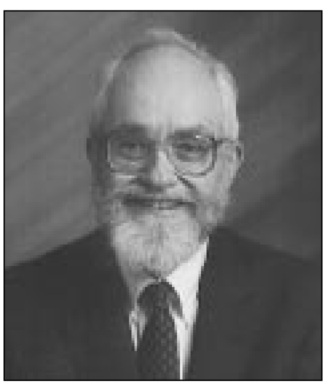
Nominations for President-Elect

Nomination for Felix Browder

Jerry Bona



It is a privilege, as well as a signal honor, for me to place Felix Browder's name in nomination for the presidency of the American Mathematical Society. Felix Browder is one of the charismatic figures in world mathematics, as well as one of the living mathematicians of highest stature in mathematical analysis, a worthy successor of Leray, Schauder, G. D. Birkhoff, and Kellogg. In the last forty years he has been the central figure in the development of nonlinear functional analysis and a major contributor to the application of both linear and nonlinear functional analysis to partial differential equations.

One of Felix's characteristic qualities is his immense breadth of learning, combined with extensive experience in both research and administration. His positive view of the future of mathematics, derived from an exceptionally thorough knowledge of the history of mathematics and combined with a keen awareness of the problems that our field presently faces, makes him a uniquely desirable candidate for the presidency of the American Mathematical Society.

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Linear functional analysis had its major development in the first half of this century, starting with the spectral theory of Hilbert and von Neumann and the work of Banach. Nonlinear functional analysis, although rooted in the work of Poincaré in the last century, had its first great upsurge in the work of Leray and Schauder in the 1930s. The subject had its origins in the study of nonlinear ordinary and partial differential equations, but it came to encompass a wider range of questions in all branches of analysis and in differential geometry, in theoretical physics, and in economics. Felix Browder has been the dominant figure in this field since the early 1950s.

In the theory of linear elliptic partial differential equations, the work of Felix Browder and his school went well beyond the techniques first introduced by Russian analysts in establishing completeness theorems for the eigenfunctions of nonselfadjoint elliptic differential operators. Browder's results on these fundamental issues remain definitive to this day.

In nonlinear functional analysis the introduction of monotone and, later, accretive operator theory led to the solution of problems that had heretofore been out of reach. Felix Browder proved a general theorem on monotone operators in reflexive Banach spaces, stating that a coercive continuous monotone operator from a reflexive Banach space to its dual space is subjective. This theorem led to the proof of some deep existence theorems for nonlinear partial differential equations and began a massive development of monotone operator methods and their applications to partial differential equations. It is especially noteworthy that Browder's theory freed us from restrictive compactness assumptions and thereby led to a very substantial enlargement of the range of nonlinear problems to which exact analysis could be applied.

The elementary contraction-mapping principle states that if a Lipschitz map from a complete metric space to itself has Lipschitz constant less than one, then the mapping has a (unique) fixed point. It is also well understood that this conclusion is false in general if the Lipschitz constant is greater than one. Mappings where the Lipschitz constant is exactly equal to one (so-called nonexpansive mappings) arise in practice, for example, in differential-delay equations, and there the question of whether or not there must be a fixed point is much more delicate. In 1966 Browder proved a result, now known as the Browder-Gohde-Kirk Theorem, stating that a nonexpansive self-mapping of a bounded, closed, convex subset of a uniformly convex Banach space has a fixed point. Indeed, a central theme in Felix Browder's work has been extensions of fixed-point theory, the concept of fixed-point index, and the related idea of degree of a mapping to mappings in infinite-dimensional spaces. For example, he extended degree theory to pseudo-monotone maps from a reflexive Banach space to its dual space. This result applies to nonlinear elliptic operators in generalized divergence form. In some of his later work Felix Browder developed the theory of nonlinear contraction semigroups, thus bringing his methods to bear upon time-dependent problems in partial differential equations. Again, his work has had a lasting influence.

A vivid memory of mine that gives an indication of Felix Browder's influence was an international conference in which every single invited speaker acknowledged his or her indebtedness to Felix's work! Some of his work has become such a part of the fabric of nonlinear analysis that when authors cite it, they do not need to provide the original reference.

Felix Browder has played an active role in the Society throughout his career. He was the Colloqium Lecturer in 1973. The list of committees on which he has served is too long to present here. His unstinting and effective work as editor of the *Bulletin* and his service on the Science Policy Committee have left a permanent mark. He has organized innumerable special sessions at regional and national meetings and was a principal organizer of the meetings that celebrated the heritage of Hilbert, E. Cartan, Poincaré, and Weyl.

More than any other mathematician I know, Felix Browder has striven successfully throughout his career to raise the level of discussion and broaden the range of interaction of mathematics and the sciences. His efforts as chair of the mathematics department at the University of Chicago went a long way to create a dialogue at the highest level between mathematicians, physicists, and geophysicists. His stewardship of the Mathematics Department at Chicago attests to his total and uncompromising dedication to excellence and to his vision of the leading role of mathematics within the sciences. He initiated many appointments of the highest quality, including those of Spencer Bloch, Luis Caffarelli, Charles Fefferman, and Karen Uhlenbeck. As vice president of research at Rutgers University he brought about the appointment of some of the best mathematicians and scientists in the country, including Gelfand, Brezis, Kruskal, Zabusky, Coleman, Daubechies, and the now-famous string quartet in quantum field theory. This was accomplished despite the very limited portfolio and official power this administrative position carries. At the same time he initiated the successful Science and Technology Center that involved Bell Labs, Rutgers, and Princeton University. He has had extremely close relations with the French school of nonlinear analysis, where the addition of applied mathematics to pure mathematics departments paralleled, and was decisively influenced by, the development of the department of mathematics at the University of Chicago under Felix's chairmanship.

Felix Browder has been strongly supportive of efforts to improve undergraduate and graduate education in the mathematical sciences. In precollegiate mathematics education he was instrumental in bringing about the creation of the AMOCO project in teacher education and curriculum development at the University of Chicago, and lately he has been the main supporter of I. M. Gelfand's outreach program.

Felix Browder is one of the few mathematicians whose influence and leadership are acknowledged and appreciated in the scientific community at large, as well as among mathematicians. The depth of his mathematical research, his broad vision, as well as the effectiveness of his leadership make him, in my opinion, the candidate who will successfully steer the Society through the difficult years ahead.

Nomination for Srinivasa S. R. Varadhan

Daniel Stroock

In connection with his nomination to become president, the AMS asked me to write an appreciation of my first, and still my closest, mathematical colleague, Srinivasa S. R. Varadhan.[1] I have agreed to do so because I find win-win situations irresistible: If I do a good job and he wins, then I will have done the AMS a great service; if I do a poor job and he loses, then I will have done Varadhan an even greater service.

Varadhan, whom everyone else calls Raghu, came to these shores from his native India in the fall of 1963. He was, officially, age 23; he was, in fact, 22. In any case, like many other postwar immigrants to this country, he arrived by plane at Kennedy Airport (or was it still Idlewild?)

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[1] *I have been told, but cannot reconstruct, what the "S" and "R" denote.*

and proceeded to Manhattan by bus, past twenty miles of uninterrupted cemeteries. For a Hindu from Madras accustomed to tidier procedures for disposing of the dead, it is understandable that the trip from the airport remains the most vividly remembered part of his journey. His destination in Manhattan was that famous institution with the modest name The Courant Institute of Mathematical Sciences, where, at the behest of Monroe Donsker, he had been given a postdoctoral fellowship. Actually, in spite of its moniker Courant had not yet moved out of the hat factories to which NYU had originally consigned Richard Courant's reincarnation of Göttingen. Thus, when I, a humble graduate student from the opulent Rockefeller Institute, first met Varadhan, he was sequestered in one of the many dingy, windowless offices out of which flowed a remarkably large fraction of the postwar mathematics of which America (or at least the American mathematical community) is justly proud.

Varadhan had completed his Ph.D. at the Indian Statistical Institute in Calcutta. As much as any other institution, ISI is responsible for the (apparently incorrect) rumor that the Indian term for statistician is "Rao". Thus, it was a surprise to no one that Varadhan came equipped with a superb grounding in statistics (a subject about which few other probabilists know anything at all). But CIMS was hoping for more. Varadhan's own arrival at CIMS had been preceded by that of V. S. Varadarajan, another renowned graduate of ISI, whose extraordinary mathematical erudition was already evident in the much-coveted set of notes which he produced during his sojourn there.

Within a year or so Varadhan demonstrated that he certainly could and probably would fulfill or exceed any of the hopes that Donsker and the rest of CIMS might have for him. Because no less a figure than K. Itô had found most of the results slightly earlier, Varadhan never published the research done during his first year at CIMS, yet within a few months of his arrival his promise was never in doubt. Rather than pine over his misfortune, Varadhan dropped the project on which he had spent a year and took up, mastered, and brought to fruition an idea of Donsker's which made its first appearance in the beautiful thesis of Donsker's student, M. Schilder. The general idea in Schilder's thesis was that one should attempt Laplace-type methods to develop asymptotics for the evaluation of Wiener integrals. Although, thinking in terms of Feynman integral representations for solutions to Schrödinger's equation, physicists had made somewhat casual reference to related ideas in order to justify Ehrenfest's "theorem" (the one which asserts that quantum mechanics becomes classical mechanics as Planck's constant goes to 0), Schilder seems to have been the first mathematician to come to grips with the challenge presented by carrying out Laplace asymptotics in an infinite-dimensional setting. However, Schilder's treatment was somewhat primitive, and its applicability was severely limited. In particular, it was only after Varadhan took up the problem that it became clear that Schilder had been studying a very special example of what statisticians call *the theory of large deviations.*

The study of large deviations goes back to the work of Khinchine and Cramér, but the term *theory* is not an accurate description of what those august gentlemen had produced. In fact, if there is, even now, something which deserves the name, the theory of large deviations was born in Varadhan's famous 1966 article on the subject, in vol. XIX no. 3 of the CIMS journal C.P.A.M. It was in that article that he clarified the analogy between large deviations and the theory of weak convergence of measures, an analogy on which he based his formulation of *the large deviation principle* in terms of an upper bound for closed sets and a lower bound for open sets. Of course, a formulation does not a theory make. But Varadhan provided the theory as well. Namely, as summarized to me by a Japanese friend, the theory of large deviations consists of two steps: the first step requires you to prove either the upper or lower bound yourself; the second step requires you to get on the telephone and ask Varadhan how to prove the other bound.

As anyone who has followed his career will confirm, large deviations has been a recurring theme in Varadhan's mathematics. For one thing, Varadhan has had an uncanny ability to understand that large deviations are manifest in all sorts of situations where nobody else even suspected their presence. To me, the most spectacular example of his special insight lies in his realization that M. Kac's old formula for the first eigenvalue of a Schrödinger operator can be interpreted in terms of the large deviations. Like those in Schilder's thesis, the large deviations here involve Wiener measure. However, whereas Schilder dealt with large deviations of Brownian (typical Wiener) paths over a very short interval, the explanation for Kac's formula must be sought in the large deviations of Brownian paths from ergodic behavior over very long intervals. So far as I know (and I was one of his students), Kac himself, much less anyone else, had never guessed that such an interpretation might exist. Furthermore, I suspect that not even Varadhan anticipated the wealth of results to which systematic exploitation of his insight has led over the last twenty years. His insight not only underlies the profound applications which appear in his own famous work with Donsker but also accounts for the subsequent (possibly overabundant) effusion of articles by others (including me) on the topic.

Toward the end of the period when Varadhan was polishing off the program initiated in Schilder's thesis, he and I began the discussions which eventually led to our formulation of diffusion theory in terms of what we called *the martingale problem.* Those discussions took place nearly thirty years ago, but they remain in my mind as the single experience which makes me most grateful to have entered mathematics. Of course, the pleasure of participating in what turned out to be a successful enterprise was great. But I think that I am being honest when I assert that the ultimate success of our collaboration was only part of the pleasure which I derived from it. The other part was my getting to know Varadhan. I was a young man who had been afforded every advantage: I had educated, prosperous parents who paid my passage through the best schools in America. Here was a man my own age whose parents, though certainly educated, were far from prosperous. He had won his passage by outperforming all but a handful of the literally millions of Indians his age. (Actually, the confusion about his age, alluded to earlier, means that he outperformed people a year older than he.)

I am not going to claim that Varadhan does not enjoy his success; he does. Nor am I about to say that he is some sort of saint; we could never have become friends if he were. Nonetheless, what distinguishes Varadhan from nearly all the other gifted people whom I have met is the remarkable command he exercises over his own gift. In particular, he has learned how to prevent his unusual intellectual powers from poisoning his relations with lesser intellects. For example, Varadhan can tolerate being wrong, at least occasionally.[2]

In addition, he is not one of the many mathematical princes who espouse the notion that all their obligations to humanity can be met through their contributions to mathematical research. Varadhan has already carried more than his share of the administrative burdens at CIMS, and so his acceptance of the nomination to become president of the AMS only confirms his well-established commitment to take his turn at the common wheel.[3]

I hope that the preceding remarks help to explain why I believe Varadhan to be the best choice for the president who will guide the AMS into the twenty-first century. He is as talented, caring, and effective an individual as I have ever encountered. If he cannot improve the reputation that our subject has in America, then I must doubt that anyone can.

[2] *In fact, he is sometimes wrong on purpose. I recall his claiming one fall afternoon in the CIMS coffee lounge that Indians do not suffer from allergies. Needless to say, this announcement was greeted with some skepticism, especially from a few hay fever-ridden colleagues like Louis Nirenberg. At the Christmas party that year, when Varadhan's wife Vasu thoroughly undermined his claim by discussing her own allergies, Varadhan smiled and admitted that he had been led to exaggeration in a mild fit of Indian chauvinism.*

[3] *Be that as it may, it has yet to be determined whether his acceptance means that he will relinquish his privilege, as a member of the Indian Mathematical Society, to pay negligible annual dues to the AMS.*