An Interview With Michael Atiyah

Michael Atiyah was born in 1929 and received his B.A. and Ph.D. from Trinity College, Cambridge (1952, 1955). During his career he has been Savilian Professor of Geometry at Oxford (1963–69) and Professor of Mathematics at the Institute for Advanced Study in Princeton (1969–72); he is currently a Royal Society Research Professor of Mathematics at Oxford University.

Among other honors Professor Atiyah is a Fellow of the Royal Society and a member of the National Academies of France, Sweden, and the USA. He received the Fields Medal at the International Congress of Mathematicians held in Moscow, 1966. His research interests span a broad area of mathematics including topology, geometry, differential equations and mathematical physics.

The following is an edited version of an interview in Oxford with Roberto Minio, former editor of The Intelligencer.

MINIO: I think some information about your background might be [valuable]. When did you start getting interested in mathematics? How early?

ATIYAH: I think I was always interested in maths from a very young age. But there was a stage—I was about fifteen—when I got very interested in chemistry, and I thought that would be a great thing; after about a year of advanced chemistry I decided that it wasn't what I wanted to do and I went back to maths. I never seriously considered doing anything else.

MINIO: And that was already noticeable very early?

ATIYAH: Yes, I think so. My parents always thought that I was cut out to be a mathematician from a very young age, all the way through.

MINIO: But they weren't mathematicians?

ATIYAH: They weren't mathematicians, no.

MINIO: And were you helped at school? Were your teachers reasonably good with you?

ATIYAH: Well, I think I had good teachers and my relations with them were good. I was at school first in Egypt and I went to quite a good school there.

MINIO: Were you born there?

ATIYAH: No, I was born in England, but we lived in the Middle East—my father worked in the Sudan—so I went to school in Egypt for my main secondary schooling. I did a couple of years in England when I came over after the war—that was a good school. There were a lot of good pupils there. Then I went to Cambridge, and there were many good students around.

I don't think I was particularly influenced by one person. But I had a good education, and I had plenty of opportunity to meet good mathematicians; I had a good background in that sense.

MINIO: Did you largely work on your own at Cambridge?

ATIYAH: Well, I came to Cambridge after I had two years of National Service: it was a marvellous contrast. Actually, I went to Cambridge a little early in the academic year. I had a summer term there and the lovely weather and beautiful surrounding made a tremendous impression on me. I used to enjoy just going into the library to read, being surrounded by all of those books. It was an impressive atmosphere; it captured my imagination.

There were a lot of very bright students there and I got reasonable help from the teachers. I don't think any of the teachers was particularly inspiring; some of the lectures were good and some were not so good.

MINIO: One of your early papers was with Hodge, is that right?

ATIYAH: Yes, that was part of my thesis really. He was my research supervisor, and that was very important for me—working with him. I'd come up to Cambridge at a time when the emphasis in geometry was on classical projective algebraic geometry of the old-fashioned type, which I thoroughly enjoyed. I would have gone on working in that area except that Hodge represented a more modern point of view—differential geometry in relation to topology; I recognized that. It was a very important decision for me. I could have worked in more traditional things, but I think that it was a wise choice, and by working with him I got much more involved with modern ideas. He gave me good advice and at one stage we collaborated together. There was some recent work in France at the time on sheaf theory. I got interested in it, he got interested in it, and we worked together and wrote a joint paper which was part of my thesis. That was very beneficial for me.

MINIO: One thing that's noticeable is that you worked quite a lot with other people—with Singer, with Hirzebruch, with Bott.

ATIYAH: Yes, that's right, I work a lot with people, and I think that that's my style. There are various reasons, one of which is that I dabble in a number of different areas. My interest is in the fact that things in different subjects interact; it's very helpful to work with other people who know a bit more about something else and complement your interest. I find it very stimulating to exchange ideas with other people.

I've collaborated with many people, some of them—many of them—on an extended basis for many years. It's partly my personality, the way I think and interact with people, and partly because of the kind of mathematics I like doing, which is rather broad and therefore hard to be completely expert in. It is very helpful to have someone else who knows a bit more about something different. So when I work with Singer, for example, he's much stronger on the analysis side, where I was rather weak, and I know more about the algebraic geometry and topology.

MINIO: Do you then separate out the problems.

ATIYAH: No, no. The collaboration is completely intermingled; we merge our interests and learn about each other's techniques. After a while we're more on an equal footing in most parts of the subjects. Our interests are very close; it's just that our backgrounds are a bit different.

MINIO: How do you select a problem to study?

ATIYAH: I think that presupposes an answer. I don't think that's the way I work at all. Some people may sit back and say, "I want to solve this problem" and they sit down and say, "How do I solve this problem." I don't. I just move around in the mathematical waters, thinking about things, being curious, interested, talking to people, stirring up ideas; things emerge and I follow them up. Or I see something which connects up with something else I know about, and I try to put them together and things develop. I have practically never started off with any idea of what I'm going to be doing or where it's going to go. I'm interested in mathematics; I talk, I learn, I discuss and then interesting questions simply emerge. I have never started off with a particular goal, except the goal of understanding mathematics.

MINIO: Is that how K-Theory emerged?

ATIYAH: Yes. It was very much an accident in some ways. I was interested in what Grothendieck had been doing in algebraic geometry. Having gone to Bonn I was interested in learning some topology. I was interested in some of the questions that Ioan James had been studying on topological problems related to projective spaces. I found that by using Grothendieck's formulas these things could be explained, one got nice results. There was Bott's work on the periodicity theorem; I knew him and his work. Using this I found that one could solve some interesting problems. It seemed necessary to develop some machinery to make this formal, and K-Theory grew out of that.

You can't develop completely new ideas or theories by predicting them in advance. Inherently, they have to emerge by intelligently looking at a collection of problems. But different people must work in different ways. Some people decide that there is a fundamental problem that they want to solve, such as the resolution of singularities or the classification of finite simple

Pictured from left to right: **Montgomery, Spencer, de Rham, Mrs. Garding, Lars Garding, Chandrasekharan, Bott,** and **Atiyah.** Bombay, 1963.

groups. They spend a large part of their life devoted to working towards this end. I've never done that, partly because that requires a single-minded devotion to one topic which is a tremendous gamble.

It also requires a single-minded approach, by direct onslaught, which means you have to be tremendously expert at using technical tools. Now some people are very good at that; I'm not really. My expertise is to skirt the problem, to go around the problem, behind the problem . . . and so the problem disappears.

MINIO: Do you feel there are mainstream topics in mathematics? Are some subjects more important than others?

ATIYAH: Yes, well I think that is true. I strongly disagree with the view that mathematics is simply a collection of separate subjects, that you can invent a new branch of mathematics by writing down axioms 1, 2, 3 and going away and working on your own. Mathematics is much more of an organic development. It has a long history of connections with the past and connections with other subjects.

Hardcore mathematics is, in some sense, the same as it has always been. It is concerned with problems that have arisen from the actual physical world and other problems inside mathematics having to do with numbers and basic calculations, solving equations. This has always been the main part of mathematics. Any development that sheds light on these topics is an important part of mathematics.

Parts that go off and are very far removed from these, and don't shed much light on the essentials of mathematics, are unlikely to be important. It may be that a new branch grows on its own and can eventually cast light on other things, but if it goes too far away and gets cut off, then it really isn't very significant in mathematical terms. There are really original ideas which may, for a while, open up new things, but still they are connected with other important parts of mathematics and interact. The importance of a part of mathematics is something one can judge roughly by the amount of interaction it has with other parts of the subject. It is a kind of self-consistent definition of importance.

MINIO: Isn't it possible though that something in fact has no impact for quite a while, but then many years later is taken up?

ATIYAH: Well, I think it is true that somebody may have a mathematical idea which is in advance of its time, and it may be that someone makes a clever suggestion and people don't see its significance for a long period of time. It obviously does happen.

I wasn't thinking quite so much of things like that. I was thinking more of the tendency today for people to develop whole areas of mathematics on their own, in a rather abstract fashion. They just go on beavering away. If you ask what is it all for, what is its significance, what does it connect with, you find that they don't know.

MINIO: Do you feel like giving an example?

ATIYAH: There are some examples in all parts of modern mathematics: some parts of abstract algebra, some parts of functional analysis, some parts of general topology—those parts where one sees the axiomatic method at its worst.

Axioms are designed to temporarily isolate a class of problems for which you can then develop techniques of solution. Some people think of axioms as a way of defining a whole area of mathematics that is self-contained. That I think is wrong. The narrower the axioms, the more you cut out.

When you abstract something in mathematics you separate out what you want to concentrate on and what you regard as irrelevant. Now that may be convenient for a while; it concentrates the mind. But by definition, you have cut away a lot of things you said you're not interested in and, in the long run, that has cut a lot of roots. If you can develop something axiomatically, then at some stage you should return it to its origin, merging and producing cross-fertilization. That's healthy.

You will find views like these expressed by von Neumann and Hermann Weyl, some thirty years ago. They worried about the way mathematics might be going; if it goes too far away from its sources then it might become sterile. I think that is fundamentally correct.

MINIO: It's clear that you have a strong feeling for the unity of mathematics. How much do you think that is a result of the way you work and your own personal involvement in mathematics?

ATIYAH: It is very hard to separate your personality from what you think about mathematics. I believe that it is very important that mathematics should be thought of as a unity. And the way I work reflects that; which comes first is difficult to say. I find the interactions between the different parts of mathematics interesting. The richness of the subject comes from this complexity, not from the pure strand and isolated specialization.

But there are philosophical and social arguments as well. Why do we do mathematics? We mainly do mathematics because we *enjoy* doing mathematics. But in a deeper sense, why should we be paid to do mathematics? If one asks for the justification for that, then I think one has to take the view that mathematics is part of the general scientific culture. We are contributing to a whole, organic collection of ideas, even if the part of mathematics which I'm doing now is not of direct relevance and usefulness to other people. If mathematics is an integrated body of thought, and every part is potentially useful to every other part, then we are all contributing to a common objective.

If mathematics is to be thought of as fragmented specializations, all going off independently and justifying themselves, then it is very hard to argue why people should be paid to do this. We are not entertainers, like tennis players. The only justification is that it is a real contribution to human thought. Even if I'm not directly working in applied mathematics, I feel that I'm contributing to the sort of mathematics that can and will be useful for people who are interested in applying mathematics to other things.

Everybody has to try to justify his life philosphically, to himself at least. If you are teaching you can say, "Well, my job is to teach, I turn out educated young people and I am paid for that. Research I do in my spare time and they allow me to do that out of generosity." But if you're a full-time researcher, then you've got to think much harder about justifying your work.

In some sense, I still do mathematics because I enjoy doing it. I'm glad that people pay me to do what I enjoy. But I try to feel that there is a serious side to it which provides a justification.

MINIO: What do you think about statements like "pure mathematics isn't very useful and within five years everybody will be just computing"?

ATIYAH: There's always a danger in such a point of view. If pure mathematicians take an ivory tower attitude, don't think about their relationship with other subjects, there is the danger that people will eventually turn around and say, "We don't really need you—you are a luxury—and we will employ people doing much more practical things." I think that is a danger that is always there and becomes much more serious in times of financial difficulty, such as we are going through now. And I think the message is beginning to come through.

Certainly in the last five or ten years there has been quite a growing appreciation among pure mathematicians that they have to justify themselves a bit more. But I still think with many people this hasn't come naturally; they have only done it under pressure. I think it would be healthy if pure mathematicians in general were more self-critical.

MINIO: Back to the mathematics that you do. Is there a theorem that you are most happy to have proved?

ATIYAH: I think so. The Index Theorem I proved with Singer is in many ways the clearest single thing I've done. I really think the Index Theorem is a nice, clear theorem which one can point to. Most of my work centers around it in one form or another.

It started from work in topology and algebraic geometry, but then it has had quite an impact on functional analysis; over the last ten years this aspect has been developed by many people. And also it is now being realized that it has interesting connections with mathematical physics. So it's still developing and active in many ways. It symbolizes, in a way, my main interest which is the interactions and connections between all parts of mathematics. It is an area in which algebraic topology and analysis come together in a very natural way, along with differential equations in various forms.

MINIO: Did you anticipate the recent revived interest in mathematical physics among mathematicians?

ATIYAH: Not really. I have had an interest in mathematical physics for quite a long time. Not very deep—I tried to understand quantum mechanics and related topics. But what has happened in the last five years—the interest of mathematicians in Gauge theories—was unexpected for me. I didn't know enough physics to know it was likely. Quantum Field Theory was one of those big mystic phrases as far as I was concerned.

I think the physicists themselves were surprised. The fact that the geometrical aspect became significant and dominant wasn't predicted by many of them (and is still disputed by some!). The main problems looked as though they were different—analytical questions, algebraic problems. Some people like Roger Penrose weren't really surprised. They had been working in it from their own point of view for a long time anyway. But I think it's a nice example: if you do interesting, basic mathematical work in the mainstream, then you shouldn't be surprised when others find it a useful tool. It justifies one's belief in the unity of mathematics, including physics.

MINIO: How far would you go with that statement?

ATIYAH: The more I've learned about physics, the more convinced I am that physics provides, in a sense, the deepest applications of mathematics. The mathematical problems that have been solved or techniques that have arisen out of physics in the past have been the lifeblood of mathematics. And it's still true. The problems that physicists tackle are extremely interesting, difficult, challenging problems from a mathematical point of view. I think more mathematicians ought to be involved in and try to learn about some parts of physics; they should try to bring new mathematical techniques into conjunction with physical problems.

Physics is very sophisticated. It is tremendously mathematical and the combination of physical insight, on the one hand, and mathematical technique, on the other, is a very deep connection between the subjects. Newer applications of mathematics, say in the social sciences, economics, computing, are important. It is important that we turn out students with this view of applied mathematics because this is what is required in the commercial world; thousands and thousands of students require this.

On the other hand, from the point of view of the depth of mathematics involved, there is no comparison. Although there are interesting questions in say economics and statistics, broadly speaking, the depth of mathematics involved is very shallow. The really deep questions are still in the physical sciences. For the health of mathematics at its research level, I think it is very important to maintain that link as much as possible.

MINIO: You are obviously interested in education. On the other hand, as far as your job is concerned, you are very clearly a research mathematician. How do you explain this?

ATIYAH: My reasons for an interest in education are the same as my reasons for an interest in the unity of mathematics. Universities are institutions that are educational *and* involved in research. I think that is very important—there should be unity in the university and unity in the whole social structure that attempts to keep a broad balance between mathematical research and mathematical education. And when universities give courses for educational purposes, they should be sure that they are performing the right task for the students, not just giving courses in (say) advanced topology because they are interested in turning out research students. That's a disastrous mistake.

Universities must try to balance two activities. They ought to know what's useful for students to learn, bearing in mind what they are going to be doing later on. At the same time, they ought to foster research. Some people will be doing all research and some people will be mostly teaching, and mainly people will be in between. Although I am only involved with the research end of it, I live in the university, I have colleagues in the university, I know what they are involved with, so I am concerned to see that a proper balance is struck between the different functions of the university.

MINIO: Do you think there was too much expansion of the universities in Britain during the last twenty years?

ATIYAH: I don't think there was too much expansion. By comparison with other countries, particularly America, it is clear that the number of people going on to higher education was really very small and ought to have been increased; fundamentally it ought to continue to expand. I can't believe that for the next century the proportion of people getting higher education will stay the same as it is now. It is bound to change.

When you have a period of rapid expansion, which was necessary after the war, it does produce some problems. It produces a discontinuity. You recruit a lot of people to teach in the universities, and when the expansion stops you have filled up all of the positions—you can't appoint any young people. One can criticize the overenthusiasm or lack of caution which the universities had in not foreseeing some of the ultimate difficulties which were going to emerge. For example, unlike American universities, English universities in this period of expansion gave tenure appointments immediately after the Ph.D. because the universities were competing with each other. I think that was a mistake and they are paying for it now.

It would have been wiser not to have given people lifetime positions from the moment they arrived but to have had some more flexible system which enabled them to adapt. Now we are getting a very sharp discontinuity and it's going to produce a crisis and confrontations. Perhaps people in the universities ought to be a bit more cautious.

MINIO: Back for a moment to teaching and research. You talked about both being essential parts of the university life, but you talk about them still quite separately. There has been a growth of research institutes—Bonn, Warwick, Princeton—at which there is no teaching. Do you find that a healthy development?

ATIYAH: I think the first thing to say about these kinds of institutions is that they have either no permanent staff or they have a very small permanent staff. Most of the people who go there are going there on refresher courses. They are going from the universities to spend a term or a year there and then they go back. So these are a kind of generalized conference centre, where people get together to exchange ideas and then go back to carry on their work. They are simply helping to keep people in universities actively interested in their research—that is their main function.

On the other hand, if you had a system like Eastern Europe, where they have big research institutions employing large numbers of people on a permanent basis, siphoning off from the universities a significant percentage of their staff—that raises different problems. Then you really are separating the university from research in a big way. But in mathematics the number of these centres is so small, the number of staff is minute, and the people going and coming back are simply strengthening the university system. I think that is quite healthy.

They can also serve another purpose which is to help to orient or steer or guide people into profitable areas of mathematical thought. In addition to going to a centre to boost your own current work, you can go to a centre of this type as a young man in order to be led into productive areas of research.

The institute at Princeton, which is where I went after I took my Ph.D., serves that purpose very well. I had done my Ph.D., written my thesis, but I was still looking around for a niche in life mathematically. I didn't know where I was going, what I was going to be doing. I went to this large centre where there were lots of very able young men, and older men, from different parts of the world with lots of different ideas. After a year or so there, I came away full of new ideas and new directions. That had an enormous bearing on my subsequent mathematical development.

MINIO: Who was it at Princeton who influenced you most?

ATIYAH: Well, I think it wasn't so much the permanent staff. I went in 1955 and many of the people there were probably slightly older than the comparable people who go now.

I met Hirzebruch, Serre, Bott, Singer . . . I met them all when I was at the institute. Kodaira and Spencer were there too. That whole group of people—I got to know them, and I was influenced by their mathematics. It's not an accident that I've collaborated subsequently with these same people I met at the institute.

I think there is another aspect. Not only do you alter your point of view and your work, but it puts you in contact with other active people and you keep up these personal contacts; they are very important in maintaining your mathematical development afterwards. Meeting people from different countries is important—mathematics is very international and these centres provide an opportunity which is very hard to get otherwise.

MINIO: Conferences also provide an opportunity for meeting people, but maybe less of an opportunity for working together and really learning things?

ATIYAH: Yes, conferences are very useful, but probably not so useful for the young person starting off. They are useful for the person who is established. If you know other people well already, and you are active, then in a very short time you can benefit from a rapid exchange of ideas. If you are a young student, or post-doctoral, you can't really talk to a lot of people because you don't really know them, you are inhibited, and also you don't understand enough to follow what they are saying. Then you need much longer exposure, I think. You need a year or so to absorb things slowly, to get to know people well. So I think conferences fulfill different functions.

MINIO: What about the International Congress?

ATIYAH: Well, I think the International Congress is totally different. I've been to every International Congress since 1954, I think; the benefit I have derived from those is very mixed.

The first one, which I attended as a young student, was great. I had a chance to hear Hermann Weyl give a talk and it was a tremendous psychological boost. I felt I was one of a large community of several thousand mathematicians. I didn't understand most of the talks. I'd go to them and be lost. I don't think I gained any-

M. Atiyah (right) talks to **F. Hirzebruch** (center). **Michael Artin** is pictured on left.

thing in terms of concrete mathematical understanding, but the psychological boost was substantial.

Now as I get older the International Congress is of little value. I go out of a sense of duty—I have functions to perform—to talk to people, to give lectures. I don't really benefit because there are so many people. Some lectures I quite enjoy; I think the International Congress has some benefit, but not very much.

Besides the benefit they give to young people, giving them some sense of international identity, their main function is probably to help people from countries outside the small circuit of very active mathematical countries. If you come from Western Europe or the United States they are probably not very essential. But if you come from Africa or Asia or Eastern Europe, where the opportunity for travelling and meeting people is much less, then I think it is the one chance you have of seeing what is going on. I suspect that is its main justification.

MINIO: Do you think that the Fields Medals serve a useful function?

ATIYAH: Well, I suppose in some minor way. I think it's a good thing that Fields Medals are not like the Nobel prizes. The Nobel prizes distort science very badly, especially physics. The prestige that goes with the Nobel prizes, and the hooplah that goes with them, and the way universities buy up Nobel prizemen—that is terribly discontinuous. The difference between someone getting a prize and not getting one is a toss-up—it is a very artificial distinction. Yet, if you get the Nobel Prize and I don't, then you get twice the salary and your university builds you a big lab; I think that is very unfortunate.

But in mathematics the Fields Medals don't have any effect at all, so they don't have a negative effect. They are given to young people and are meant to be a form of encouragement to them and to the mathematical world as a whole.

I was encouraged by getting a Fields Medal. It helped my self-confidence, my morale. I don't know whether if I hadn't got a Medal it would have been any different, but certainly getting it at that stage gave me encouragement and made me enthusiastic. So I think in that sense it can help.

I found out that in a few countries the Medals have a lot of prestige—for example, Japan. Getting a Fields Medal in Japan is like getting a Nobel Prize. So when I go to Japan and am introduced, I feel like a Nobel Prize winner. But in this country, nobody notices at all.

MINIO: Do you find that mathematicians generally are treated significantly differently in different countries?

ATIYAH: Well mathematics can mean slightly different things in different countries, of course. Particularly the division between mathematics, applied mathematics, and physics is quite different in this country; in most other countries, pure mathematics is much more separated. This probably has a general effect on what people think about mathematicians; they don't identify them quite so narrowly here with pure mathematics as they do in America, where mathematician means pure mathematician.

Otherwise, I suppose it is true in France that mathematicians have a higher status, traditionally. That is because France has a tradition of assigning higher status to philosophy, literature, and the arts, and mathematics belongs to that group. Whereas in this country, they never attach much weight to these things.

In Germany also professors traditionally had a higher status, though that is now rapidly changing.

I think there are obvious national differences, about how people view mathematics or universities. But that is changing—the distinctions between different cultures are blurring.

MINIO: I have a few questions here about how you work. For example, what sort of mental images do you use?

ATIYAH: I'm not sure I know the answer. I think I do have a visual picture in my mind sometimes, some schematic diagram. But whether that is really a picture or whether it is purely symbolic I don't know. I think it is a very difficult question, more to do with the general nature of psychology than mathematics.

MINIO: I suppose the question was meant to draw a distinction between geometrical intuition and algebraic manipulation.

ATIYAH: Yes, there are differences there. I suspect that dichotomy is quite real in the brain. I work with things that are more geometrical but I'm not the sort of person like Thurston who sees complicated, multidimensional geometry in that way. My geometry is rather more formal. But I'm not an algebraist either—I don't just enjoy manipulations. Perhaps I am not sufficiently extremal for the psychology to be evident; I'm sort of the ordinary man in the middle.

If you ask Thurston, perhaps he says that he does see complicated pictures in his mind and all he has to do is draw it on paper to get the proof. Ask Thompson how he sees a group; I don't know what his answer is. There are differences. It is a complicated question, but it is three-quarters psychology and only one-quarter mathematics.

MINIO: How important is memory for your work?

ATIYAH: I mentioned that when I was fifteen I was very keen on doing chemistry. I did a whole year of chemistry, then I gave it up for the simple reason that in chemistry you have to memorize vast amounts of facts. There were big books I had in inorganic chemistry and I simply had to remember that you produced different chemicals out of different substances by various processes. The amount of structure that helped you to remember these things was infinitesimal. Organic chemistry was a bit better. Compared with this, in mathematics you needed practically no memory at all. You didn't need to memorize facts; all you needed to do was to have an understanding of the way the whole thing fitted together. So I think, in that sense, mathematicians don't really need the sort of memory that scientists or medical students do.

Memory is important in mathematics in a different way. I will be thinking about something and suddenly it will dawn on me that this is related to something else I heard about last week, last month, talking to somebody. Much of my work has come that way. I go around shopping, talking to people, I get their ideas, half understood, pigeon-holed in the back of my mind, I have this vast card-index of bits of mathematics from all of those areas. So I think memory plays a role in mathematics, but it is a different sort of memory than you would use in other areas.

MINIO: When you're working do you know if a result is true even if you don't have a proof?

ATIYAH: To answer that question I should first point out that I don't work by trying to solve problems. If I'm interested in some topic then I just try to understand it; I just go on thinking about it and trying to dig down deeper and deeper. If I understand it, then I know what is right and what is not right.

Of course it is also possible that your understanding has been faulty, and you thought you understood it but it turns out eventually that you were wrong. Broadly speaking, once you really feel that you understand something and you have enough experience with that type of question through lots of examples and through connections with other things, you get a feeling for what is going on and what ought to be right. And then the question is: How do you actually prove it? That may take a long time.

The Index Theorem, for example, was formulated and we knew it should be true. But it took us a couple of years to get a proof. That was partly because different techniques were involved, and I had to learn some new things to get the proof, in that case several proofs. I don't pay very much attention to the importance of proofs. I think it is more important to understand something.

MINIO: Then what *is* the importance of a proof?

ATIYAH: Well, a proof is important as a check on your understanding. I may think I understand, but the proof is the check that I have understood, that's all. It is the last stage in the operation—an ultimate check—but it isn't the primary thing at all.

I remember one theorem that I proved and yet I really couldn't see why it was true. It worried me for years and years. It had to do with the relationship between K-Theory and representations of finite groups. To prove the theorem I had to break up the group into solvable groups and cyclic groups; there were lots and lots of inductions and there were various bits on the way. In order for the proof to work, every single thing had to go just right—you had to be remarkably lucky, so to speak. I was staggered that it all worked and I kept thinking that if any one link of this chain were to snap, if there was some flaw in the argument, the whole thing would collapse. Because I didn't understand it, it might not be true at all. I kept worrying about it, and five or six years later I understood why it had to be true. Then I got an entirely different proof by going from finite groups to compact groups. Using quite different techniques, it was quite clear why it had to be true.

MINIO: Do you see some way of communicating this understanding to someone without the proofs?

ATIYAH: Well, I think ideally as you are trying to communicate mathematics, you ought to be trying to communicate understanding. It is relatively easy to do this in conversation. When I collaborate with people, we exchange ideas at this level of understanding—we understand topics and we cling to our intuition.

If I give talks, I try always to convey the essential ingredients of a topic. When it comes to writing papers or books, however, then it is much more difficult. I don't tend to write books. In papers I try to do as much as I can in writing an account and an introduction which gives the ideas. But you are committed to writing a proof in a paper, so you have to do that.

Most books nowadays tend to be too formal most of the time, they give too much in the way of formal proofs, and not nearly enough in the way of motivation and ideas. Of course it is difficult to do that—to give motivation and ideas.

There are some exceptions. I think the Russians are an exception. I think the Russian tradition in mathematics has been less formalized and structured than the Western tradition, which is under the influence of French mathematics. French mathematics has been dominant and has led to a very formal school. I think it is very unfortunate that most books tend to be written in this overly abstract way and don't try to communicate understanding.

But it is hard to communicate understanding because that is something you get by living with a problem for a long time. You study it, perhaps for years, you get the feel of it and it is in your bones. You can't convey that to anybody else. Having studied the problem for five years you may be able to present it in such a way that it would take somebody else less time to get to that point than it took you, but if they haven't struggled with the problem and seen all the pitfalls, then they haven't really understood it.

MINIO: Where do you get your ideas for what you are doing? Do you just sit down and say, "All right, I'm going to do mathematics for two hours"?

ATIYAH: I think that if you are actively working in mathematical research, then the mathematics is always with you. When you are thinking about problems, they are always there. When I get up in the morning and shave, I'm thinking about mathematics. When I have my breakfast, I am still thinking about my problems. When I am driving my car, I am still thinking about my problems. All in various degrees of concentration.

Sometimes you wonder whether it is worthwhile thinking about it while you are doing these things, whether it really helps. You are just turning it over idly in your mind.

There are occasions when you sit down in the morning and start to concentrate very hard on something. That kind of acute concentration is very difficult for a long period of time and not always very successful. Sometimes you will get past your problem with careful thought. But the really interesting ideas occur at times when you have a flash of inspiration. Those are more haphazard by their nature; they may occur just in casual conversation. You will be talking with somebody and he mentions something and you think, "Good God, yes, that is just what I need . . . it explains what I was thinking about last week." And you put two things together, you fuse them and something comes out of it. Putting two things together, like a jigsaw puzzle, is in some sense random. But you have to have these things constantly turning over in your mind so that you can maximize the opportunities for random interaction. I think Poincaré said something like that. It is a kind of probabilistic effect: ideas spin around in your mind and the fruitful interactions arise out of some random, fortunate mutation. The skill is to maximize this degree of randomness so that you increase the chances of a fruitful interaction.

From my point of view, the more I talk with different types of people, the more I think about different bits of mathematics, the greater the chance that I am going to get a fresh idea from someone else that is going to connect up with something I know.

For example, the Index Theorem was partly a matter of chance. Singer and I happened to be working at Oxford on things related to the Riemann-Roch The-

Michael Atiyah (left) and **Laurent Schwartz** (right) in Japan.

orem, coming out of Hirzebruch's work. We were playing around and we had the idea of looking for a formula with the Dirac operator. And then Smale passed through and we talked to him. He told us that he had read a paper just the other day by Gel'fand, which had been about the general question of the index of operators, and he suggested that it might have some relation to what we were doing. I found this paper very difficult to understand, but it was the general formulation of the problem and we were looking at an important special case. Then we realized that we had to generalize what we were doing and that led to the whole thing. But it was Smale passing through that put us on the right track.

Another example is the work I did on instantons. That was also a bit of a chance. I knew that Roger Penrose and his group were working on geometrical aspects of physics and one of them named Richard Ward was doing some nice things. He was giving a seminar and I asked myself, "Should I go to it or not, would it be a bit of a bore? Well, all right." So I went to the seminar. It was a very clear seminar; I understood what he was doing, and I came away saying, "Gee, that's really good." I went back and spent three days thinking very hard about it, and suddenly it dawned on me what was going on, how it related to algebraic geometry. From then on, the thing took off. I could easily have not gone to that seminar and the subject would still be where it was. The gap between the mathematicians and physicists was very big; I doubt whether the ideas would have been taken up nearly so rapidly. But of course, a lot of time you go to seminars and don't get ideas.

MINIO: Do you have a favourite theorem or problem?

ATIYAH: That's not such a serious question because I don't really believe in theorems, per se. I believe in mathematics as sort of an entity; a theorem is just a staging post. I know lots of nice nuggets, nice facts, nice things, but I don't attach much importance to them individually. I guess the same goes for problems.

I don't want to give the impression that I think of mathematics as simply abstract theory with no body to it. A theory is interesting because it solves lots of special problems and puts them in proper context; it enables you to understand them all. Quite often a theory evolves because somebody solved some very hard problem first and then you try to understand what is going on—you build a superstructure around it. Soft theory which has no hard problems in it is of no use.

MINIO: What is your feeling about the classification of finite simple groups?

ATIYAH: Well, I have slightly mixed feelings about that. First of all, it takes so many pages to prove; it seems to me the degree of understanding must be pretty limited if that is the only way it can be done. One would hope that a much better understanding of all this would emerge. Maybe I am wrong in this, but I believe that if it is going to emerge, it is going to come from people who look at groups from an extroverted rather than introverted way.

As groups arise in nature, they are things that move things around, they are transformations or permutations. In the abstract version, you think of the group as an internal structure with its own multiplication—that's a very introverted point of view. If you only allow yourself to use the introverted point of view, you have a very limited array of techniques. But if you think of the group through its manifestations, from the outside world, then you have the whole of the outside world to help you. And you have got, or ought to have, a much more powerful understanding. My idea, my dream, is that one should prove these deep theorems about groups by using the fact that the group occurs in some natural context as a group of transformations, and then out of this the structure should become transparent.

Also, I am not entirely sure how important this whole result is. Some people will say that in mathematics the most important thing is to set up an axiom system with axioms one, two, three. There are objects—groups, spaces. The problem is then to classify all such objects. I don't think that is the right point of view. The goal is to understand the nature of these things and to use them; classification simply gives you an indication of the scope of the theory.

For example, the classification of Lie groups is a bit peculiar. You have this list of groups, both classical and exceptional. But for most practical purposes, you just use the classical groups. The exceptional Lie groups are just there to show you that the theory is a bit bigger; it is pretty rare that they ever turn up. And the theory of Lie groups would not be very different if the classification had been vastly more complicated, if there had been infinitely many more of these exceptional groups.

So I don't think it makes much difference to mathematics to know that there are different kinds of simple groups or not. It is a nice intellectual endpoint, but I don't think it has any fundamental importance.

MINIO: But if there were another way of doing it, some extroverted point of view, would that have more of an impact?

ATIYAH: That would have an impact in the sense that it would show people that one can do things in some other way. But I don't think the result has fundamental importance; it doesn't compare with, say, the theory of group representations.

The classification point of view can be greatly exaggerated. It is a focus for a while, it points out nice problems and challenges. But if it takes a lot of effort, one suspects that there ought to be better ways to do it. Getting a better way to do it may in itself be interesting and show new ideas and new techniques at work. You see this result, it looks nice, you've got this long complicated proof, it is a challenge to find better ways to prove it. The search may be beneficial, but the benefits come more from the new ideas than from the fact that you've got the new proof.

George Mackey once said to me something that I think is very true. In a given area of mathematics the things that are important are quite often not the most technically difficult parts—the hardest things to prove. They are quite often the more elementary things because those are the parts that have the widest interaction with other fields and other areas—they have the widest impact.

There are many things about group theory which are tremendously important and occur in all sorts of mathematics everywhere. Those tend to be the more elementary things: basic ideas about groups, about homomorphisms, about representations. General features, general techniques—those are the things that are really important.

The same is true in analysis. There are some very fine points in proving exactly under which conditions a Fourier series may converge; these are technically demanding, very interesting. But for the rest of mathematicians who use Fourier analysis they are not really important. The specialists in a field, of course, get enamoured of the hard technical problems. But from the point of view of mathematicians as a whole, while they admire them, they don't use them.

MINIO: Who is your most admired mathematician?

ATIYAH: Well, I think that is rather easy. The person I admire most is Hermann Weyl. I have found that in almost everything I have ever done in mathematics, Hermann Weyl was there first. Most of the areas I have worked in were areas where he worked and did pioneering, very deep work himself—except topology, of course, which came after his time. But he had interests in group theory, representation theory, differential equations, spectral properties of differential equations, differential geometry, theoretical physics; nearly everything I have done is very much in the spirit of the sort of things he worked in. And I entirely agree with his conceptions about mathematics and his view about what are the interesting things in mathematics.

I heard him at the International Congress in Amsterdam. He gave the Fields medals there, to Serre and Kodaira. Then I went to the Institute at Princeton, but he was in Zurich at the time and he died there. I never saw him at Princeton, I only saw him that one time. So it wasn't personal contact that makes me admire him.

For many years whenever I got into a different topic I found out who was behind the scene, and sure enough, it was Hermann Weyl. I feel my centre of gravity is in the same place as his. Hilbert was more algebraical; I don't think he had quite the same geometrical insights. Von Neumann was more analytical and worked more in applied areas. I think Hermann Weyl is clearly the person I identify with most in terms of mathematical philosophy and mathematical interests.