Faces of Mathematics: Tony Scholl

Marc Atkins and Nick Gilbert met with Professor Tony Scholl at the University of Durham on the 29th May 2000.

NDG: *One of the things we are trying to do in this project is to give impressions of mathematical research to the general public. I wondered when you first became aware of mathematical research, mathematics in the professional sense and began to perceive that it might be a possibility for you.*

AJS: Well that's rather interesting. I think really very early on, though I wasn't sure until quite late in life I wanted to be a mathematician, it certainly wasn't until I was at university that I thought about that. I was nine when I discovered I had quite an aptitude for maths, reading books about the history of maths.

NDG: *So something quite special happened at age nine?*

AJS: No, not really. I had quite an inspiring teacher who gave me extra reading, a little book on calculus...

NDG: *At age nine?*

AJS: It was very elementary, explaining how you do differentiation, which seemed easy, and integration, which seemed harder... which in terms of mathematical sophistication seems the wrong way round! Then I became interested in physics as well. I had a physics teacher who pushed me towards physics, so I actually ended up going to Oxford to do physics. I didn't have a great love for experiments and the theory didn't really appeal to me in a great way. The things that I really liked were the structures, the mathematical aspects, so I became a mathematician with a vengeance. I think my tutors thought I would end up becoming an applied mathematician, so when I started studying algebra they were surprised.

NDG: *Did you find your undergraduate experience of mathematics at Oxford to be what you expected, or what you were looking for intellectually? It must have been stimulating in that you decided that you wanted to carry on and do postgraduate study.*

AJS: I think so. It was quite an unusual experience: I was quite young when I went to Oxford, and I enjoyed my time to the full. I did lots of things apart from mathematics.

NDG: *So you were younger than the standard undergraduate?*

AJS: I was the youngest person in my year: I was seventeen. If I had been an undergraduate with the intention to become a professional mathematician, I think I would have done things differently. But it was all rather organic, and it was really only in the third year as an undergraduate that I decided I wanted to do research, and chose number theory because it seemed interesting, not because I knew much about it.

NDG: *So you were able to make a definite choice of that area of research?*

AJS: I felt my way in. I did a choice of courses in my third year that gave me various options, and I did toy with the idea of group theory for a while, though I didn't find I had an aptitude for the technical stuff that finite group theory was all about at the time. So then I decided to go and work with Bryan Birch: well, not to go but to stay. And again I wasn't really sure whether I was going to continue with that, or whether I was going to get my foot in the door of research and then go and do some thing different. It was all rather undecided, but it gelled in a particular way.

NDG: *The area that you work in - arithmetic algebraic geometry - can seem very forbidding even to a mathematical outsider. Do you have a way in which you could describe its concerns and aims that might be accessible to a non-specialist?*

AJS: You mean a non-mathematician...

NDG: *Even a non-mathematician, yes.*

AJS: When I've tried to talk to people, people in the street, about arithmetic algebraic geometry, there are some things you can describe. You can talk about Pythagoras' Theorem, and explain that this gives rise to properties of numbers - Pythagorean triples - and that there are lots of interesting problems that arise from that. Although I'm not motivated by classical problems so much as by structure, and that's what really fascinated me about arithmetic algebraic geometry. When I first started studying the theory, it seemed - well, a sort of Pandora's box, all kinds of different things, some completely unrelated and all rather unstructured. I remember looking at a book on Diophantine equations and thinking that was really rather strange: lots and lots of seemingly unrelated problems. When I was a first-year graduate student, I started to learn some algebraic geometry and I took an instant liking to the structural aspects of it. Even algebraic geometry per se, as it is in the second half of the twentieth century, is a very beautiful mix of algebra and geometry. That appealed to me because I feel happiest when I'm doing algebra, but I like to think geometrically.

NDG: *Do you see your approach to your own research more motivated by problem solving, or by theory building? Or are you equally concerned with those issues?*

AJS: I'm not really a theory builder: I think you have to be brought up in the French School... I think theory arises as a by-product of trying to solve problems. But just having a problem and solving it is not something that interests me in itself: I like solutions that have some surprise, and that is one of the really nice things about arithmetic algebraic geometry. You have some fairly concrete problems - the best problems are ones you can actually explain to the man on the street - you can't perhaps explain the Riemann Hypothesis, but you can say that it is something related to the distribution of primes, and of course you can explain the Goldbach conjecture. And there are classical problems about elliptic curves that you can explain to a reasonably numerate person in the street: of course, you can explain Fermat's Last Theorem. The beauty of these problems is that to get to grips with them, you have to put in an enormous amount of technical machinery and abstract structure, and that somehow justifies producing this structure.

I remember that one of the first things I did after my thesis, I did some work on modular forms for non-congruence subgroups of the modular group. There was a collection of conjectures which Atkin and Swinnerton-Dyer had produced, essentially through computer experiment, about the arithmetic nature of these modular forms. I spent some time working on these and eventually found a solution: I'm not sure it was the solution, because not all the connections were perfectly formed, but certainly an explanation for all the phenomena that they had observed. I wrote to

Swinnerton-Dyer explaining that I had done this, and I think he was probably rather horrified, because the apparatus that you needed to do this was incredibly sophisticated. It used all the mechanism of étale cohomology and crystalline cohomology as well. It was fun to do - I had to learn a lot of mathematics to solve it, but at the same time I felt that you could see how that very abstract cohomological structure can actually be use to solve relatively concrete problems.

NDG: *The subject seems to have been driven by key conjectures. Is there one problem, or class of problems, that has focussed your research interests?*

AJS: Well for some years, yes, but not entirely. There is an awful lot of the subject that is driven by the Birch-Swinnerton-Dyer conjectures and related things. I think that to a great extent the subject is driven by the relation between arithmetic and L-functions, and for quite a long time I have been doing things related to Beilinson's conjectures which are generalisations of the analytic class number formula to higher dimensional varieties. But in a sense even working on these things other structures emerge.

Just recently I have been thinking about - in the time I have to think about mathematics, when I am not running the department - I have been thinking about representations of p–adic groups because of a problem which arose in connection with Beilinson's conjectures. It was rather cute the way it arose because I made a mistake in writing the draft of a paper and I tried to understand why the mistake was a mistake and there was some phenomenon that looked interesting and might be worth trying to explain. I had an idea a few years ago which I worked out in a paper with Michael Harris, trying to make some evidence for Beilinson's conjectures more consistent with what the conjectures should be, and that involved studying some properties of representations of p–adic groups because the evidence was in connection with modular forms. In the course of writing this paper I noticed that there was a phenomenon which seemed not yet to have been observed by people working in the area. I've been thinking about that for a few months and maybe over the summer... I've got a particular conjecture which I want to work out. It's not especially difficult, but it is quite interesting the way these things emerge.

I don't like to be too concentrated on one particular problem or one particular area, and that's another thing about the subjects that is particularly nice: you have to know a lot of mathematics - algebra, topology, analysis, geometry. When I was a first-year student there was a conference in Oxford on Lie groups and somebody came and sat next to me in the coffee room, who had come along to go to this conference, and he asked me what I was doing, so I said "Number theory". "Gosh, number theory" he said, "that must be such a narrow subject." I'm still amazed that anyone could have such a perception of number theory!

NDG: *When we spoke earlier, you said you were very excited about a piece of research that you were involved in. Was this the representation theory stuff that had really captured your imagination?*

AJS: Yeah - whatever I do, I get really gripped by it. You wouldn't be a mathematician if you weren't. It is a thing about representations and e-factors, which occur in the functional equations for L-functions. It is quite technical of course, but it is quite pretty as well. I don't think the solution is going to be very exciting mathematically - which is why it suits me at the moment that I don't have much time to do anything particularly exciting - I have to balance things, unfortunately, and I don't have these long uninterrupted periods which are really necessary to do much really creative.

NDG: *So, as you said, being gripped by problems and being excited by problems is the very condition of success in being a mathematician. You also need some method for dealing with frustration or failure, and with making mistakes: do you have a particular approach to this?*

AJS: My approach to making mistakes is simple: I just make them. I make them quite unashamedly. I'm quite happy to make mistakes, because that is how you learn. Well, for me, that is how I learn. I'm very much a believer in the mathematical process being one of successive approximation. When you are teaching, that is something that students are terribly afraid of. They are brought up to feel that they mustn't make mistakes, and that the most important thing is to get something right. The accuracy of what you come up with at the end is secondary to the process of understanding, as far as I am concerned. If I work on a problem and get it completely wrong, but I learn something in the process, then that's great.

NDG: *I would certainly associate much of your work with Beilinson's conjectures. Could you explain what those are really about?*

AJS: Well, perhaps the easiest way to explain is via one of the classical examples which are the motivation, and that is the analytic class number formula, which was discovered at the end of the nineteenth century. It concerns the ζ–function of a number field. The ζ–function – defined in the same way as the classical Riemann ζ–function but using the integers of the number field instead of the natural numbers - has an analytic structure that is fairly well understood. There is a simple pole at $s = 1$ and the analytic class number formula is a formula for the residue at that pole. The ingredients that go into this formula are the class number of the number field - which is why it is called the analytic class number formula - the number of roots of unity that the number field contains, and the regulator, which is sometimes the most interesting part, because that's the transcendental part of the number, which is the determinant of a matrix of logarithms of units. Beilinson's conjectures are really a wild generalisation of that, in which the ζ–function of a number field is replaced by an L–function arising from an algebraic variety over the rationals - so a number field is the simplest example, it is just a 0-dimensional algebraic variety - and the numbers on the other side are replaced by things to do with the certain cohomology groups associated to the variety. The simplest, though not perhaps the right way to describe them, is to say they're invariants associated to the higher K-groups of the variety. Those are the numbers that arise in the classical formula, because the class group is just the torsion subgroup of K_0 of the ring of integers of the number field, and the roots of unity occur in K_1, and the units of course are K_1, so the regulator and the roots of unity measure in some sense the size of K_1 of the ring of integers of the number field. So there is a rather fantastic generalisation of this, which include that and other conjectures as special cases. Beilinson's original conjectures described the transcendental part: so they describe the value of these L-functions up to a rational factor. The determination of the rational factor requires more subtle ideas, and that's essentially the substance of the Bloch-Artin conjectures. But a first approximation would be to try to get the thing correct up to a rational factor.

I got interested in this because we had a working conference at Oberwolfach in 1985 to study Beilinson's paper on his conjecture, which came out in the early 80's. That was very interesting, because the mathematics that goes into just formulating the conjectures is fairly formidable. This was one of those conferences where if you go along, you have to volunteer to give some talks. So I and Norbert Schappacher gave several talks on one particular part of the paper which was on modular curves. While we were working on that, I had some ideas on how this might generalise to other L-functions of other modular forms. The time was just right, since I'd been working on what I mentioned before, on non-congruence subgroups, and I'd been looking at some problems to do with transcendence of coefficients of modular forms on non-congruence subgroups. And it seemed that there was some subtle algebro-geometric invariant that worked, and it was in the the process of trying to understand that, and after talking to various people who understood algebraic K-theory rather well - which I didn't at the time - I realised that there must be something there, that would measure the deviation between congruence and non-congruence subgroups. So it was rather a surprise, as I was thinking about this, that I came across this work of Beilinson, and it turned out that he had thought about this for a completely different reason, and had just come up with the answer. So I was able to use that in looking at the higher-dimensional analogue of L-functions.

NDG: *What is the status of these conjectures at the moment?*

AJS: Not much is known, really. A new idea is needed. A new idea is needed because at the moment, the only way we know to relate L-functions and arithmetic is through modular forms of one sort or another. There are classical modular forms on Dirichlet L-series and the Riemann ζ–function and such things, but they are modular forms of a very simple kind. Without such a connection we are completely lost. For example, all the deep results about the arithmetic of elliptic curves rely on the Shimura-Taniyama-Weil conjecture. Until that was known, all the results were for elliptic curves that had the property that they could be parametrised by modular functions. That's sad, because what we now know from studying Shimura varieties and representation theory, is that you can't hope to get hold of other varieties - even relatively simple ones like elliptic curves over imaginary quadratic fields. There's no way of getting a handle on their arithmetic by studying the geometry of varieties associated to modular forms. They work very well for elliptic curves over $\mathbb{Q}$, modular forms over $GL(2, \mathbb{Q})$, but beyond that the arithmetic information that you are going to get about the objects that you want to associate to these particular forms, becomes scarcer and scarcer. So I think that what people have dreamt about is to have some wonderful cohomology theory for arithmetic varieties that would somehow be a substitute for the l-adic tale cohomology theory that proved so useful for studying varieties over finite fields. Some people have thought about that, and have thought about trying to actually produce such a cohomology theory, but not with any degree of success. There are no candidates, and no ideas of where a candidate might come from.

NDG: *So how does motivic cohomology fit into this?*

AJS: Well that's very much on the geometric side. What you need is somehow - how can I put it? Look at classical algebraic number theory. A lot of the classical results - finiteness of the class group, finite generation of the group of units - come about because you can embed the units of a number field, modulo the roots of unity, as a lattice in some Euclidean space and you can use some geometry of numbers to get a grip on it. Now that is what is really lacking when you are looking at varieties of higher dimension. Arakelov theory is the way in which this has developed, but still we don't have an analogue for the logarithmic embedding of the group of units, not an analogue that we can actually do anything with. We don't know that any of the K-groups of these algebraic varieties are finitely generated, except in some very special cases. Some of the ones that we can produce finiteness results for, the methods of proof use p-adic analysis, quite subtle things. In our dream world, we'd have an arithmetic cohomology theory that associated to an algebraic variety certain infinite-dimensional spaces with extra structure, which you could use to construct L-functions to prove that they had nice analytic properties. If you had such a machinery, things like the Riemann Hypothesis would come out as properties of these operators. But at the moment that is still a dream, not a dream that I've been working on in any serious way myself, but there are people who have been. It is well known that Connes has been thinking about the relationship between the Riemann Hypothesis and foliation theory. I'm not an optimist: I think this is a dream of the way things might be, rather than a dream of how they are, and it such a long way from present knowledge. I think that what is really needed is a few more startling ideas.

NDG: *Are you an optimist about the conjectures themselves?*

AJS: I am an optimist, in that the conjectures have a lot of internal logic, and it is hard to imagine they could be otherwise. Other conjectures I'm not sure I'm so optimistic about. I see no reason to be particularly optimistic about Goldbach's conjecture. It wouldn't disturb me greatly if it turned out that there were a few gaps. But the evidence, the internal logic, of the Riemann Hypothesis is a different matter. It has analogues over finite fields which are known to be true, and I think that I'm an optimist in that respect. I'm not so optimistic about a proof being found in my lifetime. But I don't really care that much!

NDG: *When you're working in this area, do you tend you work with the background assumption that the conjectures are true, or does that not impinge on the technical details?*

AJS: Yes, because... in this case I've never looked at the case of what would happen if they were false... It's not the kind of thing... Life would be messy, that's probably the best way of putting it!

NDG: *Is Beilinson himself still active?*

AJS: Yes, he does lots of things. At the moment he's thinking more about quantum groups. I saw him just over a year and a half ago at a conference in Germany, and he gave me a copy of a preprint he had about motives, so he is thinking about these things as well.

NDG: *If we could turn from the technicalities of your research to the practicalities. Do you tend to or prefer to work on your own, or with collaborators?*

AJS: I'm not a great collaborator. I've never really worked on a single problem jointly with someone else. It would be quite interesting to do so, because I don't know... the dynamics of such a working environment must be very interesting. It just suits me to work by myself.

NDG: *Do you have a preferred place in which to work?*

AJS: Not really, no. Some people say that they like to work at home. I like to work in places where there are no distractions, which usually means not in the office, but can mean not at home as well! Somewhere different actually, where there are no distractions at all... in a hotel room.

NDG: *What about places like Oberwolfach that you mentioned earlier?*

AJS: The trouble with Oberwolfach is that there is so much going on. I do often work there, but if I do, I find I don't get very much sleep. Early starts and working until nine o'clock at night

- and I do like working into the late hours - that' s certainly true, I do have a preferred time of working, and that tends to be late in the day. Sometimes I've worked all through the night, and that can be quite pleasant, provided you haven't got any terrible commitments the next day. I find it very difficult to work and to know that I've got to stop. Snatching half an hour at a time is just not... unless there's a particular thing which I want to do and I think I can do in that space of time... I find it very difficult to think when there's a horizon.

NDG: *When you are engaged by and thinking about research, do you think that you are a different Tony Scholl from the person who goes about and does the rest of life?*

AJS: Oh, definitely. Certainly I'm a different person to speak with, from what I've been told! But I feel it too: when I stop working and do other things, like domestic things or to go for an evening out, I feel that only half of me has come back to the real world.

NDG: *Does that kind of state last for hours, or days, or weeks?*

AJS: Well, days: a day probably. At the moment I can only manage that if I go on sabbatical.

NDG: *So as Head of Department, we may be talking minutes...*

AJS: Unfortunately, yes. Trains are a good place to work: an uninterrupted three hours down to London, except that I can never read my notes. And there's a problem with the paper, because I like to spread the paper out everywhere, so it does spread to the floor of the railway carriage...

NDG: *Your fellow passengers are holding back the progress of mathematical research...*

AJS: Oh, I had a great time with Bryan Birch. We were on a train, I think we'd gone down to London for some reason, and we were talking about mathematics. Bryan was saying something about this cow's stomach, and you have to cut it here, and cut it there, and glue these pieces together... and there was a person sitting opposite who was completely perplexed, and I think rather worried, and didn't realise that these mathematical metaphors were that, and thought we must be completely mad.

NDG: *Do you take any philosophical stance on what you are doing? People might regard mathematics as a formal game, or as the discovery of absolute truths.*

AJS: Am I a Platonist? I suppose I am, though perhaps not in the strictest sense. I'm not really a formalist. I'm not sure that the question is all that meaningful, because once the mathematics has been discovered, it is there and you can't uninvent it, you can't say it wasn't there before. I'm not sure that is a very meaningful question. Is there a universal mathematical truth? I think most people think there is. It must be very hard to do mathematics unless you think that the rules are fixed. Although one knows there are set-theoretic problems, I'm not one of those people who worries about these things. I think I take a fairly cavalier attitude to foundations.

NDG: *Do you have any analogy for the processes of mathematical research? Do you find analogies in art, or music, or chess playing, or anything of that kind? To give you an example that Tim Gowers offered us, there was a toy that his son had been given, a manipulation puzzle with cars and lorries and so on. Tim said he had played with this for ten minutes, and he realised it was really like mathematics: he wanted to get a truck from this corner to that corner, and to do that you had to move the car first, and to do that you had to* *move the people. Tim had played this for ten minutes and thought that it was a bit like doing mathematics, in that if you wanted to explain to someone what the process of doing mathematical research was like, you could give them this puzzle, and say "Try and do this: get the truck from this corner to that corner".*

AJS: I prefer what Andrew Wiles said at the beginning of that Horizon documentary. It was exactly how I felt about doing mathematical research: it is like walking into a darkened room, and there's just furniture in it, and you don't know what's there, and you just feel to get the shapes. It is much more like that, as I see it. I like doing recreational puzzles, but not in a very serious way as some mathematicians do. I don't see that for me, doing mathematical research is not the same process as solving puzzles, because I'm always thinking about structure.

NDG: *Geometric structure?*

AJS: Geometric structure, but possibly algebraic structure. I think if I had to pigeonhole myself, I'd settle for algebraist. I was thinking of this very question a few days ago, and I had a very good analogy that I'd thought of, but has now escaped my mind.

NDG: *Do you see any connection between the music that you like to play and the way you approach mathematics? Do you like particularly structured music, or music where there is a sense of discovery?*

AJS: One of the things that makes a good piece of music for me is that you can listen to it many times, and that each time you hear it there is something new to discover. You unwrap a particular level of complexity, a particular formal level. My musical taste is perhaps not what you would expect from a mathematician, for traditionally mathematicians are supposed to favour Bach, and are not supposed to like the romantic nineteenth century stuff. My tastes are pretty wide, and I enjoy nineteenth century music greatly. I do like to have some structure.

NDG: *How about twentieth century music?*

AJS: The problem with twentieth century music is that you have to spend a lot of time listening to it before you get anything out of it at all. I'm intrigued by twentieth century music: intrigued from a purely academic point of view, the way in which twentieth century developed. I'm talking about the first half of the twentieth century, because the second half is rather chaotic, and music seems to have fragmented considerably.

NDG: *Have you ever found appealing the sort of music that is supposed to have some direct connection with mathematics, say Xennakis or Lutoslawski?*

AJS: As far as order... I'm not sure that there is anything intrinsically mathematical about the serial approach to music: it is a structure within which one can compose. That's my interest in listening to such a piece of music: I'm aware that there is a structure within which it is written. I don't think there is much intrinsic beauty in that structure. Now Xennakis is rather different. I never really understood the mathematical complexities that he is using in his so-called stochastic music. I don't know if it possible to write stochastic music. It is almost a contradiction in terms.

Once I was at Oberwolfach, and I saw a book there, which I have never been able to find since. It was a proceedings of some conference on mathematics and music, and the people who were writing there were trying to use sophisticated mathematics as a basis for writing music - group theory - they were using algebraic structures to formalise their musical language. I saw this on a bookshelf, and thought that I must have a look at this when I have some time, and I didn't make a note of what it was, and I've never found it since.

NDG: *Thinking again of the mathematics that you do, is there something that you would like to achieve in the nest five, ten, or however many years, that you would see as a long term aim or ambition?*

AJS: You mean particular projects? I don't have any projects that I'm thinking of as a problem I'd like to solve in the next five or ten years. I don't think I've ever thought that way - well, not for some time. I prefer much more thinking about a general area in which I work, and the problems almost arise simultaneously with the solutions. I have a couple of other projects which I have been thinking about, and as I work on them more they'll probably develop into something very different. I don't have any specific ambitions, except to stop being Head of Department before I retire.

NDG: *And then they'll be time to think about projects...*

AJS: Really, it is like trying to drive a car with just one cylinder working...

NDG: *Which other mathematicians, in your area or more widely, have you particularly admired? Obviously Bryan Birch, as your graduate supervisor, would have been a big influence. But are there others whose ideas and style have been particularly influential?*

AJS: I don't know so much about style and ideas, but there are certainly other mathematicians whose work I've admired and studied. I'd really have to say Deligne is one such person: his work is amazing, and his writing has such insight. In just a paragraph, he can in such a succinct way... it is a combination of being precise and detailed, and at the same time just showing you the insight. It is possible to write in a very formalistic way, in such a way that everything is completely clear, and to understand it all in the sense of understanding how you get from one line to the next, but at the same time being very dry. His writing is never dry: it is very iridescent, and full of ideas even when he isn't solving problems. It is very inspirational to read.
