1990 Steele Prizes Awarded in Columbus

Three Leroy P. Steele Prizes were awarded at the Society's ninety-third Summer Meeting in Columbus, Ohio.

The Steele Prizes are made possible by a bequest to the Society by Mr. Steele, a graduate of Harvard College, Class of 1923, in memory of George David Birkhoff, William Fogg Osgood, and William Caspar Graustein.

Three Steele Prizes are awarded each Summer: one for expository mathematical writing, one for a research paper of fundamental and lasting importance, and one in recognition of cumulative influence extending over a career, including the education of doctoral students. The current award is $4,000 for each of these categories.

The recipients of the Steele Prizes for 1990 are R. D. Richtmyer for the expository award; Bertram Kostant for research work of fundamental importance; and Raoul Bott for the career award.

The Steele Prizes are awarded by the Council of the Society, acting through a selection committee whose members at the time of these selections were Luis A. Caffarelli, Alexander J. Chorin, Charles L. Fefferman, William Haboush, Jun-ichi Igusa, Arthur M. Jaffe, George Lusztig, Mark Mahowald, and Michael E. Taylor (Chairman).

The text that follows contains the Committee's citations for each award, the recipients' responses to the award, and a brief biographical sketch of each of the recipients.

Expository Writing

R. D. Richtmyer

Citation

For his book *Difference Methods for Initial Value Problems*, Interscience, first edition 1957, second edition (with K. Morton) 1967. Richtmyer's book has been the most influential book in the development of numerical methods for solving partial differential equations, and has been widely used and studied for more than 30 years since its first appearance, and is still widely used now; this is a remarkable record in a field that has changed radically over that period of time.

R. D. Richtmyer

The book (in both editions) has two parts: theory and applications. The theory part is a masterful exposition of stability and convergence theory for difference methods. The fundamental techniques have proved to be widely applicable. Most working numerical analysts have learned their stability theory from this book, and the clarity of the exposition is responsible in great part for the fact that all numerical analysts have a good, shared understanding of these important ideas. The second edition is also responsible for the wide understanding of the conditions under which the von Neumann condition is sufficient for stability and of stability theory for mixed initial-boundary value problems, theories developed between the two editions.

The part of the book devoted to applications presents a variety of numerical schemes for solving problems in fluid mechanics, neutron transport, elasticity, and related topics. It is an amazing fact that the ideas that are at the forefront of recent developments, for example Godunov's scheme, schemes based on Riemann solutions, and shock tracking, are discussed quite extensively in this book. This is in part due to the outstanding insight of the author who saw what was important and promising, clearly and at an early date; it is also a consequence of the fact that this widely used book has kept these ideas in front of the mathematical community and thus fostered later developments.

The book has been an indispensable part of the library of all numerical analysts and all applied mathematicians interested in computation for over 30 years and will remain so for a long time to come.

Response

In writing the book on difference methods, I was serving mainly as a reporter, reporting on an exciting new kind of mathematics that had come into existence through the work of many people, and it is those people who ought to be getting the credit and honor of this presentation.

Before the Second World War, difference methods had been used for ordinary differential equations. Astronomers had been doing it for years in celestial mechanics. In those problems, the instantaneous state of a physical system was represented by a small number of quantities, for example coordinates and momenta of bodies, and the equations of evolution of the system were ordinary differential equations, with time as the independent variable. Application to more complicated problems, where the instantaneous state of the system required functions (generally of several variables) for its description, could not be considered until computers became available, although important theoretical work had been done by Courant, Friedrichs, and Lewy in 1928.

Nowadays, we read in our newspapers that scientists have studied a problem such as that of global warming by computer simulation of the earth, with its atmosphere and oceans. It may seem difficult to imagine a time when the term "computer simulation" didn't exist, and the idea of it didn't exist. It was one of those ideas that hadn't even been thought of — that is, until someone thought of it. The first such simulations, to my knowledge, were the implosion calculations at Los Alamos in 1944 and 1945. It was important to predict the implosion quantitatively, in order to determine whether, and to what extent, nuclear criticality would be achieved. Just who first thought of doing it numerically by difference methods, I don't know. I would guess that many people at Los Alamos thought of it simultaneously, probably including Hans Bethe, John von Neumann, Klaus Fuchs, Rudolph Peierls, Bill Penny, and Stanley Frankel. Under the conditions of temperature and pressure in the implosion, the materials behave like fluids, so that one had to deal with the hyperbolic partial differential equations of fluid dynamics, together with shocks and other nonlinear effects. The ideas of Courant, Friedrichs, and Lewy were of course involved. The computation was done by a group of people using a large room full of old-fashioned punch-card machines, together with a staff of people operating hand computers. Each calculation took about a week.

Soon after the war, in 1947, it occurred to people at Los Alamos to use similar methods for the nuclear explosion, after criticality had been achieved. That is more complicated, because there was not only the fluid dynamics, but also heat transfer, which satisfies a nonlinear parabolic equation, and the integro-differential equations of the neutron multiplication, all these being coupled together in nonlinear ways. The planning and programming of that calculation took about a year and a half, after which the problem was put on IBM's SSEC computer at IBM World Headquarters in NYC. That was probably the physically largest computer ever built; it occupied several floors of the building, with 10,000 vacuum tubes (not miniature tubes, either), 40,000 relays, and other electronic and mechanical components. Each computation took about a month of round-the-clock operation. That provided the model for other simulations at LASL and elsewhere, in nuclear science, meteorology, and stellar evolution.

Both during and immediately after the war, the most important single person involved in all of that was von Neumann, who knew the work of Courant, Friedrichs, and Lewy and knew how to generalize it, and whose vast knowledge of both physics and mathematics provided the guiding principles that were constantly needed in order to make the work meaningful. Without von Neumann's guidance, those simulations could not have been done.

At about that time, in the early 1950s, other people became interested in difference methods, and a number of significant papers were written, too numerous to discuss in detail. Some of the most important contributions were made by Peter Lax of NYU, including what became known as the Lax Equivalence Principle, which deals with the basic problem of deciding whether the results of such a gigantic calculation really do represent the evolution of the physical system. The basic idea there was to think of the functions that describe the instantaneous state of the system as being a point in a function space, in fact a Banach space, so that powerful principles of functional analysis could be used.

The other person I feel I must mention explicitly is K. W. Morton of Great Britain, who joined me as coauthor for the second edition.

The book was indeed quite timely (but even for that I claim no credit — I just happened to be at the right place at the right time), and it was used in many countries for many years, and both editions were translated into Russian.

If we can agree that the credit belongs to the people I have mentioned and to many I have not mentioned, I am happy to accept the great honor of this award on behalf of all those people.

Biographical Sketch
R(obert) D(avis) Richtmyer was born in Ithaca, N.Y. on October 10, 1910 (i.e., 10/10/10), the son of a self-made man of great energy and motivation, F. K. Richtmyer, who made it from farm life in upper New York State to Professor of Physics and Dean of the Graduate School at Cornell University. Professor Richtmyer took his family to Europe on a sabbatical in the academic year 1927-28; there, R. D. Richtmyer, who had just completed high school, attended lectures and classes at Göttingen University in calculus, analytic geometry, topics in analysis, and basic physics, taught by J. Grandjot, B. L. van der Waerden, R. Courant, and R. W. Pohl, respectively. Back in Ithaca, he attended Cornell and received the degrees of A.B. in Physics (1931) and M.A. in Physics (1932). From 1932 to 1935, he was a graduate student at M.I.T., where he received the degree of Ph.D. in Physics in 1935. His thesis was an application of quantum mechanics to double-ionization x-ray lines. From 1935 to 1940 he taught at Stanford University as Instructor in Physics, where he became acquainted with F. Bloch, J. R. Oppenheimer, V. Weisskopf (summer visitor), W. W. Hansen, and N. E. Bradbury (later Director at Los Alamos). From 1940 to 1945, he was in Washington, DC, as civilian scientist, first with the Navy Department (magnetic and acoustic minesweeping) and then with OSRD (Manhatten Project; atomic energy). During those years he became acquainted with G. Gamow and E. Teller and was privileged to share their interest in astrophysics. At Los Alamos as Staff Member from 1945 to 1953, he became acquainted with J. von Neumann, H. A. Bethe, R. P. Feynman, O. Frisch, K. Fuchs, L. Nordheim, S. M. Ulam, J. C. Mark, and other well-known physicists and mathematicians. During that time, he conducted studies (some of them later published) on fluid dynamics, nuclear and thermonuclear reactions, the Monte Carlo method, computer programming, cosmic rays, continued-fraction expansion of algebraic numbers, and systematic sampling. From 1953 to 1964 he was at New York University, first as Associate Professor of Mathematics, then as Professor; there he had the opportunity to extend his mathematical knowledge by learning many things from P. D. Lax, L. Bers, and J. Schwartz. He worked in numerical analysis and computer methods and collected material for the book on difference methods. After 1964, he was at the University of Colorado at Boulder as Professor, for one year in computing science and then with a joint appointment in the Departments of Physics and Mathematics. He is now Professor Emeritus there. He has taught for shorter periods at the Universities of Chicago, New Mexico, Heidelberg, Munich, and Uppsala. He is the author of a two-volume work on advanced mathematical physics, based on courses taught at C.U. Boulder.

Fundamental Paper

Bertram Kostant

Citation
For his paper "On the Existence and Irreducibility of Certain Series of Representations", *Lie Groups and Their Representations* (I.M. Gelfand, editor) pages 231-329, J. Wiley, 1975. For many years one of the major problems of mathematics has been to describe all the irreducible unitary representations of a semisimple Lie group. A major force in the attack on this problem was Harish-Chandra, who produced an array of tools. Some of his results had an entirely algebraic formulation, but by and large the study of unitary representations remained almost exclusively analytic. Hilbert spaces were realized as L^2 spaces, and irreducibility results rested on Fourier transforms. The subject appeared too difficult for algebra.

Bertram Kostant

Kostant's work changed this situation completely. He used algebraic methods to solve completely a problem that had resisted analytic methods, and as a consequence found a simple and powerful construction for new families of unitary representations. The focus of the cited paper is the family $X(\lambda)$ of spherical principal series, parametrized by λ in a certain complex vector space $\mathfrak{A}^* = \mathfrak{A}_0^* + i\mathfrak{A}_0^*$; $X(\lambda)$ is unitary for $\lambda \in i\mathfrak{A}_0^*$. Extending work of Bruhat, Kostant determined precisely the set of λ for which $X(\lambda)$ is irreducible. Generally, there is an irreducible subquotient $Z(\lambda)$, and Kostant determined when $Z(\lambda)$ has an invariant Hermitian form. His approach used work with Rallis to reduce the problem to cases when G/K has rank one, which were treated by hand, in an elegant fashion.

Kostant's paper has had many interesting offshoots in analysis on symmetric spaces, influencing well known work of Helgason, Kashiwara, and others on the Poisson transform. Connections with intertwining operators have been pursued by Kostant, Wallach, and others. Deep mathematical connections with the Langlands program have surfaced in recent years.

Undoubtedly the greatest effect of Kostant's paper was that it established the algebraic approach to infinite-dimensional representation theory as a powerful and important one. It brought to bear ideas from algebraic geometry, invariant theory, and non-commutative algebra; ideas that seemed unrelated then, but seem indispensable now. What more can we ask of any mathematics?

Response

It was one of the great fortunes of my mathematical life to meet and become friendly with Hermann Weyl. It happened at the Institute for Advanced Study in the middle 50s, approximately a year or so before his death. The main topic of our conversations was representation theory and, in particular, the newly emerging infinite dimensional representation theory of non-compact semisimple Lie groups. With regard to the latter, it was my impression that Weyl was quite doubtful as to whether there would ever be much of a coherent theory. At least this was the case until one day I found out about Harish-Chandra's result on what today is referred to as K-finiteness—and told Weyl about this. I was amazed at how enthusiastically he greeted this fact. His attitude completely changed. He was now almost certain that there was indeed a beautiful theory to be developed.

Among other things Harish-Chandra's result opened the door to a serious algebraic attack on a subject which hitherto had been pretty much in the domain of analysis. It seemed to me, however, that in order to get real detailed information this way, one first needed to get a better understanding of the universal enveloping algebra U of a semisimple Lie algebra. Some time later I showed that U was nothing but "invariants" times "harmonics". Chevalley told us what the "invariants" looked like and I found out that the "harmonics" admit an elegant description involving the nilcone—which, to make matters even nicer, turned out to be a normal affine variety. At any rate with this kind of structure theory and Harish-Chandra's results, the stage was set for the paper which is being honored by this year's Steele Prize and I thank the Council of the American Mathematical Society for the honor.

Anyone who is working on some problem and making negligible progress is continually faced with the decision as to whether to stay with it—investing more time and energy—or to go on to other things. Making such a decision is as hard for me today as it was when the paper being honored here was written. The years have given me no great wisdom in dealing with this matter. Nonetheless, concerning this quit or go on business, there was an experience I had when working on the paper that I would like to tell about. At one point in order to make progress I had to know—and know very explicitly—all the roots of each member of a family of polynomials. Each day's calculation over a period of many weeks only yielded expressions of a typical such polynomial as some sort of sum—information which was totally useless for the purpose of finding the roots. Under such circumstances it surely would have been a prudent decision to abandon the whole approach. I didn't however and the reason was the fascination I had with the structure that gave rise to the polynomials. One of the cases had to do with the exceptional Lie group F_4. At some early stage in my education the last chapter in Chevalley's book *Theory of Spinors* had made an indelible impression on me. It had to do with the Principle of Triality and it was clearly the gateway to the magnificent world of the exceptional Lie groups. This gateway and more was also certainly encoded in the structure for the case of F_4. The corresponding polynomials thus took on a special personal importance for me. At any rate the point of this story is that simplistic persistence paid off. One morning the summands started collecting in strange ways and by the end of that sunny day in 1967 in a motel room in Reno, Nevada, the polynomials split into a product of linear terms and the roots were staring up at me.

I am not sure whether or not there is a lesson to be learned from the experience cited above except perhaps this. Before you invest a large amount of intellectual energy in trying to solve a problem in some area I think it is wise to be sure that (1) you care a great deal about the problem you are working on and (2) the area you picked has literally unbounded intellectual richness.

Biographical Sketch

Bertram Kostant was born on May 24, 1928 in Brooklyn, New York. He received his Ph.D. from the University of Chicago in 1954. His research interests are Lie theory, algebra, differential geometry, and geometric quantization.

At Chicago Professor Kostant won the Frank Lewis award and was University and AEC fellow. He was an NSF fellow and member of the Institute for Advanced Study from 1953 to 1955. For the year 1955-1956, he was a Higgins lecturer at Princeton University and from 1956 to 1962 he was on the faculty at the University of California where he progressed from assistant professor to professor of mathematics. Since 1962 he has held the position of professor of mathematics at the Massachusetts Institute of Technology. During this time he was a member of the Miller Institute of Basic Research (1958-1959); a Guggenheim fellow, a lecturer at Oxford University, and a professor at the University of Paris (1959-1960). He was also a professor at the Tata Institute in 1969 and the University of Paris in 1982. He has been a Sackler Institute fellow since 1982.

Professor Kostant has been a member of the AMS since 1952. He is an associate editor of *Advances in Mathematics, Revista Mathematica Iberoamerica* and was an editor of I. M. Gelfand's collected works and the *Annals of Global Analysis and Geometry.* He is a member of the Standing Committee of the International Colloquium on Group Theoretical Methods in Physics and is a member of its Advisory Committee for the next colloquium.

Professor Kostant gave an Invited Address at the International Congress of Mathematicians in Nice in 1970 and an Invited Address at the AMS Annual Meeting in Cincinnati in 1962. He delivered the AMS Colloquium Lectures in Albany in 1983.

Professor Kostant received a medal from the College de France in 1983 and was given an honorary degree from the University of Cordova in 1989. He is a member of the National Academy of Sciences and the American Academy of Arts and Sciences.

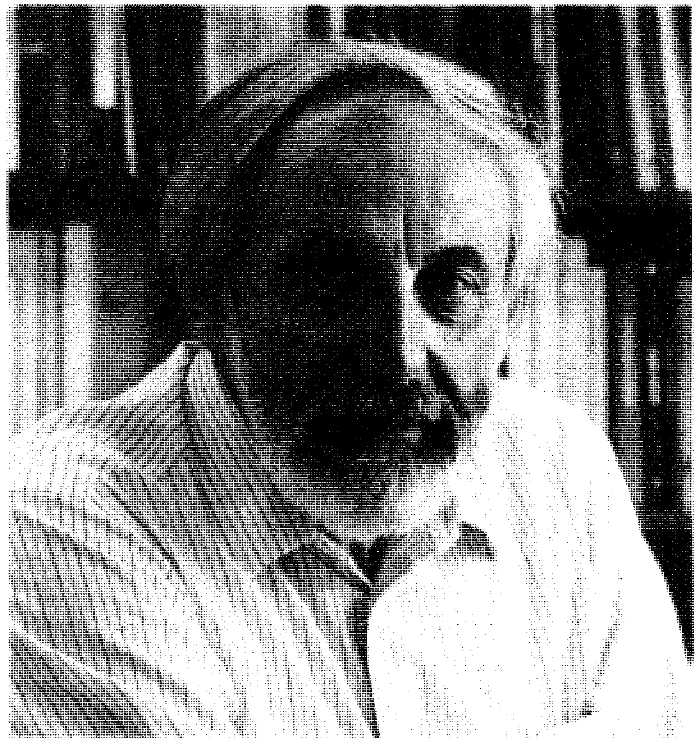
Career Award

Raoul Bott

Citation

Raoul Bott has been instrumental in changing the face of geometry and topology, with his incisive contributions to characteristic classes, K-theory, index theory, and many other tools of modern mathematics. His early spectacular success was in the application of Morse theory to the study of the homotopy of Lie groups, giving the celebrated Bott periodicity theorem. This central result has had several further incarnations, first as a cornerstone of topological K-theory, in work of Atiyah and Bott, and more recently in operator K-theory and noncommutative differential geometry.

Periodicity bears crucially on the study of the index of elliptic operators, a theory molded and made a central part of mathematics primarily by collaborations involving Atiyah, Singer, and Bott. This result is, amongst other things, both a far reaching extension of the Riemann-Roch theorem and a tool for exposing, via the Dirac operator, mysteries of the A-genus of spin manifolds. This has provided fresh insights over the decades; a notable example is its role in calculating the dimension of moduli spaces of Yang-Mills fields. It has also stimulated further development, such as Connes' work on index theory for foliations, itself a field in which Bott had done basic work, such as his introduction of secondary classes for foliations.



Raoul Bott

There are many other subjects graced by Bott's thoughts, such as his lovely extension of the Borel-Weil theorem, complementing the realization of irreducible representations of a compact semisimple Lie group on spaces of holomorphic sections of natural line bundles with a result on vanishing of higher $\bar{\partial}$-cohomology. This leads to valuable insights, such as connections between character formulas and the Atiyah-Bott-Lefschetz fixed point formula. The Atiyah-Bott-Gårding work on lacunas is an inspiring piece of work involving applications of algebraic geometry to the fine structure of solutions to hyperbolic partial differential equations.

Also to be remembered is his work on meromorphic sections of vector bundles, giving rise to Bott-Chern forms. These developments will continue to influence generations of mathematicians, inspiring them by the breadth and depth of a truly magnificent career.

Response

Thank you for this great honor. The fact that it is most probably not deserved makes it only the more enjoyable! Thank you also for the very generous citation. When in the early days of my academic life I would tell my step-father that I had given a Colloquium at some prestigious place, he would lean forward intensely in his chair and ask: "Now tell me precisely how they introduced you". And then I could always see the restrained disappointment in his face when I would counter with: "Well, they said that now Raoul Bott would speak on such and such". For, you see, he was reared in the Austro-Hungarian empire where they were used to rich and pungent rhetoric on such occasions. Now finally I could have shown him a citation with some teeth in it!

This occasion comes some forty years after I gave my first "three minute talk" to the Society, right here in Columbus. Yes, they had three minute talks in those days and actually I remember it more vividly than any of the hour talks I have delivered to the Society since. My dear friend and at that time research director Dick Duffin rehearsed me for the talk. After my first presentation he said: "Very good Raoul, but cut it in half". When I had done so I tried again. "Excellent", he said, "but cut it in half". And I must say this principle of cutting one's lectures in half twice has stood me well ever since. Would that all my professional brethern had learned it also.

Of course I learned other things from Richard Duffin as well. First of all, I liked and have ever tried to emulate his way of being a sort of mathematical Samurai. From the moment I walked into his office at Carnegie Tech. and explained to him the network problem I had brought with me from my engineering days at McGill, we became collaborators. And many months later we resolved the question—this is now called the Bott-Duffin theorem in engineering circles—in a manner which addicted me to joint work for life. After a long and hard session at the blackboard, we parted to go home exhausted and again defeated. But walking along the busy road it suddenly became clear to me that the answer had been staring us in the face all afternoon! I rushed home and called Dick. The phone was busy—he, of course, had made the same observation and was calling me!

This then was the first of many joyous collaborations that I now look back upon: with Hans Samelson—the geometry of Lie Groups; with Chern—equidistribution theory; with Michael Atiyah—our fixed point theorem in Woods Hole, and the many ramifications of the index question and K-theory. There are also those exciting ten days when we teamed up with Lars Gårding, dealing with lacunas in partial differential equations—when we were essentially locked up in Gårding's institute. With Graeme Segal-Gelfand Fuchs cohomology; with Andre Haefliger the secondary characteristic classes of foliations, and with Paul Baum the residue formulas for holomorphic vector fields. In a different vein there is the work with Loring Tu on our book, and in more recent times, now already under the influence of ideas from Physics, the Yang-Mills theory on Riemann Surfaces—again with Michael Atiyah, and most recently, with Cliff Taubes the rigidity questions in elliptic cohomology. These were all wonderful rejuvenating experiences during which our subject seemed all the more alive because it was shared so intimately, and the collaboration let the mathematics shine through the formidable ego's that, after all, beat in all of us. In accepting this award my first word of thanks is then to you, my long-suffering collaborators.

But there are also so many teacher-friends and near-collaborators and students that deserve my gratitude that I don't know where to begin or end.

I doubt that I would have had the courage to switch from Engineering to Mathematics but for the constant support of Lloyd Williams at McGill, or the inspired bending of all the rules at Carnegie by J.L. Synge to make such a switch viable. Thereafter I was fortunate enough to be brought to the Institute at Princeton by Hermann Weyl and then received personal instruction by a truly stellar cast: Ernst Specker, Kurt Reidemeister, Norman Steenrod in topology; Armand Borel, Fritz Hirzebruch, Iz Singer and Arnold Shapiro in characteristic classes. J.P. Serre tried to teach me sheaf theory and I attended the famous Kodaira Spencer seminars. Marston Morse was my first mentor in Morse theory, and Reneé Thom and Steve Smale—my first Student!—later added finishing touches.

I have now been at Harvard over thirty years and there is not a single colleague or student who has not added to my education, or uncovered some hidden mystery of our subject. To all of you then I address my second word of thanks.

But let my final *Thank You* be to this Country which has accepted so many of us from so many shores with such greatness of spirit and generosity. Accepting us—accent and all—to do the best we can in our craft as we saw fit. Having just returned from a brief visit to my newly liberated homelands, the dimensions of this gift has only now came truly into perspective.

Biographical Sketch

Raoul H. Bott was born on September 24, 1923 in Budapest, Hungary and received his early education in Europe. He received his Bachelor's degree in Engineering in 1945 and his Master's in Engineering in 1946, both from McGill University. He then switched to mathematics and received his Sc.D. from the Carnegie Institute of Technology in 1949. He spent the next two years at the Institute for Advanced Study. From 1951-1959, he was at the University of Michigan, with a leave of absence to return to the Institute in 1955-1956. In 1959, he moved to Harvard University where he is currently the William Caspar Graustein professor of mathematics. During his years at Harvard, he has held numerous visiting positions; the longer-term of these include Oxford University, Tata Institute, University of California at

Berkeley, the Institute for Advanced Study, the Institut des Hautes Etudes Scientifiques, and the University of Bonn.

Professor Bott has served as editor of *Topology* (1965-1985) and on the Board of Editors of the *American Journal of Mathematics* (1969-1971). He has been a member of the Society since 1950 and has served the Society in a number of ways. He was elected a Member-at-large of the Council (1961-1963 and 1968-1970); served on the Executive Committee of the Council (1961-1962 and 1971); and was elected Vice-President in 1975. In addition, he has served on numerous AMS committees.

Professor Bott has presented Invited Addresses at several AMS meetings; the Summer Meeting in Ann Arbor (August, 1955), the Sectional Meeting in Philadelphia (October, 1966), the Summer Meeting in Providence (August, 1978), and the Centennial Celebration in Providence (August, 1988). He spoke at the 1958 International Congress of Mathematicians in Edinburgh and presented a Plenary Address at the 1970 International Congress of Mathematicians in Nice. He was the AMS Colloquium Lecturer at the Summer Meeting in Eugene (August, 1976).

Professor Bott was a Sloan Fellow in 1956-1960 and received a Guggenheim Fellowship in 1976. He received an Oswald Veblen Prize in 1964. In 1964, he was elected to the National Academy of Sciences and, in 1987, was awarded a National Medal of Science. He is an honorary member of the London Mathematical Society and an honorary fellow of St. Catherine's College, Oxford. He has received honorary degrees from Notre Dame University (1980), McGill University (1988), and Carnegie Mellon University (1989).

NOMINATIONS FOR THE 1991 FULKERSON PRIZE

This is call for nominations for the D. Ray Fulkerson Prize in discrete mathematics that will be awarded at the XIVth International Symposium on Mathematical Programming to be held in Amsterdam, The Netherlands, August 5 - 9 1991.

The specifications for the Fulkerson Prize read:

"Papers to be eligible for the Fulkerson Prize should have been published in a recognized journal during the six calendar years preceding the year of the Congress. This extended period is in recognition of the fact that the value of fundamental work cannot always be immediately assessed. The prizes will be given for single papers, not series of papers or books, and in the event of joint authorship the prize will be divided.

"The term 'discrete mathematics' is intended to include graph theory, networks, mathematical programming, applied combinatorics, and related subjects. While research work in these areas is usually not far removed from practical applications, the judging of papers will be based on their mathematical quality and significance."

The nominations for the award will be presented by the Fulkerson Prize Committee (Martin Grötschel, Chairman, Louis Billera, and Paul D. Seymour) to the Mathematical Programming Society and the American Mathematical Society.

*Please send your nominations by **January 15, 1991** to:*

Professor Dr. Martin Grötschel
Institute of Mathematics, University of Augsburg, Universitätsstr. 8, 8900 Augsburg, West Germany