994, 1998). The fruit of 3M's structure and culture - innovation - is one good test of the soundness of the structure and the culture, as well as the philosophy from which they spring. 3M's culture is reflected in these four major aspects of its structure and system (Brand, 1998, p.21):

1. *Supportive Reviews*. Bringing together people, face to face, or by video conference, who have diverse backgrounds in a supportive review environment can result in the generation of new ideas.

2. *The 15% Rule*. 3M's 15% rule states that 3M people can spend 15% of their time working on innovative ideas of their own choosing. Not everybody uses it - and some take far more than 15% time. The consequence of this 15% rule has been a number of important new businesses for the company.

3. *Grant Money*. Money and time are required for innovation. Technical people can use the grant to pay for temporary labour to do some of their existing work while they spend more of their time developing their “15% project”.

4. *Recognition Programs*. Organizational structures and support programs encourage learning and knowledge transfer. As a result, 3M people tend to know more about “the heroes of innovation” than they do about senior management.

To summarize, the key elements 3M employs to support its scientists and technical people are: bringing people together in a supportive research environment, allowing them time to pursue their innovative ideas, making money grants readily available and providing recognition. It is clear that there are parallels between 3M and Bell Labs; most notably, scientists are encouraged to be freely creative during a significant portion of their time. This is a crucial point. For, as Kornberg (1992) observes, "major discoveries that have altered the face of medicine - for example x-rays, penicillin, recombinant DNA - have all come from the pursuit of curiosity about nature" (p. 859).

We estimate that 3M spends 25% of their resources on initiatives at the idea stage of innovation. (This is based upon 3M’s target of 15% of the scientists’ time spent on innovative ideas plus another 10% for free access to company-wide resources and easily available grants to support these “15% projects”.) The support that the research scientists at 3M receive makes for a consistent and research-rich culture, one that has resulted in innovative products for some 80 years.

At 3M, the formal structure is largely removed in the idea stage – scientists and technicians have much free rein on resources and time. This reflects the philosophy that as risk declines, “structure increases as a new technology moves across the development continuum” (Nicholson, 1998, p. 39). At 3M, structural formality progressively moves from the background to the foreground, with no management involvement (structure) during the initial idea stages but with top management very much involved during the final, “product to market” or commercial stage.

The US NIH as the "Gold Standard" of Medical Innovation

The final example of research innovation that we will examine is the National Institutes of Health (NIH) in the U.S.A. The US NIH is held by the Medical Research Council of Canada (MRC) as its standard for modelling the new Canadian Institutes of Health Research (CIHR) (cf. Lamont et al., 2000). Unfortunately, there are two problems with this approach: (1) the NIH structure is only partly consistent with the innovation process; and (2) the MRC does not take into account the dual structure of the NIH. The NIH organization includes an "extramural" program of grants, and an "intramural" program of its own scientific research. It is interesting to compare the two.

Most NIH intramural scientists are *de facto* tenured and salaried, having Federal civil service positions after a probationary period of between two to seven years. Tenure at NIH is defined as the long-term commitment of salary, personnel and research resources. Tenure is considered to be "at the heart" of the creative freedom experienced by NIH intramural scientists (NIH Fellows Handbook Tenure-Track Program, last modified 25-September-1998:

<http://www.training.nih.gov/handbook/tenure.html>).

NIH intramural scientists are reviewed every four to five years and resources adjusted up or down on the basis of evaluations such as provided by the outside Boards of Scientific Counselors (Chen, 1984). Tenure at NIH "frees intramural investigators to do high-risk investigations and direct their efforts at disease, spending no time competing for grants.... 'When the system works, it encourages risk-taking in a way that is extremely beneficial for science'" (Cohen, 1993b).

This is not to say that the NIH intramural program has been without criticism. Critics note the high research expenditures of the intramural program and warn that its overly bureaucratic procedures and control at political levels may limit the number of promising, young scientists in the future (Cohen, 1993a,b). But over-all, the freedom to innovate in the NIH intramural program and the level of innovation are high.

One common function of NIH intramural staff is to participate on extramural peer review committees, without being competitors for the same funds. NIH intramural expertise is also called upon to formulate "Requests for Applications" which solicit research for specific

problems, and to arrange consensus conferences, which can set standards and directions for clinical research. Despite the success of the NIH intramural program, there has been no intention to make the CIHR echo the intramural portion of NIH (Henry Friesen, MRC President, lecture on May 14, 1999, Winnipeg); instead the CIHR “Institutes” are to disperse funds by the MRC peer review system.

Critique of the NIH Extramural Program

When we speak about modeling CIHR on NIH, we should thus be aware that two very different research support structures are in place at NIH: intramural and extramural. An example of their contrast is that many NIH extramural applicants are on soft money and/or salaried for only 75% of their time, often without tenured or tenure track positions.

In recent years, peer review methods at NIH for extramural research have come under increasing criticism for not funding the best examples of innovation and for inefficiency. Sowers (1995) asserts that the proposal - evaluation process is destructive to the diversity of science. He states that the process generates “high activity in popular areas at the expense of orphan subject areas, and almost ensures that many important discoveries will not be made because they cannot be predicted... and an increasing possibility that good scientists may lose their careers” (p. 12).

NIH institute administrators concede that reviewers have become risk-averse in the 1990s (Marshall 1997, p. 888). Persistent criticism has led NIH Director Harold Varmus to overrule some of NIH's top administrators and add innovation to the decision criteria on a grant application (<http://grants.nih.gov/grants/guide/notice-files/not97-010.html>). Even with the new reforms, innovative ideas have to be approved by peers while they are still at the incubation or idea stage.

In contrast, the intramural system permits significant, high-risk innovation, and the majority of its scientists take advantage of the opportunity. Since an intramural scientist is well supported for a lengthy tenure, the innovative ideas can take off more often than those of his or her extramural counterpart. Publication is one measure of difference: investigators at the intramural NIH produce more biomedical research papers per year than any other group in the U.S. and these papers appear in highly influential journals (Narin & Keith, 1979). Another measure of impact is external recognition: five NIH intramural

scientists have received the Nobel Prize (Anon., 1999). (Bell Labs has had 11 Nobel Prize winners:

<http://www.bell-labs.com/history/heritage/awards.html#nobel>.)

The point that merits emphasis is that the NIH intramural program, not the extramural program, closely follows the innovation process of the successful commercial enterprises in supporting all qualified scientists at the early stages of idea development. Pioneering interests defy prediction, as critics of the peer review process point out (Gordon, 1993; Berezin, 1998; Forsdyke, 2000). Horrobin (1981) highlights the classic problem where so-called experts negatively assess important discoveries, not out of maliciousness but because they have a “blind spot”, an inability to look at the innovation from a “radically new angle” (p. 329).

The standard response of demanding evidence of feasibility at the idea stage is an infeasible proposition. The curiosity-driven researcher may even be unaware of the possibilities at this stage. This "honest ignorance" leads to several important questions. For example, can pioneers have peers in any meaningful sense of the term? How can a scientist begin to convey to others what the idea is when it is unclear to that scientist what the possibilities may turn out to be? Such questions capture the key problem faced by decision-makers: *a priori* predictability of really new ideas is most often impossible.

The two main lessons from our review of Bell Labs, 3M and the intramural NIH program are: give qualified scientists the means to pursue their interests as a matter of trust, and provide the structure that will foster their progress. We thus now turn to the Medical Research Council of Canada and compare its approach to these three innovative organizations to suggest possible improvements in its replacement, CIHR.

THE MEDICAL RESEARCH COUNCIL OF CANADA (MRC)

The Medical Research Council of Canada (MRC) was created by an Act of the Canadian Parliament in 1969. The MRC was to promote and undertake “basic, applied and clinical research in the health sciences and to advise the minister of the Department of National Health and Welfare on matters of health research” (Miller, 1988, p. 1326). The federal government made its aims more explicit during the 1980s and by 1986 its position was that

medical research should include a balance between basic and applied research that would result in improved health and health care.

Miller (1988) reports that there are a growing number of critics of medical research in Canada. One of the reasons most often cited is that "the conservative nature of the peer review system precludes progress of innovative science" (p. 1326). This is basically the same charge that has been lodged against the NIH extramural system of peer review over the past two decades.

A new decision-making framework appears necessary to ration limited funds and resources, one that would address the concerns of critics of the present peer-review system. As the critics contend, it is the scientists who are doing the research who are best able to determine what is worth pursuing. It is important to note that only among the leaders - the most innovative, research-oriented organizations - are the richest answers likely to be found. Then approach and structure, not domain or industry can be expected to spell the largest differences in performance and result.

Recent Support of Research Grant Applications by MRC

It would seem incumbent upon the scientific community to find out the level of funding support received by medical scientists and the distribution of the funding. But to our knowledge, there has been no survey conducted on the level of funding support for eligible scientists in Canada until 1999. By eligible, we mean qualification (e.g., Ph.D. or research active M.D.) and position (tenure track University appointments or equivalent).

In January 1999, R.G. and Joseph Pear (Psychology, University of Manitoba) conducted an e-mail survey with telephone follow-up of all heads of university departments in Manitoba whose members were eligible to apply for MRC grants. The Manitoba survey questions asked were these:

- How many of your faculty members are active in research?
- How many of these are eligible to apply for an MRC grant?
- How many presently have MRC funding?

The results of the survey are presented as Table 1.

(Place Table 1 about here)

In Manitoba, the MRC funded only 21% of university investigators active in medical research. MRC defined its "funding rate" as the fraction of *applications* funded, not the fraction of active and eligible researchers funded. (A somewhat similar study of the NIH extramural program found that 21.6% "of the total membership of the nine FASEB societies were NIH principal investigators in FY 1994": Garrison & Heinig, 1995.) The most recent data in operating grants for the MCR/CIHR indicates \$340 million and 2558 grants (http://www.cihr.ca/research_database/cihr_database_e.shtml). However, there are researchers who obtained more than one grant, yielding about 2000 principle investigators with current yielding grants. MRC reported funding rates are presented as Table 2.

(Place Table 2 about here)

These MRC funding rates overestimate of the actual level of funding: the Manitoba survey results indicate (comparing figures in Table 1 with Table 2), that many eligible and active investigators do not apply for MRC funding. This is likely because they anticipate a negative response based upon past experience with MRC.

The MRC Grant Selection Process

MRC had 672 members on Peer Review Committees in 1998-99. In the March 1999 competition, 89 of these members received grants, or 13% of the total. With a grant duration of 1 to 5 years and two competitions per year, we make the rough estimate that nearly 100% of the Peer Review Committee members received grants from other members of these committees on which they serve, albeit at different times. Who are the "peer" reviewers? The *Guidelines to be Followed in Determining Membership on MRC Peer Review Committees*, dated October 22, 1997, state:

Individual members should meet the following standards:

1. Scientific excellence, as reflected by ability to obtain continued peer-reviewed grant support for a research program; and
2. Quality as a referee - breadth of knowledge, maturity of judgement. (MRC, 1997; cf. nearly identical CIHR wording in: http://cihr.org/funding_opportunities/peer_review/ctteguide_e.shtml.)

This definition of “excellence” selects for reviewers who are successful at getting funding and, as mentioned previously there are opportunities for favoring applicants from one's own network and penalizing other applicants. This creates positive feedback (Milsum, 1968) that may continue to exclude the 80% who didn't receive funding in the first place.

Excellence versus Innovation

The word "excellence" permeates discourse about science research and its management at CIHR where excellence is considered distinct from innovation. The word "innovation" appears occasionally in conjunction with "excellence", but with no indication of a direct link between the two. For example: "MRC's strategic goals, laid out in its master plan, *Investing in Canada's Health*, are as follows: enlarge the scope of MRC activities, committing to a greater range of health science research; continue the pursuit of excellence and innovation in the areas of basic and applied health research" (Friesen, 1999).

It would seem that “innovation” is considered as separate from “excellence” at MRC. In contrast, the innovative Bell Labs, 3M and the intramural NIH acknowledges the overlap between the two terms: excellent research is innovative but not all innovative research turns out to be excellent and the risks of innovation are recognized in their grants approval processes.

Its mandate may be somewhat different, but CIHR is, in philosophy, likely to be an expanded MRC. This is primarily because the “Interim Governing Council” of CIHR was drawn in part from the same ranks as MRC, from the president on down (formerly: <http://www.mrc.gc.ca/aboutmrc.html>, now archived at <http://www.cihr.ca/>).

SUMMARY AND RECOMMENDATIONS

The Medical Research Council of Canada has been the major grant-funding agency for medical research in Canada. There has been evidence of growing dissatisfaction with MRC's approach as a perception emerges that medical research in Canada is falling behind Canada's neighbor to the south. The U.S. has also had its share of criticisms in the area of medical research, despite leading the world in this area of research since the foundation of the US National Institutes of Health. But the NIH extramural program is responding to

such concerns and suggestions as we are making here. Specifically, the NIH has instituted the following modifications to their extramural program, all of which should help maintain its leadership in medical research:

1. Addition of innovation as one of the five major criteria for reviewing and approving funding applications.
2. Inclusion of non-peer reviewers, e.g. patients and non-research health providers in the review process.
3. Innovations to the grants approval system, including feedback and speedy reprocessing of worthy applications.

These structural and systemic changes will bring the NIH extramural program slightly closer to structures that exist at 3M, Bell labs and the intramural NIH. The US NIH administration is leading the way by meeting past criticisms with action that addresses long-standing concerns with peer review, especially its tendency to be too conservative and not innovative enough. CIHR may follow. For example, the CIHR's Interim Governing Council Sub-Committee on Peer Review did reach consensus that "the criteria used by peer review panels should place greater emphasis on innovation in the assessment of the scientific merit of research proposals" (Mustard et al., 1999).

The CIHR's Peer Review Sub-Committee recommended, but the CIHR has not yet adopted, the five criteria adopted by the NIH (Mustard et al., 1999). The NIH criteria are: 1) the significance of the proposed research, 2) the rigor and adequacy of the research approach and methods, 3) the degree of innovation in the proposed research, 4) the calibre of the investigator or the research team and 5) the quality of the research environment in which the research will be done.

Answering the Research Questions

Two questions were asked at the beginning of this paper. They are repeated here, together with answers that are consistent with our interpretation of the evidence.

Question 1: Is CIHR organization likely to lead to more discoveries and innovations as it is envisioned? In other words, are all worthy new ideas and innovations likely to be encouraged, and if not, what kind of vision is required of CIHR to make this so?

Answer: In short, we have found some promise of change (e.g., the possible adoption of innovation as one of CIHR's evaluation criteria in peer review). But the new CIHR's blanket adoption of the MRC's formal and exclusive peer review process at all levels of the innovation process, and of its philosophy on funding "excellence" first and foremost (Charbonneau & Bernstein, 2000), leave it open to criticisms on resource allocation that are similar to the NIH extramural program (e.g., Jones, 1999). Therefore we hypothesise that the CIHR structure and funding process is unlikely to foster discoveries and innovations in any way proportional to its resources. This hypothesis is reinforced by the common structural features of the three innovative organizations studied - Bell Labs, 3M and the intramural NIH - which are distinctly different than both the old MRC and the new CIHR.

However, we must concede that we do not now know if other factors might confound this picture. For example, how do we know if the evident success of the 3M, Bell Labs and the intramural NIH is not at least in part due to more funding resources made available to their researchers? Or what about the productivity effects of researchers in these organizations being able to communicate directly because they are physically in one or two locations rather than dispersed across the country? And what might be the effects of hiring better researchers at higher salaries and providing them with better-equipped labs (cf. Gershon, 2000)? These questions need to be answered, if only to increase our confidence that factors other than organizational vision, structure and funding process are not unduly influencing the results.

Question 2: What key factors of organizational structure drive optimum idea generation and innovation, and why? Are the key factors likely to be given priority at CIHR, and if not, what kind of structure and systems are required?

Answer: The key elements of an effective organizational structure and system of funding decisions are essentially the same as are applied at 3M and at Bell Labs over much of this century. Many similar but not all elements also exist at the US NIH's intramural program. The evidence suggests that an appropriate structure is quite independent of whether the

institution is public or private: it is the quality of the leadership and the management that matters, not ownership per se. The evidence also suggests the lasting lessons of Bell Labs, 3M and NIH's intramural structures must be considered seriously in the new CIHR structure. The ramifications to these insights can be summarized in terms of the following six recommendations:

1. Hire well-qualified scientists in the universities who are diverse in their interests (with promotion and tenure committees providing check points).
2. Allow for seeming chaos in the earliest, idea stage of innovation. (The value of the work will become clearer as the research progresses.)
3. Resist the temptation to classify research as pure or applied. (Both are needed and often they overlap.)
4. Assist the innovation process by progressively moving from informal to formal procedures as more becomes known about the direction of the research. (This would be the transition from baseline to competitive research grants in a granting agency.)
5. Assess at the feasibility stage on criteria that give weight to significance of idea and innovation as well as the more traditional criteria such as approach, investigator, and environment.
6. Align the research project with the marketplace only after it becomes apparent that the research is ready to be commercialized.

Structure does not create ideas and innovations; people do. The quality of the research is dependent on the skill and creativity and innovation of people - individuals and groups - and the freedom to innovate without fear of loss of job or research funds. This was seen to be the case in the transistor discovery at Bell Labs and is apparently the case with so many of the intramural NIH breakthroughs and the 3M innovations. This is why it is imperative to hire researchers with great potential, and then support them well. It is not possible to duplicate the aggregation of researchers and research seen at, for example, the intramural

NIH. But much more can be done to stimulate scale effects among scientist-researchers across the country by encouraging greater collaboration and more collegial work relations.

The Process of Structuring CIHR and Funding

Our major hypothesis revolves around designing the organizational structure and funding system to foster the innovation process. We argue that a funding system that more reflects the innovation process would become an important factor affecting the research potential at each stage: idea, feasibility, and commercialization. This would mean that the rigorous application of peer review at the idea stage of innovation that was prevalent in the MRC be relaxed or eliminated in the new CIHR. Our premise is that peer review is generally counter-productive when it comes to the idea stage (McElheny, 1998).

We contend that the lessons from the three cases are transferable to the CIHR situation, at least in principle. The main lesson is that reviews need to reflect the particular stage of innovation of the research - idea, feasibility or commercialization - with rigor and formality of review increasing as more is known about the research and its potential. This would be distinctly different than the undifferentiated treatment that existed at the MRC and seems to be occurring at the new CIHR. A three-stage funding structure for CIHR could be budgeted as follows:

Grant Type	Percent of the CIHR's Total Budget for Grants					
		Recommended			Planned	
Stage I baseline grants	40%	\$200	million	0%	\$ 0	million
Stage II competitive grants	50%	\$250	million	90%	\$450	million
Stage III commercialization	<u>10%</u>	<u>\$ 50</u>	<u>million</u>	<u>10%</u>	<u>\$ 50</u>	<u>million</u>
Total	100%	\$500	million	100%	\$500	million

Stage I baseline grants would normally be granted to all qualified individuals (tenure track at a university, in appropriate departments), with *only a short, simple form to identify the applicant and the nature of the research*. This is in contrast to the evanescent “search for excellence” at CIHR (Charbonneau & Bernstein, 2000), with all its attendant requirements including complex forms and scrutiny of peer review, regardless of the stage of the research. Value of the baseline grants would be capped so as not to exceed available funds. This 40% baseline grants figure is significantly higher than the 25% enjoyed by scientists

at 3M for the idea stage of innovation. But this extra level of support can be justified by the relatively greater emphasis on idea generation and research, and lesser emphasis on commercialization by the public research institutions, compared to the private sector commercial enterprises.

Our proposal is phrased in percentages, so that it can float with the over-all propensity of the government to fund medical research. For example, a drastic decline in funding is predicted if cancer were to be solved (Horrobin, 1996). For now, let us take \$500 million as a round figure for the CIHR budget. In the Manitoba survey (Table 1), we found 519 active researchers including 390 MRC eligible medical researchers. If we assume a simple scaling to the rest of the country, based on population (Statistics Canada postcensal population estimates for 1999: <http://www.statcan.ca/english/Pgdb/People/Population/demo02.htm>), we get 10,000 active medical researchers across Canada. With \$200 million for the annual baseline budget, this comes to \$20,000/year for each eligible researcher and this would be the capped figure. The present system supports 21% of these people or about 2,000 researchers (Table 1).

We suggest that the only paperwork that qualified researchers would need to provide is proof of eligibility. Universities spend orders of magnitude more time in evaluating individuals (for hiring, promotion, and tenure) than peer review grant committees (Sowers, 1995), and should thus be trusted to decide who is eligible.

We are not suggesting that \$20,000 per research scientist will be sufficient for more advanced stages. But we believe this will go a long way toward bringing innovative ideas forward to the feasibility state where peer evaluation is more appropriate. Further, there is nothing preventing researchers from combining their efforts and funding and entering the competitive process at the later stages. The \$20,000 baseline funding would be in addition to that received by those fortunate enough to receive funding under the competitive system. We acknowledge that there will be \$160 million less funding for peer-adjudicated researchers (80% of \$200 million).

But is our proposal efficacious? The question of efficacy revolves around whether our differentiated, three-stage model is superior to the undifferentiated, peer-review model practiced at the former MRC and the new CIHR. It is also a question of whether there is a

trade-off between “excellence” (by funding a few ideas by the most promising researchers) and “innovation” (by funding the many more ideas of all qualified researchers). As previously stated, the problem with the former approach is the real difficulty, if not impossibility, of judging among qualified researchers before the results of innovative ideas are widely accepted, and this can be decades or more into the future. We suspect that policy-makers will only act with more evidence than we have presented so far and it is our intention to collect this evidence as soon as possible.

In our opinion, the Stage I, annual baseline grants of \$20,000 should be sufficient to pursue most ideas to the feasibility stage and significantly stimulate innovation. One reason is that baseline grants encourage co-operation instead of competition. Major innovation requires cooperation as new ideas often cut across established specializations (Mintzberg et al., 1998). The baseline grant would also eliminate the common feeling of dishonesty in "stealing" from one's grant to test new ideas. It could be thought of as an increase in the “professional development allowance” offered to all academic staff at some universities. The baseline grant shows a societal commitment to individuals, a trust that they will use the opportunity to create a lifetime devoted to research and innovation (Cole, 1995). Stage I baseline grants could be thought of as permitting each researcher to develop an idea and generate "preliminary data" which is needed to convince reviewers of increased funding via a competitive grant. We foresee the day when all tenure track and tenured medical researchers, regardless of whether they hold second-stage or third-stage grants, have baseline grants.

Stage II grants would be competitive, judged by peer review, with conflict of interest minimized. Thus we are not suggesting dismantling the peer review apparatus, only applying it where it is most appropriate. The Stage II grants would go to those ready and able to expand their research operations, who have proof in hand from testing their innovations that they are ready for this stage. With hassle free baseline grants available for everyone, these Stage II competitive grants might be fewer in number and in some cases larger than at present.

Stage III grants must pass criteria aimed at successfully bringing an idea to market. We agree that approximately 10% of the budget should be for university/industry technology transfer grants, i.e. commercialization, as stated in MRC recommendations: "In total, an

estimated 5% to 10% of CIHR funding would be directed to activities that underpin industrial development" (LePage, 1999; cf. Glynn et al., 1999). The main criteria at Stage III, the commercialization stage, would be social desirability and the ability to attract sufficient industry support.

Some Implications to the Recommendations

A fivefold increase in the engaged number of highly trained and motivated minds, 8,000 additional, might be expected to hasten solution of Canada's and the world's medical problems. The recent doubling of the MRC/CIHR budget would mean that funds would continue to be available to those presently identified as top researchers. Moreover, these researchers would also be qualified to receive the "no questions asked" baseline funding, permitting them too to innovate without peer review scrutiny. We are already paying about five times the baseline amount in salaries and benefits to all eligible researchers, which continues whether or not they do research. Baseline funding is not a lot of money for an individual researcher, but it keeps every qualified person doing some research, rather than dropping out. Some will be motivated to try for the Stage II competitive grants. Others may choose to combine their baseline grants with colleagues, for instance by sharing the purchase of equipment or the stipend of a graduate student or support of a technician. We might anticipate that allowing the aggregation of funding among researchers will go some way to bring scientists together.

FUTURE WORK

Future research needs to be guided by good questions such as those asked by Robert A. Phillips, Executive Director of the National Cancer Institute of Canada: "Do we have any studies to show how innovative are those scientists in government and university labs where freedom, flexibility and stability of funding exists, as we suggest? How do these compare to researchers that had to compete for funds through the granting councils?" (personal communication, 1999).

To our knowledge, no detailed research on this question has been conducted in Canada or elsewhere. The extant "research" consists largely of collections of observations and anecdotes such as we have drawn attention to in this paper. Gaining an appreciation for the relative effectiveness of different structural forms in different research contexts is overdue. For example, it would be interesting to estimate the "cost" per Nobel Prize in the international

versus extramural programs of NIH. How is it that NIH ended up with two such different structures?

The open question of possible trade-off between excellence and innovation - generosity to “top researchers” vs. baselines support for all eligible researchers - needs serious investigation. If it turns out that innovation is indeed stimulated by a flexible structure and funding system in that these encourages more cooperation than competition then there may not be a trade-off. This effectiveness of greater co-operation may more than make up for the lessened effort due to the diminished funding for a few, select researchers as well as provide greater opportunities for the many qualified researchers who receive no funding under CIHR at this time.

We need to document examples of productive and not so productive research and innovation, and causal factors, in a much more systematic way. This will not be easy. Innovation is perhaps a less slippery term than “excellence” but innovation is also a difficult concept to define and then measure objectively. It can be done but it needs a subtle approach. Of course, we will not be able to “prove” that our three-stage model is optimal. What we will be seeking to do is demonstrate, with relevant examples and detailed evidence, that one is superior to the other in stimulating innovation.

CONCLUSIONS

Our examination of the evidence suggests that Canadians enjoy less than optimal rates of medical research and innovation, for the money spent, as a result of structural problems. One consequence has been support for a few, privileged scientists at the expense of the majority of scientists. This may impede the innovation process.

Our proposed flexible structure and three-stage funding system respects the very different conditions that surround each stage of the innovation process. Furthermore, this structure is a manifestation of a philosophy that is both idealist - it is fair to include and not exclude as many people as feasible - and realist - it has worked over a very long period of time, as the example of 3M shows. Their structure and system represents one good answer to the paradoxical puzzle of delivering valued ends in an environment that can appear chaotic at times.

Our concluding hypothesis is that an inclusive organization structure together with the three-staged funding process will deliver more innovative, superior scientific outcomes. Limited though that evidence is now, we suggest that the potential benefits of more flexible structure and a three-staged funding system merit consideration within the new CIHR organization. Future research - both qualitative and quantitative - is needed to test the proposed link between the inclusive structure and funding process that we propose and the research outcomes that we all desire.

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TABLE 1: Results of Federal Grant Eligibility & Funding Survey, 1999*

University	Department	Research active	MRC eligible	MRC funded
Brandon	Botany	4	3	0
	Nursing & Health Studies	8	8	0
Manitoba	Anesthesia	25	9	1
	Animal Science	10	1	0
	Biochemistry	7	7	5
	Business Administration	15	15	0
	Cell Biology	13	13	8
	Chemistry	18	18	2
	Community Health Sciences	12	12	5
	Computer Science	16	3	0
	Electrical and Computer Engineering	23	4	0
	Entomology	5	1	0
	Geography	15	1	0
	Human Anatomy & Cell Science	8	8	2
	Human Genetics	4	4	2
	Immunology	15	18	6
	Internal Medicine	76	76	17
	Medical Microbiology	35	35	3
	Oral Biology	11	11	6
	Pathology?	32	32	2
	Pediatrics & Child Health?	64	64	6
	Pharmacology	12	12	8
	Pharmacy	8	8	0
	Physical Education	14	1	1
	Physiology	7	7	7
Sociology	22	4	0	
Statistics	14	1	0	
Winnipeg	Biology	15	3	0
	Chemistry	11	11	1
Totals		519	390	82
% Funded				21%

*A question mark (?) beside the category means no or incomplete response, so our estimates are not confirmed. MRC eligible are active researchers engaged in medical research. Last updated March 21, 1999, just before the subsequent round of MRC grant announcements.

TABLE 2
MRC Grant Applications

Date	Number Funded	Total Applied	% Funded	“Excellent” but Not Funded	MRC Budget (millions)	Budget for Operating Grants (millions)	Annual Budget/Grant
1994					\$265.9		
1995M	203	946	21%		\$250		
1995S		1005					
1996M	407	1277	29%		\$242.3	\$26	\$65000
1996S	261	1111	23%			\$19	\$87071
1997M					\$237.5		
1997S	325	1103	29%				\$66900
1998M	403	945	43%		\$271		
1998S	425	1220	35%	210		\$23.5	\$82600
1999M	379	987	38%		\$302.5	\$33	\$87071
1999S	338	1220	28%				
2000					\$373.8 (incl. CIHR)		
2001					\$484.2 (incl. CIHR)		

Sources: MRC Press Releases: formerly <http://www.mrc.gc.ca/press/press.html>,
<http://www.mrc.gc.ca/health/sld002.htm> and <http://www.mrc.gc.ca/cihr-icrs/slideshow/sld005.htm>. Now available via searching the archives at <http://www.cihr.ca/>.
Blanks represent information not located on MRC web pages. M = March, S = September.