NSERC LRP REPORT

Dear NSERC Colleagues,

The Chair and Associate Chair (Research) consulted with various groups in our department regarding NSERC's long range planning exercise. The groups consulted consist of Algebra, Geometry, Analysis, Applied Mathematics, Statistics, Math Finance and Math Biology. Because of the explicit interdisciplinary nature of their work the Math Biology group has decided to independently send their own response directly to NSERC; thus no mention of it here in this discussion paper. The Analysis group is sending their own detailed response; a shorter version articulating some of their main points of their reply is given in this report.

Some of the groups responded with a vision of the important directions in their areas that are deemed important, whereas others were more concerned about protecting the integrity of the Discovery Grants program.

To preserve the spirit of the discussion appended below, and apart from the contribution from Analysis, we only did some minor editing of the contributions from the various groups.

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1. Analysis

The analysis group at the University of Alberta has a strong reputation in Geometric and Harmonic analysis, including the subgroups of Geometric Functional Analysis, Banach algebra and harmonic analysis, and Multivariate approximation theory.

They have collaboratively benefited from the Institutes and BIRS. PIMS, Fields and BIRS have played significant roles in supporting their research, particularly in Collaborative Research Groups, 5 and 2 -day workshops, Research in Teams, and Thematic Programs. They also play a fundamental role in the training of PhD students, and postdoctoral fellows through the programs listed above as well as through the Northwest Functional Analysis Seminar held bi-annually at BIRS.

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The group is grateful for the very strong support by NSERC Discovery grants. They strongly feel that the budget for the Discovery grants should be left alone as presently done, and is not to be mixed with other new initiatives, or funding for the Institutes, which will otherwise eventually dilute their funding, and seriously disrupt training of HQP.

2. Algebra

The Algebra group is one of the strongest in Canada. A central theme of their activities is Lie theory, an area in which Canada has a long and well-established international reputation fostered by the work of H.S.M Coxeter and R. V. Moody among other. The present group conducts research in Infinite Dimensional Lie Theory, and connections to Galois cohomology, and Algebraic principal homogeneous spaces (Torsors), Algebraic groups, Iwasawa theory, Vector valued modular forms and connections to Physics, Motives and Chow groups.

The Discovery grants program should be protected, preserved, even enshrined in a Canadian Constitution of Funding for the Sciences. Tamper with it only with great reluctance. In fact, don't tamper with it, period. Institutes are fine, but do not let them encroach on Discovery Grants. There is widespread fear here for the Discovery Grants, and anger at the changes that are perceived to be happening to them.

In its heart, math is a solitary activity. Most of the group members collaborate actively, benefit from conferences, etc. yet most sparks happen in unpredictable ways. Even when they happen with a colleague at the blackboard, work reverts back to working details alone, to absorb it into our own picture. Research efforts are completed after (often long) exchanges. Discovery Grants mesh perfectly with this. They offer the flexibility and self-sufficiency that is simply glorious. It is the gem of the Canadian scientific community.

The group is very pleased with our institutes, especially BIRS. These bring the international research community to us, and diverse disciplines together, in a more systematic way than individual researchers could.

But don't let the Institutes take funds away from the Discovery Grants! It will be all too easy to divert funding to the institutes with the intention (or under the pretence) that these funds will come back to the individuals. The group has made it very clear, unanimously, that this is completely unacceptable.

3. Geometry

The research of the Geometry Group at the University of Alberta centres on algebraic geometry and its applications. Algebraic geometry is the study of the sets of solutions to systems of algebraic equations. Algebraic geometers investigate the geometric structure of these sets, and use notions and intuition from geometry to guide their investigations. Much of the mathematical machinery employed in modern algebraic geometry was only developed in the past 50 years. Concepts and motivation from algebraic topology (especially cohomology and homotopy theory), differential geometry, transcendental geometry (e.g., Hodge theory), and even mathematical physics all have left their mark on this rapidly expanding field. Conversely, algebraic geometry has proven to be a powerful, and in some ways crucial, tool for the development of each of these subjects: e.g, A^1-homotopy theory in algebraic topology, canonical metrics in differential geometry, algebraic cycles and higher K-theory, and the geometric language of string theoretic physics.

Here at the University of Alberta, we are fortunate to have a strong and interdisciplinary group of geometers. Under the leadership of Professor James Lewis (algebraic cycles), the group has grown to include Associate Professors Xi Chen (algebraic geometry) and Charles Doran (algebraic geometry and mathematical physics) and Assistant Professor Vincent Bouchard (string theory). We anticipate hiring further in arithmetic geometry in the near future. It is important to note that the truly interdisciplinary nature of algebraic geometry (and in particular our group in Edmonton) causes us to straddle the artificial divisions of the current NSERC Discovery Grants Evaluation Groups. More importantly, we would straddle even the existing proposal for the mathematical sciences envelope, since Lewis and Chen have always been supported by the pure mathematics GSC, Bouchard is supported by sub-atomic physics, and Doran has been supported by both. Particular attention to both the difficulties in funding research at the "geometry and physic" interface and the tremendous opportunities that such research represents would be welcomed as an outcome of the current exercise.

4. Applied Mathematics

- Research highlights in Computational and Applied Mathematics in recent years:

(i) Development of very fast compression techniques for data assimilation and analysis based on wavelets; the "digitalization" of all

communications and imaging of all sort which had a great impact on all human activities, would be impossible without it. It gave rise to the nonlinear approximation theory, which had great impact on the Numerical Analysis of PDEs.

(ii) Development of very fast parallel numerical methods for PDEs which allowed for the numerical solution of extremely large scientific and engineering problems, unthinkable until recently.

- Opportunities in the near future. In the next five to ten years, large parallel computers will become routinely available at low price. This gives tremendous possibilities for new important applications of the methods and algorithms currently available in areas like climate and weather prediction, medical and biological applications etc. The analysis of these methods is not well understood and needs to be developed. This in turn will allow for their optimization and further improvement, possibly to the appearance of principally new methods and algorithms.

There is at least one open millennium problem related to both, Fluid Mechanics and Approximation theory the existence and uniqueness of the solution of the incompressible Navier-Stokes equations (a Clay Institute problem). It gives a tremendous opportunity but it is unclear whether any progress will be made in the next decade.

- Clearly, Computational Mathematics is a highly interdisciplinary area, developing interactions with virtually all areas of Engineering and increasingly large number of Sciences Physics, Chemistry, more recently, Biology and even Social Sciences. The developments in these areas are the locomotives for the further development in Computational Mathematics, however, the various areas of Science and Engineering highly depend on this development.

- Principally, there are no barriers to the development of an interdisciplinary research on behalf of NSERC. However, there is a tendency that funding is primarily directed towards the final applications rather than the more fundamental development of Computational Mathematics. As a result, researchers in other areas try to develop methods and algorithms with an often insufficient mathematical knowledge, and sometimes reinvent existing methods or apply them wrongly. Therefore, the funding in such interdisciplinary areas should be done in balance. One possible strategy is to set apart some funding from various committees whose areas have some common interface and devote it to interdisciplinary research.

- In terms of infrastructure, the most important for Computational Mathematics is the network of large parallel computers available in Canada. In terms of availability of supercomputers, Canada seems to be lagging somewhat behind the rest of the developed countries. But

even the existing infrastructure can be used more optimally. For example, large parallel systems should be allowed for use only to parallel codes. Using such systems for sequential codes is a waste of resources.

5. Math Finance

Science

1) Recent progress in the study of more-realistic models for financial markets (e.g., incomplete markets, transactions costs, defaultable markets, interacting agents). Strategic developments in Mathematical Finance will be concentrated in the problem of risk minimization, which is extremely important now. These studies will be accomplished by corresponding investigations in the area of Stochastic Analysis and Statistics of Stochastic Processes as a natural and more important base for the modern Mathematical Finance.

2) By its very denition, mathematical finance is interdisciplinary (perhaps the most interdisciplinary of all applied mathematics). It applies a wide range of tools from mathematics (probability theory, statistics, numerics, analysis, applied math, functional analysis,...,etc.) to problems in finance and economics. Therefore, cross-disciplinary work is of crucial importance.

Research funding

1) We need funding for more intensive Canadian and international collaborations.

2) Some colleagues believe that some referees are not qualyed in interdisciplinary research such as Math Finance. Thus, close involvement of Math Finance specialists in funding committees may be a solution.

Institutes

1) Due to the interdisciplinary nature of Math finance, both BIRS and PIMS activities in probability theory (e.g. stochastic processes and their applications), and in applied math (PDEs, ODEs,....,etc.), are very important and relevant to our research and our HQP training (the BIRS activities in these domains are the most broad ones).

2) The role of these bodies (BIRS and PIMS) in training HQP is crucial and important. Through the conferences, workshops and training courses, BIRS and PIMS provide a valuable opportunity to our future researchers to start their career.

Training

1) Some colleagues in Math Finance believe that the department has to give some recognition for successful training. More precisely, values should be given to training that lead to high quality jobs after graduation.

2) Universities and/or Departments need to have enough funding to support Canadian and international students in learning and doing research (very often, international students are excellent but they cost more).

International

1) No funding available in Math Finance is available for distinguished visitors (only three months and the visitor should give many talks and therefore not enough time left for research).

2) No funding for the group for inviting outside speakers

6. Statistics

If a common denominator of the response of the statistical group at the department of Mathematical and Statistical Sciences of University of Alberta had to be found, it could be characterized as a reluctance to spend time with that at all. The initial soliciting met with no response; those select senior members that have been pushed into some reaction personally, expressed rather their conviction about the futility of such exercise, based in part on a belief that NSERC would not listen to their voice anyway. Of course, this is something that will likely be contested by the recipient of this message - but the necessity of having to defend their feelings may have been the reason why the prevailing majority opted rather not to express them at all. The following may thus represent rather isolated opinions - by no means any type of significant consensus or anything similar - but in the light of said above, it may be perhaps useful to voice them anyway.

The first of the issues concerned putting Statistics back, for the Discovery Grants program, into the same committee with Mathematics, which one of the members characterized as "certainly a return to the 'bad old days' of being judged by criteria which are quite irrelevant". While the last part of this statement may or may not be disputed, another member would second this knowing that for the decision purposes the aggregated committee decomposes into relevant subcommittees anyway - so what the purpose of unification then? But perhaps NSERC had deeper reasons for such a move.

The second of the issues were, as expressed by one of the researcher above, the dismay with the new, panel system of appraisals—whose introduction is seen not only as a proof that NSERC does not listen to opinions from its base (it was argued that in this matter, NSERC did commission a review of its policies, was essentially told to leave the things as they were, and then went ahead and made these radical changes anyway), but also a reason why "established researchers no

longer have any assurance that a program of curiosity-driven research, which might well not yield immediate results, will be funded". However, here the other voice from the group may partially dissent from this: if the consequence would be breaking of "hereditary" character of NSERC grants, when certain senior researchers can secure more than significant funding long past their prime, then the effect would be laudable; this appears to have happened lately, at least here and there. However, it is not clear, whether this is really a consequence of the introduction of the panel system, and would not be achieved also within the old system - should only the committee members changed as they did.