Interview with Raoul Bott

This is the edited text of two interviews with Raoul Bott, conducted by Allyn Jackson in October 2000.

Raoul Bott is one of the outstanding researchers in geometry and topology in recent times. He has made important contributions to topology, Lie group theory, foliations and characteristic classes, K-theory and index theory, and many other areas of modern mathematics. One of his most spectacular successes was the application of Morse theory to the study of the homotopy groups of Lie groups, which yielded the Bott periodicity theorem. This central result has reappeared in many other contexts, including several versions of K-theory and noncommutative geometry.

Raoul Bott was born on September 24, 1923, in Budapest, Hungary. At McGill University he earned a bachelor's degree in 1945 and a master's degree in 1946, both in engineering. He then switched to mathematics and received his Sc.D. from the Carnegie Institute of Technology (now Carnegie Mellon University) in 1949. He spent the next two years at the Institute for Advanced Study in Princeton. From 1951 to 1959 he was at the University of Michigan, except for a stay at the Institute during 1955–57. In 1959 he accepted a professorship at Harvard University. He retired from Harvard in 1999. His honors include the AMS Oswald Veblen Prize (1964), the National Medal of Science (1987), the AMS Steele Prize for Lifetime Achievement (1990), and the Wolf Prize (2000).

Bott at McGill University, about 1942.

Notices: First let's talk a little bit about your early background. You had an unconventional education, and when you were a youngster you were not especially distinguished in mathematics.

Bott: That's putting it mildly!

Notices: But looking back now, do you see some experiences from that time that put you on the path to becoming a mathematician?

Bott: Well, I've always thought my interest in electricity was a manifestation of trying to understand something, but it certainly wasn't mathematics. When I was about twelve to fourteen years old, a friend and I had fun working with electricity, and it was really a collaboration. We had a lab where we tried to make very primitive things, such as a microphone. We enjoyed creating sparks, and we wanted to know how gadgets work. So I think this was closest to what really makes a mathematician—someone who likes to get at the root of things.

Notices: *This was much more practical than your mathematics is.*

Bott: Yes, it was definitely practical, and, although I wasn't so very good at it, I enjoyed working with my hands. I always said later on that I would have liked to live in the day of Marconi. I would have loved to invent things in a small lab, discover the basic properties of electromagnetism. I think that would have been wonderful.

Notices: *In mathematics you worked in pretty pure areas; you didn't work in applied areas.*

Bott: Well, after I got my degree in engineering, I went into applied mathematics. I solved a quite famous problem with my thesis director, Richard Duffin. The result is now called the Bott-Duffin theorem [1]. It was practical and was put to use by Bell Labs for a while.

Notices: *What did this theorem allow them to do?*

Bott: This problem had to do with building filters. In those days one didn't have transistors, so if one made electric circuits, one had only very standard objects: resistors, capacitors, and coils. If these elements are hooked up in an arbitrary fashion and placed into a "black box"—as it was called—so that one has only two terminals showing, then the steady-state frequency response of such a network is determined by a rational function of the frequency, called the "impedance" of the box. And because such a box contains no energy sources, this impedance has the crucial property that it maps the right half of the complex number plane into itself. So the mathematical problem was: Given such a rational function, can one build a black box for it? This was a very natural question, because the frequency response in a filter is the important thing: one wants certain frequencies to go through and others to be blocked.

This problem had fascinated me when I was at McGill, and I brought it to Carnegie Tech with me. In my first interview with Duffin I immediately divulged it to him, and he became interested. Actually, the problem had been nearly solved by Brune, who was a South African engineer, many years before. He had given an inductive procedure for building a black box, starting from such a rational function. Unfortunately, at one step in his procedure he had to introduce an "ideal transformer". His procedures were quite feasible, except for this one step. In practice, your black box would become as big as a house to accommodate an ideal transformer! So my dream was to get rid of these ideal transformers at the cost of making a more complicated network. And that is precisely what Duffin and I managed to do. This work wasn't my thesis, but it was much more interesting than my thesis, and it started my career, no question about it. The engineers were amazed, because they had written wrong papers on the subject for twenty years. Hermann Weyl heard about it when he

E. Pitcher, Johnson (first name unknown), R. Bott, H. Samelson, J. Nash, H. Rauch, 1956 AMS Conference in Seattle.

visited, and I am sure it was this theorem that brought me to the Institute in 1949.

Notices: *Were you aware of the literature that the engineers had written?*

Bott: No. Duffin and I didn't like to search through literature. We still don't! We thought one should be able to get rid of the ideal transformers, and we knew these other papers didn't do that. There is a tremendous literature in the mathematical world about functions that map the upper half-plane into itself—the moment problem—but nothing in those papers actually helped. The final proof of the theorem was really quite easy once we learned of a theorem of I. R. Richards in abstract complex variable theory. I recently asked my friend and colleague Curt McMullen to provide a proof for this Richards theorem, and he produced a purely algebraic proof from the Schwarz lemma, very ingeniously applied. The original version of the theorem seemed more complicated.

What I like about this work with Duffin is that it also brought about a wonderful moment of collaboration. We had been working on our problem with the Richards formula all afternoon, and it didn't seem to work. We then went home, and on the way I saw that of course it did work! So rushing home, I immediately called him up, but his phone was busy. He was calling me with the same insight!

Notices: *What area of mathematics was Duffin working in?*

Bott: He was a jack of many trades. He was a physicist to start with, and I liked to tease him that he did applied mathematics the wrong way around. He would take his physical intuition and try to make it mathematics.

Notices: *Why is that the "wrong way around"?*

Bott: Well, ideally, the physicists would like to have mathematics predict nature. There is something more exciting if you predict, in terms of mathematical ideas, a phenomenon that was unexpected. So I used to think of it as the wrong way

Raoul Bott lecturing at Universität Bonn, 1969.

around. But Duffin was definitely a master at it. And of course, as I said, he worked in many different areas. For instance, there is something called the Duffin basis in physics for spinors. He had also worked extensively in complex variable theory. He was an artist in a way. He wrote beautifully written, short papers. He was not a specialist at all, and that impressed me from the start.

Notices: You wrote in a Notices *article that you tried to emulate his way of being a "mathematical samurai".*

Bott: Yes, that's the point. It's the problem you go after rather than the field. You have to trust your instincts and hope that sometimes you will hit upon a subject to which you can maybe make a contribution.

Notices: Did you come into contact with John Nash at Carnegie Tech?

Bott: Yes, indeed. He was in my class. In fact, in this class there was Nash and also Hans Weinberger, a very good applied mathematician now at Minnesota, and maybe two or three others. Duffin was teaching us a very amusing course on Hilbert spaces. One of Duffin's principles was never to prepare a lecture! So we were allowed to see him get confused, and part of the fun was to see whether we could fix things up. We were reading von Neumann's book on quantum mechanics, which developed Hilbert spaces at the same time. And it soon became clear that Nash was ahead of all of us in understanding the subtleties of infinite-dimensional phenomena.

Notices: Was he an undergraduate?

Bott: He was an undergraduate, yes, and the rest of us were graduate students. I was friends with Nash; he didn't have any close friends, really, but we often talked about this and that. When he later got sick and had a really bad bout, he would sometimes send me a postcard with some very strange associations, usually with religious overtones. My closest contact with John was at Carnegie Tech. When I came to the Institute in Princeton, he came to Princeton as a graduate student, and then I only saw him casually. Later when he came to MIT and started his work in geometry, I unfortunately wasn't at Harvard yet. I would have been glad to have been part of the development of geometry by Ambrose and Singer at MIT at that time. However, this whole devel-

opment turned Nash off. Eventually he went to Ambrose and asked for a "real problem". And then of course Nash proved his remarkable embedding theorems. But I was at Michigan at that time. Unfortunately, Nash's great gifts were marred by his terrible disease.

Notices: After Carnegie Tech, you went to the Institute in 1949. You had been doing things associated with engineering up to that point. How did your perspective on mathematics change when you went to the Institute?

Bott: Well, I felt like a kid in a candy store. First of all, the people around me were so outstanding! It was a sort of Valhalla, with all these semigods around. Amazingly enough, we mathematicians have a type of negative feedback built into us: If we don't understand something, it makes us want to understand it all the more. So I went to lectures, most of them completely incomprehensible, and my gut reaction was: I want to understand this. Ostensibly I was at the Institute to write a book on network theory, but after I found out I didn't have to do that, I went to an incredible number of lectures and just absorbed the atmosphere. I didn't write a single paper in my first year there. So I was very delighted when Marston Morse called me up at the end of that year and said, "Do you want to stay for another year?" And I said, "Of course, yes!" He said, "Is your salary enough?" It was $300 a month. I said, "Certainly!" because I was so delighted to be able to stay another year. My wife took a dimmer view! But we managed.

Notices: So this was a big change for you, to go from an environment where you had been working on the engineering side to a place where there was so much mathematics.

Bott: I didn't think of it that way.

Notices: It wasn't such a contrast for you?

Bott: No, because the actual work is just the same. When I worked with Duffin, it was mathematical thought; only the concepts were different. But the actual finding of something new seems to me the same. And you see, the algebraic aspects of network theory were an ideal introduction to differential geometry and the de Rham theory and to what Hermann Weyl was studying at the time, that is, harmonic theory. In effect, networks are a discrete version of harmonic theory. So when I came to the Institute, the main seminar I attended was Hermann Weyl's, and Kodaira and de Rham were lecturing on harmonic forms. Weyl wanted to have, finally in 1949, a proof of Hodge's theorem that he could live with. Hodge's theorem was proved in the 1930s, but in a somewhat sloppy way. The details were cleaned up in this seminar by two quite different people from different points of view. So this didn't seem strange to me; it was within my domain of thinking. It then led to topology, and there my course with Steenrod was the dominant experience for my future development.

Notices: In your collected works Paul Baum wrote some reminiscences about working with you. One of the things he said he learned, or relearned, from you is that there is a mainstream to mathematics and that certain mathematicians like you understand instinctively what that mainstream is. How does one come to understand what that mainstream is? You're born with it? You learn it? You pick it up from the environment?

Bott: A good point. I must say I always followed my taste. And sometimes my taste led me in directions that weren't fashionable but that luckily turned out to be fashionable later on! But these things are dangerous, the fashions change, and it's hard to tell in retrospect whether you were in the mainstream. I was just very affected by the early development of sheaf theory, and especially the combination of analysis with topology that then ensued. Suddenly complex variable theory fitted in with topology and even certain aspects of number theory. So I think that at that time it was very easy to discern this development as a main road in mathematics.

But I've seen the mainstream change considerably over my lifetime. For instance, if I think of Princeton before sheaf theory, the emphasis was very different. When I first came there, much of topology in those early years had to do with very abstract questions of pathological spaces, comparing fifteen different cohomology theories, and such. This was what I would have said at first was the mainstream. Then topology moved more to what I felt was the real world: the study of compact manifolds and their invariants. Lower-dimensional topology was not emphasized then, but in the 1990s it came to the fore again. So there is really a tremendous difference in perspective over the years.

Notices: But isn't there a core of mathematics that is vital and lively, independent of fashion, and there are other fields that are more outlying; and one needs a sense of what is central and what perhaps is not so central?

Bott: I don't know to what extent I believe this. I think, for instance, that Bourbaki had that feeling, and I was always a little skeptical of Bourbaki. The subject is just too big. It doesn't just have one main road. There are too many unsuspected branches. So although I was in a sense very much influenced by Bourbaki, I don't really subscribe to the belief that there is just one way of looking at things. An example is what's happening in physics and mathematics right now. Physicists with a completely different intuition come up with things that we now find very fascinating. I believe in the virtue of quite different cultures affecting mathematics. If you had one really good main highway, it would be dangerous, because then everybody would be marching along it!

Notices: When you were in Princeton, was there any activity in relativity, or was Einstein working by himself?

Bott: When I came to the Institute, Oppenheimer had taken over, and he was very dominant in the physics community. He had a seminar that every physicist went to. We mathematicians always thought they ran off like sheep, for we would pick and choose our seminars! I felt that Einstein was pretty isolated, yes. I'm very surprised that in my own case I did not make a big effort to get close to him, because he had always been my hero, and as a young boy I wanted nothing more than to understand relativity. Also, we both liked the same music, we spoke the same languages—it would have been too easy to become a groupie. We had one or two exchanges, but they were always, "How do you do, the weather is nice...." However, at the Institute, I was much more interested in topology. And in a way, it's just as well, because what he was working on then has not been very helpful.

Bott, 1972.

Notices: That's interesting that you had so much in common with Einstein, and you could have gotten to know him more, but you didn't do that in part because you had so much in common with him. You wanted something different?

Bott: Einstein had an assistant before I arrived there, John Kemeny, who later became president of Dartmouth. Kemeny was Hungarian, like me. In fact, once Hermann Weyl mistook me for Kemeny, and I didn't want to become the second Kemeny! Maybe I am not very well cut out to be a disciple. And also, as I said, I was fascinated by topology.

At the Institute I had a marvelous tutor in topology in Ernst Specker. He was and is quite a salty character, and we got along famously. Ernst was a student of Heinz Hopf, and unfortunately—from my point of view—he eventually moved into logic. Reidemeister was lecturing to a small group, including me and Specker, on new things that Cartan was doing at the time. Reidemeister spoke in a fluent mixture of half English and half German, but for Specker and me this was not a problem, and those sessions were an inspiration to me.

From the Institute I went to Michigan and met Hans Samelson, who was also a student of Hopf. Samelson was a real master of geometry and Lie group theory. I learned a lot from him during the years we worked together. But again, it was a particular problem that brought me into Lie group theory rather than wanting to learn an area.
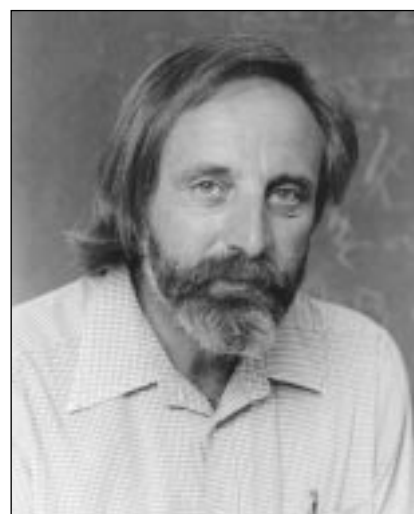
Bott and Michael Atiyah on the Rhine River, 1984.

Notices: If problems get you to learn about an area of mathematics, what guides you in your choice of problems?

Bott: It's hard to say. As in music, one falls in love with different things at different times. Right now for me it's the 4th partita of Bach. What brings on these impulses is hard to say.

Notices: Is there impetus in the opposite direction, that is, things you don't like in mathematics?

Bott: Often I don't like the way mathematics is presented. I like the old way of presenting things with an example that gives away the secret of the proof rather than dazzling the audience. I can't say that there is any mathematics that I don't like. But on the whole I like the problems to be concrete. I'm a bit of an engineer. For instance, in topology early on the questions were very concrete—we wanted to find a number!

Notices: Are you a geometric thinker? Do you visualize things a lot when you do mathematics?

Bott: My memory is definitely visual, but I also like formulas. I like the magical aspects of classical mathematics. My instinct is always to get as explicit as possible.

In most of my papers with Atiyah he would write the final drafts and his tendency was to make them more abstract. But when I worked with Chern, I wrote the final draft. Chern actually wrote a more down to earth version of our joint paper.

Notices: Is Chern even more of a "formulas man" than you are?

Bott: Oh, yes. I'm pretty bad, but he is even worse! It's strange that in some sense it was he who taught us to work conceptually, but in his own work his first proofs are nearly always computational.

Notices: Can you talk about some of your favorite results, things that you have a special fondness for?

Bott: I told you already about the first one, the work with Duffin. That was, I think, a nice piece of work and great fun to do. Later I was very fortunate to be the first to notice that the loop space of a Lie group is very easily attacked with Morse-theoretic methods [8]. It turns out that if you look at the loop space rather than at the group, then the so-called diagram of the group on the universal covering of its maximal torus plays an essential role. So you can read off topological properties of the loop space much more easily from the diagram of the group than you can read off things about the group itself. This insight was exciting. I found this relation sometime in the early 1950s at Michigan, and it is still one of my favorite formulas.

Now, the sad part of that story is that, if I had been as gifted and as thorough as Serre or somebody like that, I would have immediately discovered the periodicity theorem there and then. Well, not right then, but certainly during my subsequent work with Samelson, where we extended this insight on the loop space of a group to the larger class of symmetric spaces [22]. The techniques we learned there were all I needed for the periodicity theorem. But it took a few more years for the appropriate context to develop. This occurred in 1955–57, when I returned to the Institute.

During that period there was a controversy in homotopy theory. The question concerned the homotopy group of the unitary group in dimension 10. The homotopy theorists said it was Z_3. The results of Borel and Hirzebruch predicted it to be 0. This contradiction intrigued me, and I thought I should be able to say something about it using the Morse-theoretic techniques that Samelson and I had discovered. Finally I hit upon a very complicated method involving the exceptional group G_2 to check the conundrum independently. My good friend Arnold Shapiro and I spent all weekend computing. At the end we came out on the side of Borel and Hirzebruch, so I was convinced that they were right. And if they were right, the table of homotopy groups started to look periodic for a long stretch. In the odd dimensions they were Z up to nine dimensions, and in the even dimensions they were 0. So I thought, "Maybe they are periodic all the way." I remember suggesting this to Milnor. Well, Milnor doesn't like bombastic conjectures! He likes to be on firmer ground. And fairly soon after I saw that my old ideas would actually do the job.

In this way the unitary group was then settled. Then I started to think about the orthogonal group, and that was much harder. But I do remember precisely when I suddenly saw how to deal with it. That occurred after we had left the Institute and were moving house. You know how it is in mathematics: one suddenly understands something while one is unpacking one's books or doing something equally innocuous. In a flash I saw how it all fitted together [24].

Notices: From what you said, the periodicity theorem was hidden from everybody because of those mistakes in the original calculations. Nobody could have conjectured it.

Bott: Yes, especially the topologists and homotopy theorists who were led in a quite different direction by attacking the problem with the generally accepted method. On the other hand, I had the good luck of doing homotopy theory via Morse theory, which provided a quite different approach.

So that was really a high point, but it was a purely homotopy-theoretic result. By that time, the latter 1950s, I'd been invited to Bonn, and I had met Hirzebruch and learned all this wonderful stuff with the Riemann-Roch theorem, and those ideas started to fascinate me very much. Actually, that same year at the Institute I wrote a paper that I also like and that has been influential. It's called "Homogeneous vector bundles" [15], and it

computes the holomorphic cohomology of certain homogeneous spaces in a nice way. This paper was clearly influenced by what I learned at the Institute in 1956 from Borel, Hirzebruch, Serre, and Singer. The Riemann-Roch theorem conjectured something that I then proved on the actual cohomology level.

Notices: The Riemann-Roch theorem just gives the alternating sum of the dimensions. You computed each one individually in that case.

Bott: Yes. I related first of all the cohomology to some Lie algebra cohomology, and that was then very much more thoroughly investigated by Kostant later on. So there are various versions of this theory.

Notices: That is what's called the Bott-Borel-Weil theorem.

Bott: Yes. Then the next development was that Grothendieck came on the scene and influenced all of us tremendously. One day I received a paper from Atiyah and Hirzebruch about a generalized cohomology theory, now called topological K-theory. That paper was a revelation. Their approach had never occurred to me. It fitted in with the periodicity theorem but gave a completely new way of interpreting my computations. This was the start of my long and wonderful collaboration with Michael Atiyah. We first of all gave a new proof of the periodicity theorem which fitted into th K-theory framework [35]. Over the years he and various people have found more and more ways of doing this, completely different from my Morse theory way. K-theory then took off, and it was great fun to be involved in its development. Many famous old problems that had been difficult could be solved easily in K-theory. You see, in most cohomology theories, natural operations are hard to find and difficult to compute. But in K-theory, because you are dealing with vector bundles, exterior powers are very natural, and computing with them in this new setting turned out to be very effective.

In the early 1960s Atiyah and I were at Stanford, and we went to a cocktail party. Hörmander was there too. That was when I first heard the term *index* used in the sense that it's generally used in analysis, that is, as the index of an operator. I remember Michael was very, very interested in discussing the index with Hörmander. He stopped drinking and just talked to Hörmander. (But I continued my drinking. In fact, I was very nearly arrested that evening by a police officer! Luckily, I was able to squirm out of it.) Suddenly the Riemann-Roch theorem had taken a new turn. Hirzebruch's first run at it involved cobordism theory and all this beautiful algebraic geometry, and the index theorem of Hodge was the link between the topology and the analysis. That was very beautiful. Then Grothendieck, in the purely algebraic context, gave a completely different proof using his K-theory in the formal, algebraic way. Now

60th birthday conference of M. Atiyah, Oxford, 1989. With Bott on left: Lily Atiyah, on right: Rosemary Zeeman.

Photograph courtesy of M. Atiyah.

suddenly from the index point of view there seemed to be yet another approach to the same problem. Before, we had taken complex analysis or algebraic geometry as a given, so that the differential operator was hidden. Whereas here, suddenly the topological twisting of the differential operator came into the equation. Of course, Atiyah and Singer immediately realized that this twisting is measured with the homotopy groups of the classical groups, by the so-called symbol. Eventually the whole development of index theory fitted the periodicity theorem into the subject as an integral part. Atiyah very rightly chose Singer to collaborate on this project. I was working in a very different direction. I wanted to look at local fundamental solutions of differential equations and use them as the tool for proving the index theorem, as it was called, by patching these together in the "Čech manner".

In 1964 Michael and I were together again in Woods Hole, at an algebraic geometry conference. By that time, we had learned to define an elliptic complex, and we now saw the old de Rham theory in a new light: namely, that it satisfied the natural extension to vector bundles of the classical notion of ellipticity for a system of partial differential equations. During that conference we discovered our fixed point theorem, the Lefschetz fixed point theorem in this new context [42], [44]. This was a very pleasant insight. The number theorists at first told us we must be wrong, but then we turned out to be right. So we enjoyed that!

In a way, I always thought of the Lefschetz theorem as a natural first step on the way to the index theorem. You see, in the index theorem you compute the Lefschetz number of the identity map. The identity map has a very large fixed point set. So if you have the idea that the Lefschetz number has to do with fixed points, then of course it's much easier to first try and prove the Lefschetz theorem for a lower-dimensional fixed point set. The fixed point theorem we proved in Woods Hole dealt precisely with the case in which the fixed point set was zero dimensional. Over the years I've encouraged people to study it over bigger and bigger fixed

point sets and approach the final answer in this way. The analysis needed for the Lefschetz theorem in the case that we studied is very simple compared to the analysis needed for the true index theorem. Nevertheless, this special case fitted in nicely with many things, and we could use it also to prove some theorems about actions of finite groups on spheres and so on.

The next piece of work that I enjoyed very much was around 1977, when I came back from India and visited Atiyah in Oxford. During that visit I became aware of a new and exciting relationship between mathematics and physics. In this atmosphere we started to think about the problem of stable bundles over Riemann surfaces in terms of gauge theory. We had two ideas: first, that one had to use an equivariant version of Morse theory to tackle this problem. And second, that one then had to get at the final answer by a subtraction process. The salient feature of this work was that in the equivariant Morse theory the absolute minimum plays a very special role, in the sense that the higher critical points tend to be "self-completing". This paper [81] had connections to various other fields. On the one hand, it related to the stability theory of Mumford, and on the other, it had relations to the moment map and work of Guillemin and Sternberg. It was even related to some work of Harder and Narasimhan in number theory.

In recent times something that Atiyah and I discovered in the 1980s has been put to a lot of good use. It is called the equivariant fixed point theorem. Just recently it led to proofs of the so-called mirror conjecture in certain instances in the work of both the Russian and the Chinese schools.

But let me brag about another theorem! The question here had to do with foliations. A foliation is a subbundle of the tangent bundle that satisfies an integrability condition. To see the topological

Bott at his 70th birthday celebration at Harvard, 1993.

implication of that integrability condition seemed to me to be a very fascinating subject, and it still seems so today. In the late 1960s I was giving a course on characteristic classes, and, as is usual with me, I started from scratch, because I don't have notes and I don't like to read books. I did it slightly differently that time, because I was very influenced by the ideas of Haefliger. And then I soon noticed that integrability has a topological consequence. If you have a vector bundle that is a subbundle to the tangent bundle, then in its isomorphism class there's a definite obstruction to deforming it into an integrable one. A certain vanishing condition has to be satisfied by its characteristic classes. This work [52] then naturally led to the exotic characteristic classes of foliations, that is, generalizations of the Godbillon-Vey invariant, which were also discovered independently by Joseph Bernstein at the same time. I worked in this area with André Haefliger [56] for many years, and this was also a wonderful collaboration.

Notices: Had you encountered him at the Institute?

Bott: I did meet him in Princeton, but we were never both in residence there at same time. By the way, this whole area is also related to the so-called Gelfand-Fuks cohomology. Graeme Segal, with a little help from me, proved that actually this Gelfand-Fuks cohomology turns out to be a homotopy-invariant functor. Independent proofs were also given by others, including André. An exciting development in this area turned out to be the examples of Thurston, which showed that you could have pathological foliations. My work at that time was very much influenced by Graeme Segal and his ideas on simplicial spaces.

Notices: In your work with Duffin there were a lot of engineering papers that were wrong, but you were at first not aware of them. Later, the homotopy theorists made mistakes in their calculations, but this did not prevent you from finding the right answer. Do you think that, for example, had you known about the engineering papers, they would have stopped you from proving your theorem with Duffin?

Bott: That could very well have been. If either Duffin or I had researched the literature well enough, we would have found insurmountable problems! Although I think of myself as a rather sloppy guy, I have found errors quite often. So I'm skeptical. I do like to see the nitty-gritty of the proof. I like to understand things very much in detail. Sometimes my students get mad at me. A thesis has to be rewritten until it's an open book, so to speak. Otherwise I'm too stupid to understand it!

Notices: Your work has touched on a lot of different areas: topology, geometry, Lie groups, PDEs, analysis. But not number theory. Have you ever been interested in number theory?

Bott: Secretly, yes! In fact, I'm leaning in that direction right now. I'm interested in the papers of Candelas, who is a physicist. For example, he wrote one paper called "Calabi-Yau manifolds over finite fields". It really fascinated me this summer, so maybe in my old age...!

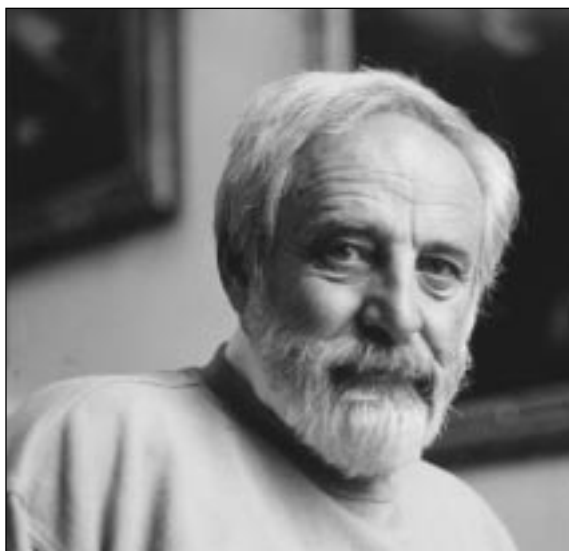*Notices: Will you try your hand at the Millennium Prize Problems? You could win a million in the process.*

Bott: No, I prefer doing the problems I dream up myself.

Notices: What do you make of offering these big prizes? Do you think it's good for mathematics?

Bott: Well, we are to a certain extent snobs and feel that there is something demeaning about bringing huge sums of money into the game. But on the other hand, it might bring some very gifted people into mathematics. For instance, during the Sputnik era the whole preoccupation with the Russians made theoretical subjects more exciting in America. At that time a group of very brilliant people went into mathematics. Today they might go into biology. So I do feel that publicity for mathematics is a good thing, but I wish it could be done in a less materialistic way. However, America is a pragmatic country and likes to look at the bottom line.

Notices: Going back to physics, it seems as though in mathematics, compared to physics, people are more individualistic. In physics, there are "tastemakers"; in mathematics, it's not like that. It's more diverse.

Bott: Thank God there are very good people in so many diverse areas that we have many more branches we can develop. This is true to a certain extent in physics too. There are the solid state people who don't care about fancy new stuff; they are fascinated by different aspects of physics. But physics is still much more circumscribed than mathematics. Physicists are in close contact with experiments, and we don't have this discipline. Some people have found it disconcerting that we are allowed to go so much in our own direction. They think we have too much license! And I must admit that my basic reaction to some mathematics lectures is, "Why in heaven's name are they doing this?!" But there are also very beautiful parts

of mathematics that are not at all appreciated at the moment and that I think will come back at some point.

Notices: What are you thinking of there?

Bott: That's hard to predict. But often some new development will at the same time resurrect old questions. However, there are also more pessimistic points of view. My friend Samelson always said, "Eventually mathematics will run out. We have been using the same ideas, the same basic things, for so long, eventually the oil will be gone." For example, Lie groups: you can trace them back to very early origins, and we've certainly mined them tremendously in this century. But I think there will always be some new slant that will keep us going.

For the truth of the matter is that there are tremendous mysteries out there, and their solution will lead us in quite new and unexpected directions. There was a show on TV yesterday about geysers in Yellowstone Park in Wyoming. There are thousands of these hot springs, where steam and water escape. Biologists found that things live in this boiling water! They found living things in a geyser—in a very deep, black hole, without any light, at temperatures and in chemical solutions that were considered anathema to life! So I do believe that the universe will have enough for us to work on for a long time.

I'm very glad I went into mathematics, and I'm certainly surprised it worked out so pleasantly. What's so wonderful in our field is the tremendous collaboration that goes on, that we enjoy showing our wares to each other and that we by and large don't fight as much about it as in most other fields. This is very rare, really; I think you don't find it in literature, or biology, or history. They don't spend half their time in other people's lectures. We are allowed to learn from each other, and although we do give credit, we also often learn much more than can be easily credited. With one offhand remark we give away our insight of years of thinking, and such a remark might illuminate a whole field or fit into one's brain just right to unlock some new insight. We do this very generously with each other.

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The reference numbers in this article correspond to the numbering in *Raoul Bott: Collected Papers*, volumes 1–4, Robert D. MacPherson, Editor, Birkhäuser, 1994.

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About the Cover

The cover photograph captures Raoul Bott at a characteristic moment, doing what he has been very, very good at all his professional life—explaining mathematics. It was Friedrich Hirzebruch who brought the photograph to our attention, and who sent along also a copy of the article in the *Unabhängige Westdeutsche Landeszeitung* of June 25, 1969, in which this picture first appeared. According to that article, Bott is lecturing to a group of undergraduates about vector fields on manifolds, which is not apparent from the picture itself.

The photographer was Wolfgang Vollrath, now working at Leica Microsystems and then in his third term as a physics student at the University of Bonn. Dr. Vollrath writes, "At that time I was attending a lecture course in linear algebra given by Prof. Hirzebruch. He used to organize once a year a symposium of very high reputation at the Mathematisches Institut of the University of Bonn. In 1969 Prof. Hirzebruch had the great idea to ask some of the symposium lecturers…to give readily comprehensible talks to the younger students. One of the lecturers was Raoul Bott. Most fascinating for the German students, however, was not the lecture itself, but that he was smoking…while he was talking and at the same time writing and wiping on the black board. This was inconceivable for German students. We enjoyed it very much. I was sitting in the audience with my camera and a telephoto lens on it and could hardly believe what I was seeing."

—*Bill Casselman* (covers@ams.org)