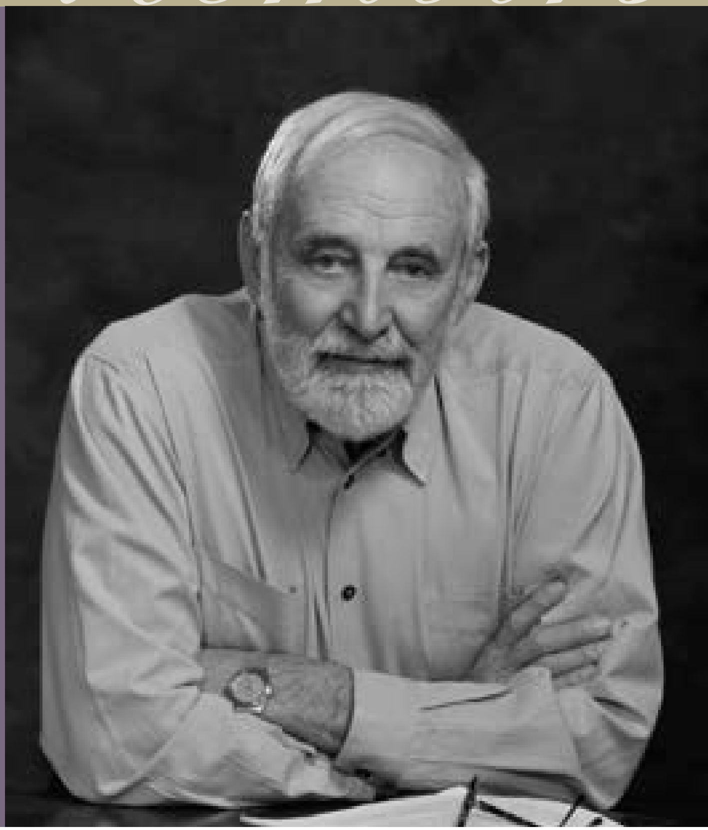
Raoul Bott

1923–2005

BIOGRAPHICAL *Memoirs*



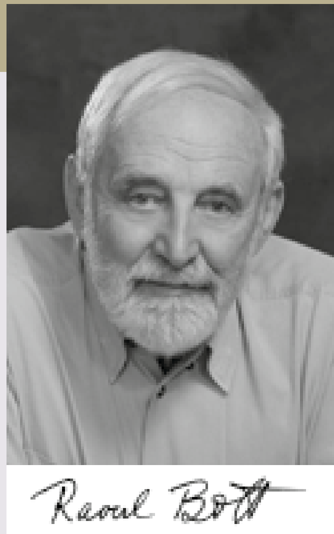
A Biographical Memoir by
Sir Michael Atiyah

NATIONAL ACADEMY OF SCIENCES

RAOUL **BOTT**

September 24, 1923–December 20, 2005

Elected to the NAS, 1964



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By Sir Michael Atiyah

Raoul Bott[1] was one of the outstanding geometers of our time, and his influence on mathematics owed much to the warmth of his personality.

Early background

Raoul was born in Budapest, Hungary, but until the age of 16 years he lived in Slovakia. He was a typical child of the Austro-Hungarian world, educated in a variety of languages: Hungarian, Slovak, German, and English (learned from his English governesses). Raoul's mother was Hungarian and Jewish, whereas his father was Austrian and Catholic. Despite the fact that his parents' marriage broke up shortly after he was born, and that he saw very little of his father, his mother brought him up as a Catholic. Though lapsing as a teenager, he remained a Catholic throughout his life. Raoul's mother remarried, to the chief executive of the local sugar factory, a position of some importance in the community. The family was well off, living a comfortable middle-class life; the parents traveled extensively and the younger children were educated at home. Eventually, in 1932, they moved to Bratislava, the capital of Slovakia. Here Raoul finally went to a proper school, where he had to master Slovak; but he was a mediocre student and distinguished himself only in singing and German. His main interests at the time were music, which remained a passion all his life, and making electrical experiments in the basement, a foretaste of his decision many years later to study electrical engineering.

Raoul's mother died of cancer when he was just 12 years old—a devastating blow that brought him closer to his stepfather. He also had a network of uncles, aunts, and cousins who provided an extended family. In 1938–1939, with the German designs on Czechoslovakia becoming increasingly clear, his stepfather acted promptly and moved to England, with Raoul following in June 1939, where he met the new stepmother from a

1 This memoir originally appeared, in slightly different format, in *Biographical Memoirs of the Royal Society* 53(2007):63-76 and printed with permission.

wealthy Jewish family in Budapest whom his stepfather had just married.

Raoul was now sent to a boarding school, but fortunately for him he escaped (for the time being) the traditional rigors of an English public school. Instead he went to a progressive private school, which believed in freedom, self-expression, and coeducation. Raoul remembered his time there as one of the truly formative periods of his life: “In one stroke it made me a lifetime anglophile as well as a great admirer of the opposite sex.”

Figure 1. Raoul Bott, McGill University, 1942.
(Photo courtesy the Bott family.)

His stepparents had only a transit visa for England, and they left for Canada early in 1940, with Raoul following shortly thereafter. In Canada he had to have a further year of schooling and, as he said, “the harsh fate of going to a British public school, which I had miraculously so avoided in England, caught up with me in Canada.” In the autumn of 1941 he enrolled at McGill University as an electrical engineering student. Here he had inspiring teachers; he worked hard and graduated in April 1945. At that stage he decided to enlist in the army, but with the end of the Pacific war his military career was cut short and he returned to McGill in the autumn for a master’s degree. During this time Raoul was very unsure what path he should follow, and he even tried to take up medicine before being sympathetically but firmly discouraged by the school’s dean of medicine. This rejection triggered a prompt response from Raoul—he decided there and then to become a mathematician.

With the encouragement of his teachers at McGill he went to the Carnegie Institute of Technology (now Carnegie Mellon University) in Pittsburgh to work on applied mathematics under Professor John L. Synge. Despite Bott’s sketchy and formally inadequate mathematical background, he was accepted as a Ph.D. student and in the spring of 1949, when he received his degree, he finally found himself on the verge of becoming a math-

There were the giants of the time—Albert Einstein, John von Neumann, and Weyl, all refugees like himself from Nazi Germany—together with leading American mathematicians such as Oswald Veblen and Marston Morse. Between them, and under their influence, the whole canvas of mathematics at the highest level was being explored and the young Bott was totally enraptured.

ematician. His thesis, written under the direction of Dick Duffin, led, also in 1949, to his first (joint) paper. Although the "Bott–Duffin theorem" came to be well known among electrical engineers, it was Bott's last contact with that profession. However, the paper itself had attracted the attention of the great Hermann Weyl, originally from Göttingen, Germany, but now a colleague of Albert Einstein's at the Institute for Advanced Study in Princeton, NJ. Weyl invited Bott to come to the institute, a move that immediately opened his eyes to the wider vista of mathematics and transformed his career.

Princeton and Michigan, 1949–1960

Bott spent the first decade after earning his Ph.D. between the Institute for Advanced Study and the University of Michigan in Ann Arbor. His teaching appointment to the Michigan faculty was preceded by two years, 1949–1951, as a visitor at the institute and he returned there for a sabbatical in 1955–1956. He described both of his stays in Princeton as decisive in his mathematical development.

In 1949, the institute introduced him to an entirely new mathematical world. There were the giants of the time—Albert Einstein, John von Neumann, and Weyl, all refugees like himself from Nazi Germany—together with leading American mathematicians such as Oswald Veblen and Marston Morse. Between them, and under their influence, the whole canvas of mathematics at the highest level was being explored and the young Bott was totally enraptured. He soon dropped the rather elementary and mundane work he had been doing at Carnegie on electrical circuits and began absorbing the new ideas that surrounded him in Princeton.

Both Princeton University and the institute were pioneers at the time in developing the new field of topology, which was rapidly maturing into the major enterprise it would become later. Morse had made his name (at the institute and earlier at Harvard University) in the application of topology to the study of critical points of functions (started by Henri Poincaré) and in topology's extension to the calculus of variations. Both pursuits enabled the deriving of information about closed geodesics on Riemannian manifolds. This was a field that attracted Bott and remained a dominating theme throughout his life.

Bott's most important work for many years centered on the application of Morse theory to the topology of Lie groups and their homogeneous spaces. Lie groups, in particular the classical matrix groups, had originated in the pioneering work of the Norwegian mathematician Sophus Lie at the end of the 19th century; by the mid-20th century, Lie groups had become of central importance. Weyl had transformed their representation theory, and they were playing an important role both in differential geometry and quantum physics.

In the 1950s the time was ripe for bringing together the new fields of topology and Lie groups, and Bott was the right man at the right time to bridge this gap. Others contributed to the algebraic side of the story, but the link with analysis through Morse theory was due to Bott and his Michigan collaborator Hans Samelson. It was Bott's good fortune that, when he went to Michigan in 1951 with his head full of new ideas, he found in Samelson a kindred spirit a little older in years and knowledgeable about Lie groups. Between them they wrote many papers about the topology of Lie groups and in particular about their loop spaces (in 1958). Fortunately, the introduction of loop spaces by J.-P. Serre in his 1951 thesis had revolutionized topology by providing a systematic approach to calculating homotopy groups. Bott and Samelson mastered Serre's work and combined it with Morse theory in spectacular fashion.

In traditional Morse theory it was customary to assume that the critical points of a function were isolated, since this would be the generic case. However, Bott realized that "in nature" things are not generic and that critical points often arise along sub-manifolds. But he also realized how to incorporate such situations into the theory, and this was applied to great effect in the study of geodesics on Lie groups. For example, the closed geodesics on the group SU(2), the three-dimensional sphere, come naturally in continuous families parameterized by the equatorial 2-sphere. The culmination of this work of Bott and Samelson was the famous periodicity theorems discovered by Bott, which he published in 1957, but these deserve a section of their own.

The Periodicity Theorems

Calculating the homotopy groups of spheres, and related spaces such as Lie groups, had become the fundamental goal of homotopy theorists. In the early days, and by fairly crude geometric methods, this was only possible for low dimensions. Serre's thesis had, in principle, provided powerful algebraic machinery for more extensive calculations, but these were tricky and delicate. By 1955 homotopy theorists had gotten as far as $\pi 10(SU(n))$, for n large, and found it to be cyclic of order 3. But early in 1957 Friedrich Hirzebruch and Armand Borel had concluded, through their quite independent computations, that the order of this group was a power of 2.

Such explicit contradictions are a challenge to mathematicians, and Bott felt that this one was right up his alley. He was sure that his methods would settle the issue, so he sat down with his friend Arnold Shapiro and calculated over an entire weekend. By Sunday evening they had adjudicated in favor of Borel and Hirzebruch. The homotopy theorists were wrong, reluctantly conceded defeat, and subsequently found their error. Serre, who was watching this battle from the sidelines, remarked "Quel dommage!" (What a pity!), observing tongue-in-cheek what a triumph it would have been for topology to be the first subject to demonstrate the inconsistency of mathematics!

This episode suggested to Bott that in fact all the high even-dimensional homotopy groups $\pi 2k(SU(n))$, for n larger than k, should be zero. Further examination of the evidence then suggested to him that the (stable) homotopy groups of all the classical groups should be periodic, with period 2 in the unitary case and period 8 in the orthogonal and symplectic cases. Moreover, he felt confident that his Morse theory techniques would yield a proof. By the summer of 1957 he had found the proof, which was then published.

This paper ("The stable homotopy of classical groups") was a bombshell. The results were beautiful, far-reaching, and totally unexpected. By using analysis Bott had proven results way out of reach of conventional calculations. His reputation was made, and shortly afterward (in 1960) he moved to Harvard, where he remained for the rest of his life.

At this stage I have to make the move from being the official writer of this memoir to becoming an active participant in the drama. I had gotten to know Raoul at the Institute for Advanced Study in Princeton, when I went there in 1955 after receiving my Ph.D. We were to go on to become lifelong friends and collaborators, publishing no less than 13 joint papers on a wide variety of topics and over many years. But our substantive collaborations really took off from the periodicity theorems and their development into K-theory.

Among the many new topics flourishing in the 1950s, algebraic geometry was sharing the stage with topology, again due in large part to J.-P. Serre, who had applied sheaf theory with Henri Cartan first in analytic geometry and then in algebraic geometry. The culmination of this work was the famous generalization of the classical Riemann–Roch theorem proved by Hirzebruch in December 1953. During the first of the influential Arbeitstagungen organized annually by Hirzebruch in Bonn, Alexandre Grothendieck expounded on his spectacular and beautiful generalization of the Hirzebruch theorem. This involved the introduction of the K-groups of an algebraic variety, groups whose definition was very abstract but that yet were simple and effective. Because I was, and remained, a regular attendee of the Bonn Arbeitstagung, I absorbed Grothendieck's K-theory and, when I heard of Bott's periodicity theorem, I eventually realized how to combine the two ideas. This led to "topological" K-theory, which I developed jointly with Hirzebruch and which rested in a fundamental way on Bott's periodicity theorem.

We needed Bott's help at this early stage, and he responded with a 1959 paper (Quelques remarques sur les théorèmes de periodicité) written, as he said later, "in fluent French." He went on (Macpherson 1994):

> *Alas, the French is not mine, and I am ashamed to see that there is no reference to the kind translator. Mathematically [the paper] deals with the 'new' K-theoretic formulation of the periodicity theorem. Grothendieck's K-theory and his brilliant functorial proof of Riemann–Roch in the algebraic category had a tremendous effect on all our thinking. Nevertheless, the ideas of Atiyah and Hirzebruch, interpreting the periodicity theorems as 'Kunneth' formulae in an 'extraordinary cohomology theory,' came as a complete surprise to me! In one swoop my special computations had become a potential tool in all aspects of topology.*

The periodicity theorem in the real case, with the period being 8, was subtler than the complex case when the periodicity was just 2. Bott and Shapiro had realized that this could best be understood through the structure of the Clifford algebras, which had the same periodicity in purely algebraic form. In 1964, in the paper "Clifford modules," I joined forces with Bott (Shapiro, sadly, having died) to clarify the way the algebra and the topology were linked. This has since proved useful in index theory.

K-theory and its further developments, including the index theorem, were at the center of my subsequent collaborations with Bott. But we were so close in our mathematical tastes that, over the years, every time we met (Figure 2) a new joint venture would start, as will become clear in the rest of this memoir.

Index theory

Figure 2. Raoul Bott with Michael Atiyah, Oxford, 1975.
(Photo courtesy the Bott family.)

In 1962–1963 Isadore Singer and I were working on the index theorem for elliptic differential operators on compact manifolds (Atiyah and Singer, 1963). This effort had many ramifications and was so close to Bott's interests that he soon joined our enterprise and played an active role in the many discussions that took place at Harvard and the Massachusetts Institute of Technology (MIT) during my two visits there in 1962 and 1964. He also spent the year with me in Oxford during 1965–1966.

Bott's first contribution to this area, published in 1965, was a direct geometric verification of the index theorem for holomorphic vector bundles over homogeneous spaces (The index theorem for homogeneous differential operators). This arose naturally from his experience with Lie groups. Another key contribution was to the index problems for manifolds with boundary (The index problem for manifolds with boundary, 1964), which required a deeper understanding of boundary value problems. As a byproduct we also produced an elementary proof of the periodicity theorem (On the periodicity theorem for complex vector bundles, 1964.) I remember planning a talk on this topic at MIT that was rather abstract. It was only Bott's insistence, at the eleventh hour, on searching out the essentials that made the talk genuinely "elementary."

The first major extension of the index theorem, published in 1966, concerned the interplay between elliptic operators and fixed points of maps (A Lefschetz fixed-point formula for elliptic differential operators.) It was inspired by questions that Bott and I were asked by G. Shimura at a conference in Woods Hole in 1964. The general formula that eventually emerged was similar in appearance to the famous Lefschetz fixed-point theorem, which relates the number of fixed points of a map to its action on cohomology. The elliptic version is a refinement in which the map is required to be compatible with the elliptic operator, and each fixed point contributes not an integer but a complex number calculated from the linearized action at the fixed point.

The fixed-point case has several beautiful applications, including a derivation of the Weyl formula for the characters of the irreducible representations of Lie groups. A very different application established an old conjecture of Paul Smith's. This asserts that if a cyclic group of odd prime order acts on a sphere with just two fixed points, then the linear actions on the tangent spaces at these points are isomorphic. It was this wide range of applications that made our fixed-point theorem one of Bott's favorite results.

Bott's final contribution in this area was to help clarify the heat-equation approach to the index formula. Earlier algebraic computations had been very complicated and difficult to understand. With the use of classical invariant theory, Bott, V. K. Patodi, and I were able to present in 1973 a conceptually simple proof (On the heat equation and the index theorem). Bott's expertise both in Riemannian geometry and invariant theory were crucial ingredients. The heat-equation proof of the index theorem has turned out to be very productive and, in particular, established a close link with work in theoretical physics. In subsequent years these links with physics were greatly strengthened, and they lay behind much of Bott's later work.

Equivariant cohomology

Bott's interest in Morse theory, together with his expertise in Lie groups, made it natural for him to be aware of the role of symmetry at critical points of functions or at fixed points of maps. One outcome of this was the systematic exploitation of equivariant cohomology in differential geometry.

Our joint 1984 paper (The moment map and equivariant cohomology) arose from our attempt to understand papers by Edward Witten on Morse theory, written from a physics perspective (Witten, 1982), and by Duistermaat and Heckman (1982) on the exactness of stationary-phase approximation. The methods were not really original but our presentation brought several strands together and has subsequently been influential.

Another of our joint papers, published in 1982 (The Yang–Mills equation over Riemann surfaces), also used equivariant cohomology but in an infinite dimensional context inspired by physics. The outcome was a new derivation of the cohomology of the moduli space of vector bundles over a compact Riemann surface. This topic has been at the center of much activity on the frontier between physics and geometry in recent years, and our paper has stimulated an extensive development.

Other results

My collaborations with Bott also included a digression into the question of lacunas for hyperbolic differential equations, published in 1970 (Lacunas for hyperbolic differential operators with constant coefficients,) an old topic going back to Christiaan Huygens and developed by Igor Petrovsky in the 1940s. The work of Petrovsky was difficult to follow and needed to be updated using the new topological methods of algebraic geometry. Bott and I were introduced to this problem by Lars Gårding, an authority on hyperbolic equations, and he essentially "commissioned" us to take on the project during a visit to Oxford in 1965.

An area in which Bott's careful approach and geometric insight paid dividends was his discovery that when a manifold is foliated, the bundle of tangent vectors to the leaves of the foliation cannot be an arbitrary subbundle of the tangent bundle but must satisfy some global topological conditions. This fairly simple observation generated a substantial follow-up, leading to a whole new theory.

In a somewhat related area Bott collaborated with Graeme Segal to study the cohomology of the Lie algebra of vector fields on a manifold, which had been introduced by Israel Gelfand and D. B. Fuks. The result, published in 1977 (The cohomology of the vector fields on a manifold,) was to express the vector field cohomology as that of a space naturally associated to the manifold and its tangent bundle. In a sense the outcome was disappointing in that the Gelfand–Fuks invariants gave nothing new—they could be identified as homotopy invariants.

A very different collaboration much earlier was that between Bott and Shing-Shen Chern, in which they set out to generalize the classical Nevanlinna theory of meromorphic functions to higher dimensions. The published result, Hermitian vector bundles and the equidistribution of the zeroes of their holomorphic sections, was published in 1965. A byproduct of their investigation was the Bott–Chern class, which is a complex refinement of the differential form of the Chern class. This has proved extremely useful in many subsequent developments.

In one of Bott's last significant papers, published in 1989 (On the rigidity theorems of Witten), he and Clifford Taubes gave an elegant mathematical proof of Witten's rigidity theorem (Witten, 1987). This was the fruit of the exciting interaction during preceding years between geometers and physicists—an effort in which Bott took an active part.

Bott's influence on theoretical physicists has been well described by Edward Witten:

> *I came to Harvard, where Raoul Bott was a professor, in the fall of 1976. This turned out to be just the period when physicists were starting to appreciate that a lot of modern mathematical ideas that we didn't know much about were relevant to understanding quantum gauge theories. Raoul did a lot to educate me and my contemporaries. He loved explaining things and had a knack for picking out the key point that would make a difficult subject clear. Later on, I and many other physicists learned much of our differential topology from the [1982] book [Differential forms in algebraic topology] by Bott and Loring Tu.*
>
> *In 1979, Raoul was invited to the summer school on particle physics at Cargèse, in Corsica. He began his lecture [Morse theoretic aspects of Yang–Mills theory, published in 1980] by saying that he was going to tell us about a favorite subject of his, which might be useful to us some day. The subject was Morse theory. Quite possibly, none of the physicists in attendance had ever heard of Morse theory, and certainly I hadn't. However, several years later, in studying a phenomenon known as spontaneous breaking of supersymmetry, I ran into some puzzling phenomena. At some point, a dim recollection of Bott's lecture sprang to mind and it became clear that the phenomenon in question was closely related to the fact that, in Morse theory, a critical point of a function can exist for a good topological reason. This led to my work (Witten, 1982) relating supersymmetry to Morse theory.*
>
> *In my paper on that subject, I tried to describe in terms of differential forms a second, superficially similar, supersymmetric construction. Unfortunately, the mathematical setting for this second construction was not clear. In hindsight, things might have been clearer if Bott had given another lecture in Cargèse! As it was, this construction was later put in its proper setting by Bott and Atiyah [The moment map and equivariant cohomology, published in 1984]. Bott went on to tutor me, and I believe other physicists, on the basics of equivariant cohomology, which has*

turned out to have numerous applications in gauge theory and topological field theory.

Students

Bott attracted a large number of talented students during his many years at Harvard. They gravitated toward him because of his friendly informal manner as well as his obvious passion for beautiful mathematics. Two of his students, Stephen Smale and Daniel Quillen, went on to win Fields medals, and many others had distinguished academic careers. Just before Bott died he confided to me, with modest pride, that he had received a letter from a recent Nobel laureate who told him how inspired he had been by Bott's teaching. This was George Akerlof (of the University of California, Berkeley), who shared the Nobel Prize in Economics for 2001. He wrote:

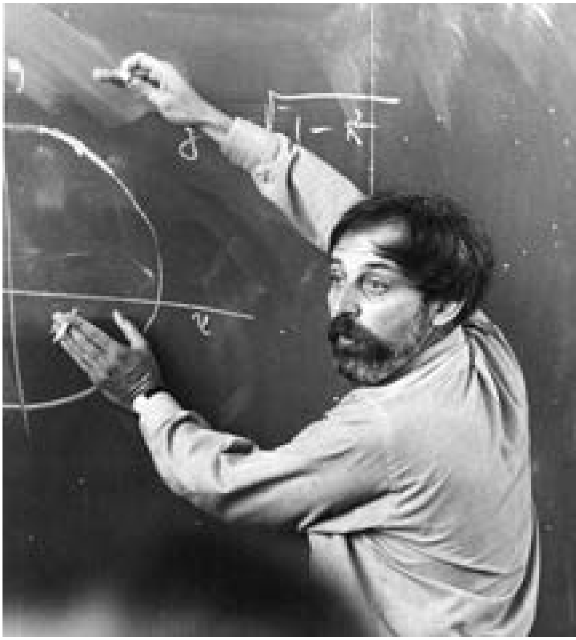

Figure 3. Raoul Bott lecturing at the University of Bonn in 1969.
(Photo by Wolfgang Vollrath.)

> *You probably do not remember but many years ago (39 to be exact) an economics student from MIT took your course in algebraic topology at Harvard. I was that student. I worked very hard in it and learned a great deal. You were not just a great teacher, but a fabulous teacher. What I learned in your course was the foundation of my whole career.*
>
> *You did not just teach the technical field of homotopy theory, but showed students how to decompose problems into their essentials and their technical details. This, of course, was the same skill that [Robert] Solow's papers demonstrated, and that I was learning separately in economics at MIT.*
>
> *This year I was named corecipient of the Nobel Prize in Economics. I merely applied to economics the common sense about mathematics that I had learned from you.*

> *I know that you are a great mathematician. I also want to thank you for being such a fine and caring teacher.*

Harvard has "houses" modeled on the Oxford and Cambridge colleges, and it was no surprise to Bott's friends when they heard that he had been appointed master of Dunster House. He and his wife Phyllis moved into the master's residence, overlooking the Charles River, and took an enthusiastic part in student life. Some years earlier he had been approached about the mastership of St Catherine's College, Oxford, where he had spent a sabbatical year and made a great impression. He declined the somewhat unrealistic offer but subsequently (1985) became an honorary fellow. In the same year he was Hardy lecturer of the London Mathematical Society.

Bott received many other honors and awards, including the Wolf Prize in Mathematics (2000) and the U.S. National Medal of Science (1987). He had honorary doctorates from Notre Dame, McGill, Carnegie Mellon, and Leicester Universities. He was a member of the National Academy of Sciences of the U. S. A. (elected in 1964) and a foreign associate of the French Academy of Sciences. Because he was too ill to come to London, arrangements were made so that I could admit him in California, shortly before he died, as a foreign member of the Royal Society.

Memorial

I can think of no better way to convey something of Bott's personality than to reproduce here the text of the address I gave at the Harvard Memorial Church on January 29, 2006, soon after his death:

> *I first met Raoul over 50 years ago at the Institute for Advanced Study in Princeton, and it was an indication of the important part that Princeton played in his life that, despite his illness, he came back there last March and we met again at (the 75th anniversary of the institute.*
>
> *Over those 50 years we became lifelong friends and worked together in many other places, including Harvard, Oxford, and Bonn, where we joined Fritz Hirzebruch's annual jamboree. We traveled the world together to conferences in exotic places— India, Mexico, China, Hungary. I recall an event in Budapest when our bus was held up by a total traffic snarl. When time passed and the deadlock continued, Raoul took charge. He stood in the middle of the road and with great authority acted as a*

Expounding ideas simply was supremely important to him. He was a born teacher who knew how to engage his audience, getting them involved so that they could really understand. It is no accident that Raoul attracted so many talented students who went on into successful careers. Unlike some great mathematicians, he did not try to intimidate his students by exposing their ignorance.

policeman, skillfully directing the traffic and unlocking the jam—which showed he was a real Hungarian!

Even as a young man Raoul exuded charm and made an immediate impression on all he met. I remember when he was interviewed in 1964 for a visiting fellowship at St. Catherine's, my Oxford college; the master, Alan Bullock, was so attracted to him that he [feared] Raoul would turn down the offer, [and] he was delighted when Raoul accepted.

In our early years together, despite the fact that he was six years older than me, we were colleagues on the same plane. Ironically, as we grew older the relationship subtly changed and he became more of an avuncular, or father, figure. I think it may have been the beard, but in fact he just grew into his natural role as a "paterfamilias." He had indeed a large loving family of children and, eventually, grandchildren, and he had a parallel family of students and grand-students. With his large towering frame and his wide embrace he was really in his element as the head of these large and extended families. I became part of this family circle, of which Phyllis was, of course, a central figure, sharing nearly 60 years of married life with Raoul and keeping him under friendly control with her quiet humor. It is very appropriate that a joint portrait of Raoul and Phyllis, as comasters, now hangs in Dunster House.

It was impossible to work with Raoul without becoming entranced by his personality. Work became a joy to be shared rather than a burden to bear. Historians and biographers frequently try to make a sharp distinction between the life and work of the creative artist. No such separation makes sense for Raoul— his personality overflowed into his work, into his relations with collaborators and students, into his lecturing style, and into his writing. Man and mathematician were happily fused.

This is not the place to describe Raoul's mathematics, but I should say something about the way he worked—his style. He loved to discuss mathematics, and we would spend happy hours together in front of a blackboard tossing ideas about and, at Raoul's insistence, doing calculations. While he liked to see the big picture, he was never happier than when he found a good example to work on in detail. He was suspicious of hand waving or airy-fairy speculation. To him mathematics was a craft, where the artisan lovingly carved his handiwork in beautiful detail.

Expounding ideas simply was supremely important to him. He was a born teacher who knew how to engage his audience, getting them involved so that they could really understand. It is no accident that Raoul attracted so many talented students who went on into successful careers. Unlike some great mathematicians, he did not try to intimidate his students by exposing their ignorance. On the contrary, he would descend to their level and provide encouragement and advice to suit the individual student. When attending a seminar he would frequently ask an elementary question, even when he himself knew the answer, in order to help the more inhibited students in the audience.

He had great sensitivity to people and situations. I remember one occasion when I wrote the draft introduction to a joint paper in which I referred to the "modest contribution" that each of us had made in earlier papers. He told me to remove the word "modest," saying it was false modesty. He was of course quite right: genuine modesty does not advertise itself.

Humor and laughter was an important part of Raoul's character. He enjoyed recounting amusing episodes of the past, such as the time Stephen Smale got them trapped between the rising tide and a sheer cliff, or the time when he arrived in India without a visa but was given red-carpet treatment—while on our return journey I was incarcerated in quarantine at Cairo airport!

In any group he was always the center of attraction—like the sun, he radiated warmth, and we planets circulated around. But beneath the jollity and humor there was a deeply serious side. On occasion Raoul would *turn on you a pensive penetrating look that seemed to see into your soul. He could see through pretensions or poses. He was anchored to the hard core of his beliefs, even though they rarely came to the surface. In Shakespearean terms, he was part Falstaff and part Hamlet but without the extremes of either, so that they happily coexisted.*

His love of beauty in mathematics was reflected in his deep love of music. His enjoyment of life found its counterpart in the sparkle of Mozart, while his more serious side found its solace in the spirituality of Bach.

All of us who knew Raoul understood what a marvelous person he was, and anything we say is inadequate. But let me give the last word to my son Robin who, as a young teenager said, after meeting Raoul, "Now I know what is meant by charisma."

ACKNOWLEDGEMENTS

Much of the material on Bott's early life came from his own reminiscences in Macpherson (1994). Additional material can also be found in Yau (2003).

I am also indebted to George Akerlof, Friedrich Hirzebruch, Graeme Segal, Jocelyn Scott, Loring Tu, and Edward Witten for their help.

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