Mathematics, Music, Masters: Conversation with Roe Goodman

Excerpts of an interview by Y.K. LEONG
Text of full interview available at
http://www.ims.nus.edu.sg/imprints/interview_goodman.htm

The Editor of *Imprints* interviewed Roe Goodman of Rutgers University on 11 February 2003 at the Department of Mathematics, National University of Singapore while he was visiting IMS and the Department of Mathematics. He was a guest participant in the IMS program on "Representation Theory of Lie Groups". The hour-long interview covered topics that range from teaching and research in mathematics to the influence of masters in mathematics and music.

Goodman's extensive research activities are centered around Lie groups. Together with Nolan Wallach, he has written a 685-page encyclopedic book *Representations and Invariants of the Classical Groups* that is both an introduction to as well as an authoritative reference on the structure and finite-dimensional representation theory of the complex classical groups.

A mathematical bassoon Courtesy Roe Goodman

Imprints: *Can you share with us some of the excitements of your latest research?*

***Roe Goodman**:* My own research started in the 1960s when I did my PhD thesis with Irving Segal in MIT. Segal himself was primarily interested in the mathematical problems of quantum field theory, viewed in a very broad sense: difficult questions in non-linear partial differential equations and their symmetry groups. My own interests and activities over the years have moved in the direction of representation theory and the symmetry groups although I maintain an interest in application to physics. The thing that attracted me to representation theory is that it lies at the crossroads of all of mathematics. You have the analysis side in connection with partial differential equations and you have algebra and geometry in the Lie groups. One of the things that is exciting about this field, as I have watched it developing over the last 40 years, is to see so many areas of mathematics come into this field - more and more of combinatorics, geometry and algebra - even though the subject started out with a lot of emphasis on analysis. Of course, one of the central things that make mathematics research so exciting is that, over the course of time, you see that problems that first seemed intractable examined by lots of people who find new ways to approach these problems. For example, problems that were originally posed as questions of functional analysis can now be approached using algebraic techniques, which simply avoid some of the difficult, maybe impossible, analytical problems. I have spent quite a lot of time over the last decade telling the story for the next generation, so to speak, in my collaboration with Nolan Wallach. We wrote quite a big book on representation and invariant theory, trying to make the basic results and philosophy of representation theory accessible to the current generation of mathematicians (and we hope to another generation).

I: Are there any unifying trends in the development of your field of research? Do you think that particular problems have to be solved first before some unifying theory can arise, or do you think that essentially new theories and concepts rather techniques need to be proposed before outstanding problems can be resolved?

G: In my field, it seems to be that there is this cycle of solving particular cases and pushing the methods that suffice for those cases as far as possible. At a certain point those methods often turn out to be insufficient or the computational difficulties simply become insurmountable and then there are new approaches that come in. One of the striking things about mathematics is the insistence to understand the subject from the conceptual point of view. For beginners of the subject, it is hard to understand the concepts without actually doing some calculations. But at a certain point, you discover that even if you have a very powerful computer doing symbolic calculations for you, the calculations alone are not going to tell you what the pattern is. You have to discern the pattern, and I think finding the pattern is one of the main purposes of mathematics. Of course being able to come out with an answer that can be translated into some of the applied domains is also very nice when you can get it.

I: In physics there is some kind of blueprint for the development of the subject whereas in mathematics there is no specific blueprint as to how mathematics should develop.

G: That's right. The remarkable thing in mathematics is that you have these extraordinary imaginative people (like Gromov, Langlands) who propose concepts that, to ordinary mortals like us, seem to just come out of the blue. Of course, they have a basis for those ideas but it can take the work of a lot of people to develop the consequences.

I: There is some perception that pure mathematicians look upon practical applications with disdain. How much of this is true? Some of the best mathematical minds like Hilbert and Poincaré have worked in both pure and applied areas. Is it possible to achieve their status in the present age of specialization?

G: Judging by my own experience, it seems hard to establish links with applied science departments like chemistry, computer science and physics partly because the faculties in those departments themselves have quite a high mathematical level and they generally view the kind of mathematics they are using as something that they are reasonably competent with and they don't seem to have an enormous need for mathematicians. Of course, one can try to create the need, and there is also the tendency on the mathematician's side to think, as the phrase goes, "We would rather build fire houses than to put out fires". But there are remarkable counter-examples. My own personal hero is Hermann Weyl, who is not very well known to the general public. He was a student of Hilbert. He gave the first set of lectures on Einstein's general relativity theory in 1917/1918 and published basically the first book on general relativity theory based on those lectures. He also worked as a forerunner in understanding and explaining what was going on in the new quantum theory in the 1920s. Certainly there are many examples of mathematicians who have done this. In recent years someone like Irving Segal is an inspiring example. Another person who comes to mind is Michael Atiyah. As physics becomes more mathematical and uses a wider range of mathematical tools like algebraic geometry, physicists have to turn into mathematicians.

From the point of view of people completely outside of science, and in particular people interested in what is the worth of mathematics to society at large, they would like to see how mathematics can be turned towards more practical things. It is interesting to observe that in my own field involving harmonic analysis as well as representation theory very recent work in wavelet analysis is essential for things like image compression and data analysis. A lot of that grew out of what used to be thought of as quite abstract kind of harmonic analysis and abstract Fourier analysis. It is a question of having links with applied mathematics. There are people like Coifman at Yale, who, when I first knew him in the 1970s, was working in the kind of representation theory and harmonic analysis that I was. He moved into wavelets and has been very successful in promoting its commercial technology.

I: What instrument do you play?

G: The instrument that I play most seriously is the bassoon. Originally I started out as a child playing the cello but then when I was fourteen I switched to the bassoon, which I consider the most non-linear oscillating system that is of any practical use. So every morning when I practice my instrument I perform experiments on a little non-linear oscillator in the form of a bassoon reed.

I: What made you switch from music to mathematics?

G: My father was a professional musician, a pianist, and I knew from personal experience the difficulty of making a living as a musician. Most of my adult friends in the orchestra that I played in as a teenager advised me that it would be much better to go into science. But it was hard for me not to go into music and composition because that was what I was most passionately interested in at the time. In retrospect, however, mathematics has been a very rewarding career, and I have still managed to maintain an active musical life.

I: Do you think that part of the problem of overspecialization is a lack of communication between mathematicians and people in other areas like engineering, physics and computer science?

G: Yes, that is certainly a problem and I think that is a real challenge for mathematicians. One can almost feel it as a drawback of mathematics that we have such a perfect system of notation that for us the notation serves all the purpose that we want in the same way as the written language serves our purposes. But students and people outside of mathematics often tend to view mathematics simply as a collection of symbols to push around. When the symbols get too complicated, only the professional mathematicians can read them and then people outside the field just turn off. I don't know how to get around that. I teach engineering students a lot and try to explain the concepts in a way that is acceptable to them. I view that as one of the biggest challenge when I am teaching. Of course, it can be quite frustrating because you know as a professional mathematician that with the benefit of an appropriate concept certain ideas can be quite simple. But this is only true if the person dealing with the concept has mastered it, and for people outside mathematics the notation and concept can be so obscure that it is very hard to get the ideas across. I think that is one area in which mathematics, as a profession, sometimes tends to be too narrow. We don't realize that the mathematical ideas are just too dry when used by students outside of mathematics.

I: There are some people who would attack a problem from first principles. They develop their own understanding of the problem and then develop essentially their own methods for the problem.

G: I think the most spectacular example in my own field is Harish-Chandra who, starting in the late 1940s, simply came into the subject of representations of semi-simple Lie groups on his own. There had been very important preliminary work by the Russian school under Gelfand, but Harish-Chandra started at the beginning and created an incredible edifice single-handedly. For a period of about 25 years, starting from the late 40s to the mid 70s, he was so clearly leading the field. that it was only in the early 70s that there was a significant number of other people working in the field. In his case, the methods were always his own. He took what were, in some way, very classical methods and extended them to serve his needs. It has taken several mathematical generations to go beyond Harish-Chandra's methods. His ideas had tremendous depth. Of course, now more recent approaches to the subject try to understand it by other methods, but he basically set the direction in the field. The results achieved were so precise and profound that everybody in the field has to take his methods into account. A parallel instance in mathematics of someone creating a monumental edifice is in algebraic geometry. Grothendieck created very general machinery that has now become the language of algebraic geometry. So I think the absolutely strongest people in the field simply create the field by using their own methods and then the rest of us have to learn those methods and see what other results can be obtained.

I: Is there a role for perseverance? How much inspiration does one need?

G: Oh, absolutely. I think without perseverance you certainly can't do mathematics. If there are never any ideas that come along, it is pretty discouraging. It is an elusive thing. Solving a mathematical problem is trying to judge at any moment whether the track that you are trying is going to pan out. Of course, perseverance alone may not work, but even if it does, you try to know whether you are moving towards a dead end. That can be very discouraging in mathematics.

I: Do you think that mathematics is a marathon race that is long, arduous and lonely?

G: I think there is a partial truth in that comment. But there is such a large social element in mathematics, public perception notwithstanding, in the sense that if you only create mathematics in writing and never tell anyone about it, then it is like running a long race where no one is even looking. I like to think that at least there is this aspect of mathematics as a communal effort. As Einstein commented, there are innumerable problems in mathematics. But I think the ones that have a life of their own are the ones that have a significant number of people (which, of course, in mathematics could be a small number) with some real interest in those problems. And then the joint efforts of people working on these problems make it interesting - you get some results yourself and compare yours with what other people have. So maybe instead of thinking that it is a long marathon race, it is more like a situation I observed once, to my surprise, at a rehearsal of the orchestra. A grand piano was on the floor of the concert hall but needed to be on the stage. I certainly couldn't lift it by myself, but with eight people it was very easy to lift the piano onto the stage. So I think hard mathematics problems may have some of that element of joint effort. Of course, it is one thing to get the piano onto the stage and another thing to get a beautiful performance. We do need the gifted mathematician to give the beautiful performance but the joint effort can play an essential role.