1. David Vogan, Massachusetts Institute of Technology

Could you start by giving a biographical sketch?

I was born in 1954 in a small town in Pennsylvania, and lived in that state until I went to the University of Chicago. My time at the University of Chicago was mathematically important because it was there that I met Paul Sally, who ended up directing my mathematical career. He did representation theory... he is the reason that I do that too!

After getting my undergraduate degree I did what he told me to, which was to go to MIT for graduate school. Lots of people at that time thought that MIT was the place to go for representation theory.

I guess it was in 1974 that I went to MIT to work with Kostant. Graduate school is never exactly the way you expected it was going to be. Things went as expected in the sense that I did in fact work with Kostant. However, after less than two years Kostant asked me if I wanted a job. I had been expecting to spend another couple of years in graduate school. I was very happy with this possibility and I had to work a lot faster and harder than I thought I was going to have to work but somehow managed to finish. After that I became an instructor at MIT, only, as it turned out, for one year. Then I spent a couple of years at the Institute for Advanced Study in Princeton.

I learned a lot at Princeton. Especially from Greg Zuckerman, who was visiting there and from Armand Borel. I also learned a lot from the many, many visitors to the institute—it is probably not safe to try to say exactly how many and who. That was a wonderful period of time. It's frightening to be in a place where you have nothing to do except mathematics. There are no excuses. Anywhere else in the world, the reason that you don't do mathematics is because you have so many other responsibilities—teaching, and this and that. At the institute, if you don't do mathematics, there is no reason for living *[laughs]*.

After two years at the Institute I became a member of the faculty at MIT, where I have been ever since.

When did you first have an inkling that you wanted to become a professional mathematician?

It was certainly clear by the time I was in college, but I had inklings before that. I wanted to do physics from a very early age, probably six or seven. I was never sure exactly what kind of physics I wanted to do—I think I had illusions of becoming an astronomer. This became more and more serious as I got older. Certainly when I was beginning college that is what I wanted to do.

In order to do astronomy I knew that you had to do physics. To do physics in college there are Laboratory classes. Both Laboratory courses and physics in general turned out to be much too hard for me. I did some of the first courses but it was clear that this was not something that I could be good at. It was something interesting to try, and I certainly continued to be interested in physics. I always thought of it as an excellent motivation or as something that was better than what I was actually doing…

I think a lot of mathematicians share this feeling!

…that's right. But during my first year in college it became clear that it was mathematics, rather than physics, that I was going to be able to do. Probably this goes back to a teacher I had in high school. He had spent most of his life running a men's clothing store in the little town where I lived. When he was extraordinarily old (in other words probably ten years younger than I am now *[laughs]*) he went back to college and studied mathematics. Some fairly short time after that he was my teacher in high school and he let me look at some of the course materials that he had studied in teacher's college. It wasn't terifically exciting mathematics but much more interesting than the usual run of high school mathematics. There was certainly no calculus taught in the high school where I was…

Really?!

…this was the good old days!! Maybe a third of the students went to college; almost certainly to Penn State ten miles away. It was from him that I found out (probably in tenth or eleventh grade) that there were interesting kinds of mathematics out there. At this stage I still wanted to do physics but mathematics began to look entertaining.

What kind of things did he show you? Calculus?

No, no. Elementary things. Pascal's triangle. Complex numbers. Things like that. Nowadays I suppose most people in good high schools do learn those things. Certainly they weren't in the syllabus at my high school. This was interesting stuff!

I am really interested in your attitude to research. Can you explain why you believe research is important for society?

That's…interesting. Of course there are a lot of kinds of research that make people's lives better in short order. A lot of medical research, for example, can be justified in this way. Lots of other kinds of research lead in five, ten or even a hundred years to that kind of directly applicable research, or they make it possible to keep the lights turned on whilst doing that kind of research, or they make it possible to eat! For that kind of research you can convince anybody in that it is worth doing.

In general some form of research is how people manage to get along. For example, Britain is uninhabitable. If you spent a year here without some kind of clever ideas you would die. But people have been managing here for a very long time by thinking of clever ways of having a roof over their head and staying warm. That is true of almost anywhere in the world. There are very few places in the world where life is possible without some kind of organisation. But even in the easy places you have to spend all your time working hard in order to survive, and ninety percent of your kids die anyway! Getting away from that requires thinking about how you live and how you organise your food, shelter etc. and clever ways of doing that. That's just how poeple survive and make their lives better.

I think almost everything that needs to get done needs to get done in a cleverer way. There are very few people that I know that do actually make food, or build places to live. Almost everybody I know is in the buisness of organising these processes in some way or another. That's what research is about.

Research means means thinking about what you're doing. Certainly most of the mathematics we teach is teaching people to think carefully. Nobody needs l'Hôpital's rule to manage in their daily life! But it does seem that thinking about mathematics is a good way to be cleverer at almost everything.

It's probably evolutionarily related that research is just so much fun!

I have heard this before. People often tell me that they just really enjoy doing it!

Almost any job can be an enormous amount of fun. I don't have a huge amount of experience outside of mathematics, but I spent a summer working in a lumber yard unloading box cars. Certainly that kind of work could be very pleasant if you made it that way or if you had the attitude that it was going to be fun. You just stacked up the wood and if you did it well the piles looked good when you were done. You had fun with the people that you were working with. Certainly there is a whole lot of work that is horribly difficult and in some sense unpleasant. But there doesn't seem to be very much that people can't enjoy. This is the case even though it doesn't look like fun and we even claim that it isn't fun. But you can make it fun. Making it fun matters a lot.

Do you consciously decide what to work on?

No. Certainly not. Except for when I tell other peole what to do, especially graduate students. I think very carefully about what I tell somebody else to do. But hardly ever for myself!

Do you consciously attack questions or conjectures?

There are various problems that are in the back of my mind all the time. Mainly because I think of them as important or I know lots of reasons why it would be great to understand them better. But it hardly ever works to decide that today you are going to do this or that. This only works when a problem is essentially already done and you have to write something down. Then you can think "today I have to find some reasonable way to explain that this lemma is true". On any more serious level than that it's a question of having some ideas of what kind of thing is worth doing and what kind of thing might work.

Talking to different mathematicians it seems that there is a different emphasis on examples. What is your position on the divide between examples and general theory?

I think that examples are the only things that matter. There is a story about Piatetski-Shapiro who gave a colloquium somewhere. His title was "Automorphic forms on GL_2". Ten years later he returned to give a colloquium at the same place. The title was "Automorphic forms on G_2". When he came the second time he wrote the title on the board and explained the title of his talk ten years ago. He said "the total progress of the last ten years is that we can erase the L. I hope maybe in the next ten years we can erase the 2."

That's beautiful!

. . . yeah right *[laughs]*. Those people in automorphic forms do one example at a time. If you understand the example properly (or at least well enough) then you will know eventually how to do much, much more. All because you understood that example.

Another nasty thing that automorphic forms people say is that Harish-Chandra showed that if you undersood SL_2 properly, then you could understand everything. Then Langlands came along and showed that if you understood GL_1 properly you would understand everything!

For many kinds of mathematics there is some basic example that, if you understand properly, contains the whole subject. I like that way of doing things a lot.

How many basic examples do you have in your repertoire?

Fewer and fewer. I mean, it changes all the time. Various versions of SL_2 are certainly enough! I think making the list shorter and shorter is important.

What percentage of your time do you spend writing?

Most of it! It takes way more time than anything else. One hundred percent of the time in some sense. The only time I'm making any measurable progress is when I'm writing something. It's certainly very slow and painful. It's also true that by the time you're ready to write anything at all you're done. So in some sense I could also say zero percent of the time! Writing certainly never gets easier.

You spend a lot of time getting nowhere and doing absolutely nothing. Then suddenly, every now and then there's a theorem. But I don't know why or how.

Do you have "eureka" moments?

Here is one of the three or four jokes that I repeat too often:

A professor is teaching. He writes something down and says "this is completely clear". One of the students says "sorry, I don't see why this is obvious". The professor walks up and down, starts pulling at his hair and eventually leaves the room. Next class he comes in and it is clear that he hasn't slept in the intervening two days. He starts the lesson by writing down a complicated proof on the board and says "so I was right. I told you that it is obvious."

Everything is so easy and obvious. I often ask myself "how could I never have known that?". There is some enormous amount of material that is so obvious and trivial that it is not worth calling it an "aha" moment. Except on the other hand there was some huge amount of time when I didn't know most of it.

Have you spent much time working on the the way you work? For example, do you have a means of deciding whether a certain calculation will lead somewhere or not?

One thing that I think is rather important is writing things down comprehensibly. For many kinds of mathematics it is very valuable to do some horrible calculation. One rule I have is that I need to write some precise and complete account of that calculation. Even though five pages of scribbling might be enough to convince me now, it is entirely guaranteed that in five years I won't be able to make any sense of those five pages! I will even have no idea of where to begin the calculation. It will be completely lost to me unless there is some sort of reasonable account written down.

I have a whole drawer full of all kinds of calculations—more or less of Kazhdan-Lusztig type data for various small groups. A lot of them are quite neatly written down and continue to be useful to me. I also have various other files of chicken-scratchy stuff which I keep because it seemed at the time that I could figure this out. However I really just ought to throw it away. No matter how hard it was and how useful it may have been, now it is not of any value at all because I can't read it!

There are arguments as to how important it is to publish papers, and what should be published. However I don't think there is any argument about how important it is to write things down comprehensively.

Do you have advice for young mathematicians?

I think that the more completely and precisely you write things down the better. The longer your papers are the better. The more complete the references are the better. Writing things down comprehensibly and well is the most important thing. There is lots and lots of mathematics that is more or less gone because it wasn't written down well enough for anybody else to understand. With Harish-Chandra or Lusztig the mathematics is incredibly difficult, however if you start at the beginning of one of the papers you can read every line and they explain how it follows from the lines before. At the end you might or might not know what the main ideas were but you have a chance of knowing whether the results they claim are true or what the results are! That is certainly not true of all mathematicians.

It is entertaining to do some calculations, and you have entertained yourself if you have done something that only you can understand. But it seems like a horrible waste (if it was really worthwhile) not to put it in some form that other people can understand. I care much more about how things were written than whether they were interesting or important in the first place. Whatever you figured out about them was in some way interesting and important to you, and it's not worth discussing that issue now. That work is done, what really matters is how much you can convey about that interest to somebody else. That's worth doing well.

What do you think about the structure of the mathematical community at the moment, with respect to publishing, journals, etc.?

I don't understand it very well, and I love it! The fact that you can search all these thousands and thousands of papers instantly for phrases and symbols is fantastic! I only have the tiniest grasp of how to make use of that. But it is wonderful and it changes everything.

I have a huge respect for the refereeing system. It often fails miserably but I don't think there is anything in the world comparable to the refereed mathematical literature as a source of reliable information. I would hate for that to disappear. The arxiv is fantastic in the breadth and speed of dissemination ... but if you look at the tables in Bourbaki every single digit is either right or you can go on the arxiv and find the erratum! That is fantastically useful to have all that stuff there.

I would love it if this reliability could be put together with this accesibility and instantaneousness. I have no idea how that might work.

I think it is converging. People do often post the latest or published versions of their papers.

In general things on the arxiv are good preprints. But something in the *Annals of Mathematics* has a certain amount of reliability. This reliability matters. It might happen that you do some calculation with

F_4 and you get an 11, and somebody else's table has a 13. If the other table is one of Roger Carter's tables, then you figure that your 11 is wrong and you have to do something about it. If the table is something from the arxiv it is not clear what to do.

What about the pressure to publish?

I don't know. I'm terrible about understanding how properly to encourage young people, how to pick the best ones to get the best jobs, and what to do with the others. All of these things are really important, and I'm lousy at them. I don't know.

It is forcing research to be much bigger than it used to be and there is room to be concerned. There is a famous characterisation of a publication "filling a much needed gap in the literature". Obviously there is some of that. However I don't think there's a lot of it. I think the gradual occupation of what used to be second rate universities with research mathematicians is happening in a reasonable way. These people are doing mathematics that wouldn't have been done twenty years ago but is certainly worth doing.

I don't know enough to judge whether it is a good thing. I mean probably it is. I am pleased if the people that used to be only concerned with teaching trigonometry at second-rate universities are now also concerned about blocks for finite groups or who knows what. They are doing something interesting and I think that's good.

Even at places like MIT I don't think that the pressure to publish things is a bad thing. It seems in general the best ones manage it, they manage to do good stuff (and in large quantaties) and everybody wins!

A last question from left field: what is your view on mathematical reality?

I don't even remember if its Platonic or not. I believe in the things that I work with.

You believe in a unitary representation!

Sure! Maybe not quite as much as some people. It may just be naiveté. I think that the most basic objects of number theory (prime numbers etc.) exist. But there is another level—the whole theory of automorphic forms for example. It isn't clear to me that we have got that right. It is clear that there are interesting questions there and that there is some reality that we want to understand but it isn't quite clear to me that the present language is the right one...

. . . but you assume that there is something there, that there is a "right" language. Possibly we are looking at some objects from the wrong angle?

One of the things that I like is Langland's classification of representations of real groups via maps from the Weil group into the L-group.

Martin Andler remarked that this can't be exactly right because there is no place to do perverse sheaves in his description. He was absolutely right and Jeff Adams and Barbasch and I rearranged his definition. Of course you can't do that: he had this list and it gave the right answers. So you had to change it in some way so that it didn't change, but so that it became an algebraic variety that you could do geometry on. It has to be the same set, but this rigid thing that you can't do algebraic geometry on is replaced by a variety.

That's a very small thing. I think there can be bigger things where we have really fundamentally missed how to see something. An example of this is what Grothendieck did with schemes. There was a huge theory of algebraic varieties. He showed us that we are not thinking about them quite right. Yeah these are all the right examples and have all the right properties, but nevertheless you haven't defined them properly. I think there is scope for more of that!

We still have to look at the same varieties that the Italians said to look at over a hundred years ago. But it is still OK to change the definition of what an algebraic variety is in a fundamental way! We could use another Grothendieck!

Thank you very much.

That was fun!

Interview by Geordie Williamson at the Isaac Newton Institute, Cambridge in January, 2009.