Nominations for President Elect

Nomination for Hyman Bass

by Irving Kaplansky

Hyman Bass

Let me turn the clock back to the late 1950s at the University of Chicago. The Stone Age was at its peak. (I hope that many young mathematicians will be reading this, and I realize they may not have a clue as to what I am talking about. In brief: In 1946 the late Marshall Stone took over the chairmanship; he promptly made a dazzling array of appointments, and this was promptly followed by the arrival of a dazzling array of graduate students.) Hy Bass was an outstanding graduate student. While still a student he published (*Proc. Amer. Math. Soc.* (1958)) a dandy paper on algebraic logic. This paper would honor the pen of a mature mathematician: it has motivation, good exposition, and clear and graceful writing.

It is not often that a dissertation introduces a new concept of permanent value. Hy's 1959 thesis added perfect rings to the armory of ring theorists. But let me jump to one of my favorites: "Big projective modules are free" (*Illinois J.* (1963)), with its entertaining subsection on very big projective modules. If you like projective modules (I do) and infinite algebra (I do), you are going to love theorems such as the following: any nonfinitely generated projective module over a Noetherian domain is free.

Mention "ubiquity" when you are standing near a commutative ring theorist of the homological persuasion. You will get a benign smile of recognition. Just the title is a classic: "On the ubiquity of Gorenstein rings" (*Math. Z.* (1963)).

I am closing in on the K-theory era. Please forgive me for bringing in a personal angle, dating from the early 1950s. Take the group of invertible n by n matrices over a commutative ring. Two subgroups beg for attention: the commutator subgroup and the matrices of determinant 1. Manifestly the former is contained in the latter. Are they equal? Certainly not; look at 2 by 2 matrices over the field of 2 elements. So let us avoid that pitfall by moving up to 3 by 3 matrices. Now are the two subgroups equal? I leaned to the belief that they were. I pestered people with the question. I encouraged a student to try some computations with algebraic integers; the results were inconclusive. But here is an oddity: although at the time I was simultaneously interested in functional analysis, I never thought of trying, say, the ring of continuous real functions on the circle. I believe that if I had done so, a negative answer would have surfaced in 24 hours and algebraic K-theory might have been born a little sooner. A Dyson style "missed opportunity"? Maybe. Anyhow, I was way off base. Not only are the two subgroups unequal, the gap between them spawned a major theory.

Grothendieck gathered projective modules into $K(0)$. Bass gathered the aforementioned gaps into $K(1)$. The result: a structure pleasing to topologists, including the inevitable long exact sequence. Soon $K(2)$ was added, and finally (Quillen) $K(n)$ for all n. Today we have algebraic K-theory, topological K-theory, the KK-theory of C^*-algebras, and doubtless scads of K's I have never heard of. K-theory has its own journal. And towering benevolently over all is Hy's monumental 762-page treatise.

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In 1975 Bass and Quillen received the AMS's Cole Prize in algebra for their work on *K*-theory.

I move to the congruence subgroup problem. In his 1993 nomination (*Notices* (1993), 815–6) Milnor described this achievement far better than I can. I hope that at least some readers will take the trouble to hunt it up. In brief: Hy was a member of the 3-person team (Bass, Milnor, Serre—what a team!) that cracked this notable problem.

In 1976 a paper in *Comm. Algebra* marked Hy's entry into the intriguing world of groups acting on trees. This played an important role in his participation in another remarkable team effort: the proof of the Smith conjecture. This concerns the nature of diffeomorphisms of the 3-sphere. Many people and many methods were assembled, and it is all collected in a book edited by Hy and John Morgan, *The Smith Conjecture* (Academic Press, 1984). Hy's contribution starts on page 127. I am going to take the space to state his theorem, slightly paraphrased and with the fourth alternative left vague.

Let G be a finitely generated group of invertible complex 2 by 2 matrices. Then at least one of the following four statements must hold: (1) G is conjugate to triangular matrices with roots of unity on the diagonal, (2) G can be thrown by similarity into the ring of algebraic integers, (3) the unipotent matrices of G lie in a normal subgroup with quotient infinite cyclic, (4) G is a certain amalgamated free product.

At a recent conference I heard John Morgan say that Hy was the ideal algebraist for the team.

Perhaps I have said enough to establish adequately his mathematical credentials. As is customary in these nominations, I finish by praising his personal and administrative qualifications. I am in an unusually good position to do this, for I had the privilege of working closely with him at MSRI. (There may be a handful of readers who do not recognize this acronym. The NSF-sponsored Mathematical Sciences Research Institute, located on the Berkeley campus, started operations in 1982.) Hy served as chairman of the Board of Trustees from the 1981–2 planning year through 1986, and then as chairman of the Scientific Advisory Council, 1989–92. I believe there is a character in one of Steinbeck's novels who was so good at fixing cars that a car would run better if he just stood next to it. That's how I felt at any meeting with Hy present. His wisdom, his good judgment, his tact, and his wide knowledge of the world of mathematics and mathematicians combined to make him a wonderful colleague. And I still think of it as a miracle that an East Coast mathematician would so unselfishly give so much to an Institute 3,000 miles away. MSRI has just had its funding renewed (in an open competition with new applicants). Thanks in good measure to what Hy gave us, MSRI's future looks promising.

We also worked together at the AMS, especially during my presidential term. I have before me an advance copy of his updated vita; it will accompany this nomination in the election materials. The list of his past services is as long as your arm. Never mind that. Right now he is chairman of the Committee on Education, he is on the Federal Policy Agenda Committee, and he is on the Editorial Board of the *Notices*.

The Committee on Education brings me to this recent phase of his career. For the past decade he has (again unselfishly) devoted a major part of his energy and talents to the problems we face in mathematical education at all levels, from K–12 to postdoctoral work. I start with the fact that he has chaired the Mathematical Sciences Education Board since 1993. On December 5–6, 1996, there was a conference at MSRI on the future of mathematics education at research universities. Hy was the keynote speaker, and the text of his address leads off the conference proceedings, just published. It is more accessible in the January 1997 *Notices*, pp. 18–21. If you have any lingering doubts about Hy as AMS president, at least browse it.

By now you will have gathered that I feel that he is ideally qualified to lead the Society into the new millenium.

Nomination for Daniel Stroock

by Raghu Varadhan and Victor Guillemin

Daniel Stroock

These few paragraphs are in support of the nomination of Daniel Wyler Stroock for president of the American Mathematical Society. The part dealing with the scientific aspects of Dan's career was written by Raghu Varadhan, and the one about the administrative aspects of his professional life by Victor Guillemin.

Victor will address Dan's contributions to our profession both at MIT and at the national level. I shall limit myself to the scientific aspects of his career. Dan Stroock has played a very significant role in the development of probability theory and its connections to analysis during the last thirty years. I have known him during this whole period. We met when he was a graduate student of Mark Kac at what used to be then called the Rockefeller Institute and now renamed Rockefeller University. I was a postdoctoral visitor at Courant Institute, in my third year. At that time there was a strong interaction, especially in probability theory between the two centers. We had a joint seminar once a week, that used to alternate between the two institutions.

Dan had found some problem on large deviations that I had worked on to be of interest to him and showed up in my office one day to talk. We found that we had a lot of common interests. He finished his Ph.D. and joined the Courant Institute as a post-doc for a year and then the faculty the following year. I had stayed on the faculty after

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three years as post-doc. We worked together for nearly six years on various aspects of diffusion processes. The so called martingale formulation did indeed get formulated during this period.

This work, for which we both shared the Steele Prize of the American Mathematical Society, focuses on the measure on path space as the basic object to be studied. This formulation provides a convenient base from which quick connections can be made to other approaches, like partial differential equations, semigroup theory, stochastic differential equations etc. The formulation has also proved very useful in the infinite dimensional context where questions on the nature of the domain of a potential infinitesimal generator hardly ever have decent answers. Over the years this approach has provided the best framework for the study of Markov Processes in diverse contexts.

I personally enjoyed working with him during these six years. We brought slightly different strengths to the partnership. Dan had developed a strong interest in harmonic analysis which he would develop further in future years. Dan's other passion was high mountains and horses. He moved to Colorado in 1972.

During his twelve years in Boulder, Dan started working on infinite particle systems collaborating with Dick Holley, a colleague at Boulder. Together they did some excellent work in an area that was developing rapidly. This involved connections between stochastic processes and statistical mechanics. It used stochastic dynamics to understand equilibrium behavior of complex statistical ensembles. Holley and Stroock made some of the important contributions to the subject. Their ideas were developed further by Stroock and Zegarlinski. In particular Stroock and Zegarlinski were able to prove for infinite dimensional systems a type of apriori estimate known as logarithmic Sobolev inequality which is more or less the only Sobolev type inequality which can survive the passage to infinite dimensions.

Around that time Paul Malliavin, in Paris, was starting a quiet one man French Revolution all by himself. He was trying to combine Wiener measure with infinite-dimensional differential geometry. Among other things this made possible a dynamical understanding of Hormander's hypoellipticity condition. Stroock and Kusuoka developed the so called Malliavin Calculus, making it accessible to probabilists. Malliavin Calculus views the solution of a stochastic differential equation as a "smooth" map on the Wiener space. The precise notions of smoothness, nondegeneracy, etc., are quite complex and thanks in part to Kusuoka and Stroock, the material is now familiar to many. Dan has used successfully the tools from Malliavin Calculus to study many problems of probability and analysis.

In fact, an underlying theme in Dan's work has always been the connection between probability and analysis. I cannot think of anyone who has explored this territory with more success than Dan. In recent years the focus has shifted ever so slightly as he has begun exploring the connections between diffusions and differential geometry.

Dan is also an excellent expositor, having written many books in probability theory. We did a book together way back and since then Dan has several books on Large Deviations, Basic Probability, Diffusions on Manifolds, etc. During his career he has collaborated successfully with many post-docs.

Raghu has given us a vivid mathematical profile of Dan. As Dan's colleague I would like to discuss another aspect of his life in mathematics: that of "good citizen". To say that over the years he has taken on his shoulders, selflessly and uncomplainingly, administrative duties that most of us would regard as onerous doesn't begin to do justice to his role as "good citizen". More to the point is the fact that he has performed these duties with an unflappable patience and sanity, often in circumstances which would try the patience of a stoic. A few items by way of illustration:

When Dan came to M.I.T. in 1984 much of the activity in his area of probability theory was centered, not in the mathematics department, but in electrical engineering and computer science. Realizing that this presented, not an adversarial situation, but a golden opportunity, he set out to organize seminars, courses and other activities that math and engineering students could participate in together; and though the personal cost in time and effort has been considerable, he has succeeded (with a lot of help from Sanjoy Mitter) in making stochastic processes an intramural field of study at M.I.T. (and creating, into the bargain, an enhanced image of the mathematics department itself as a "good citizen").

The centennial of the birth of Norbert Wiener was celebrated at M.I.T. with considerable pomp and eclat, and, in particular, by a large week-long conference. For the organizers of this conference—Dan, David Jerison, and Isadore Singer—the crises encountered in the course of securing funding, selecting speakers, and commissioning a biography of Wiener to commemorate the occasion were worse than the most pessimistic nay-sayers could have anticipated. Dan's good common sense was a large factor in weathering these crises and making the conference a success.

Dan served an exemplary two years as chair of the pure mathematics department in spite of the fact that those two years were demanding years for him in terms of duties centered, not at M.I.T., but in Washington (c.f. sopra).

Dan is currently serving as the representative for mathematics in the National Academy of Science, and as such has been very effective in promoting the agenda of mathematics (and getting mathematicians into the academy!)

Most of Dan's time this year has been taken up with his activities on the Board of Mathematical Sciences. The findings of this board will play an important role in post-millennial funding for mathematics and, in spite of the fact that Dan's frequent weekend trips to Washington have left him with little time for his favorite extracurricular activity (demolishing his junior colleagues on the squash court) he is upbeat, not unjustifiably I think, about the positive effect of the committee's recommendations. If he's right, it will clearly be, in no small part, due to his efforts!

We both hope that we have convinced you about Dan's exceptional talents as a mathematician as well as his commitment and willingness to serve our profession. We believe that he will make an outstanding president.