An Interview with Gareth A. Jones

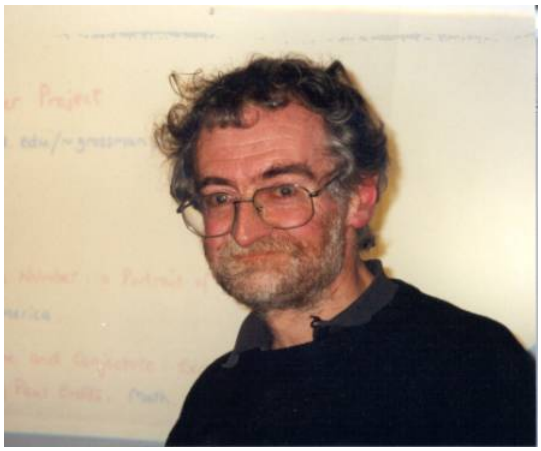
I acquired this piece of information from Nigel Hitchin back in 1986. Let me see if I get it right. You were colleagues in Oxford and during your first year you were taught by Michael Atiyah. What course was it and how was he as a teacher for students just starting university?

Nigel Hitchin and I were students together at Jesus College, Oxford, from 1965 to 1971. Oxford, like Cambridge, consists of several dozen colleges, each containing a few hundred students. Weekly individual teaching is provided by college tutors, but lecture courses and examinations are organized by the University departments. In my time, the mathematics tutors at Jesus College, Edward Thompson for Pure Mathematics and Christopher Bradley for Applied Mathematics, had excellent reputations as teachers, so the standard of mathematics in the college was very high. There were eight mathematics students there in my year: among them, Nigel Hitchin went on to do great work in geometry with Atiyah and Donaldson, while Lyn Thomas, after getting a doctorate in quantum theory, became one of the leading figures in the Operations Research community.

The first-year algebra lectures were given by Michael Atiyah: he was clear, precise, and very fast! His course started at a rather elementary level, with sets, functions, equivalence relations, and so on, but he very soon accelerated, covering as much material on groups, rings, fields and vector spaces as most university courses do in two years. He lectured with such energy and enthusiasm that it was impossible not to be inspired by him: I already loved algebra, and this course confirmed my view of the subject. As students, I don't think we initially realised how great a mathematician he was: I remember asking Edward Thompson for help with one of Atiyah's exercises, and Thompson puffing on his pipe for a while and then saying "You know, Michael is generally reckoned to be rather clever". A few months later (in 1966), Atiyah was awarded a Fields Medal, and then we knew! We had some other excellent lecturers too: Charles Coulson for applied mathematics, and later on, Ian Macdonald for algebraic geometry, though not all of the lecturing was uniformly good.

Before your Oxford days how was your life? I think you come from Wales. Did you realize at an early age that Mathematics was the science you wanted to devote your life to?

Before Oxford, I lived in Cardiff, the capital of Wales. My father was a railway traffic controller, and my mother had been a librarian. They both regretted that they never had the opportunity to go to university, and were very proud when my sister and I did.



Gareth A. Jones

Although I considered other subjects like architecture and physics, I really knew from about 15 years old that I wanted to be a mathematician. As is so often the case, it was good teachers who influenced me, especially one called Howard Williams, who spent many hours giving me individual help. What attracted me to the subject was its elegance, consistency and objectivity. I can still remember the feeling of excitement on finding a really neat solution to a problem: for instance, evaluating

$$I = \int_0^{\pi/2} \sin\theta/(\sin\theta + \cos\theta)\, d\theta$$

by using symmetry to see that

$$I = \int_0^{\pi/2} \cos\theta/(\cos\theta + \sin\theta)\, d\theta$$

and then adding. (It increased the pleasure to discover later the famous story in which the young Gauss used a similar idea.)

You stayed on in Oxford to work for a PhD. Your research was in pure Group Theory and Peter M. Neumann was your supervisor. It may be a romantic view but I

always think of you as a sort of grand-son of the famous triad HNN [1] *...*

In my final year as an undergraduate, I decided to specialise in commutative algebra, topology and group theory. The group theory lectures were given by Graham Higman, with Peter Neumann running the problem classes. Higman's lectures contained a mixture of classical material and his own recent research, which was very wide-ranging. The course was very interesting but also hard work: after each lecture, several of us would spend a whole afternoon together, going through our notes and trying to understand them. This was excellent training for research, and after a few months I knew that I wanted to do a doctorate in group theory. At the time I (unjustifiably) found Graham Higman rather daunting, but Peter Neumann, who was much younger and had a great sense of fun, seemed more accessible, so I was delighted when he agreed to supervise me.

Group theory was very active in Oxford in the late 1960s, under Higman's leadership: counting research students, research fellows, visitors and permanent staff, there must have been at least twenty at a time working on it. The big challenge in finite group theory then was the classification of finite simple groups, and people like John Conway, Don Higman and Charles Sims would come to Oxford and discuss the sporadic simple groups they were constructing. Graham Higman was also interested in infinite groups, especially combinatorial group theory, embedding theorems and decision problems, so we got a very wide education. Atiyah was increasingly influential in Oxford, and I also used to go to his seminars, and those of his visitors, like George Mackey, though few of the other group-theorists did this.

Peter Neumann is the son of the group-theorists Bernhard and Hanna Neumann (who, together with Graham Higman, introduced HNN extensions), and for my diploma dissertation he suggested one of the open problems in Hanna's book on varieties of groups, about identical relations in finite simple groups; this was a good problem to start my research on, because it forced me to spend my first year learning about varieties and finite simple groups, two very different topics. For my D.Phil. (Oxford's version of a Ph.D.), I worked for the next two years on finite permutation groups, applying techniques of Burnside, Schur and Wielandt to groups of prime-power degree.

The algebra research students, mostly supervised by Graham Higman and Peter Neumann, formed a very lively and sociable crowd; they included Peter Cameron, who is now a leading figure in permutation groups and combinatorics, and my future wife Mary Tyrer, working in combinatorial group theory. We held a weekly Junior Algebra Seminar, in which we would take it in turns to give seminars on our particular interests; this was excellent training for a lecturing career, as we could learn from our mistakes without too much embarrassment. We also had plenty of less formal activities, such as punting on the River Cherwell, or squeezing into someone's car and driving out into the beautiful countryside around Oxford, with the latest Beatles record blasting out of the radio.

Over the years your work has spread from Group Theory to several other areas which interfere with it. An important part of it is in the theory of Dessins d'enfants. *The theory was initiated by Grothendieck but I have some idea that you started working on it independently. Was that not so?*

When I finished my thesis, in 1971, I got a lectureship at Southampton. Before then, it had been relatively easy to get academic positions in the UK, but suddenly the expansion of higher education stopped, and it became almost impossible; I think my year were among the last who were reasonably successful in doing this. Mary got a fellowship at New Hall, Cambridge, and I spent most of the 1970s commuting between there and Southampton.

The Mathematics Department at Southampton was totally unlike what I had been used to at Oxford. There were about 18 pure mathematicians, mostly working in differential geometry or topology. There was no real group theory, though a number of pure and applied mathematicians needed to use the subject, and it was made clear to me when I was appointed that I was expected to collaborate with them, rather than concentrate on pure group theory. The need to learn about my new colleagues' specialities, together with the strain of commuting, slowed down my research, but I gradually absorbed a great deal of useful mathematics. Having been trained as a group-theorist put me in a strong position to do this: in almost every case, symmetry played a fundamental role, so that some form of group theory could be applied to the problems.

Two of these collaborations proved particularly rewarding. Keith Lloyd and I have applied techniques from graph theory and permutation groups to problems in mathematical chemistry, and gradually others, such as Mikhail Klin in Beer-Sheva and Reinhard Pöschel in Dresden, have also been involved. Equally fruitful has been my collaboration with David Singerman on maps on surfaces, now more fashionably called *dessins d'enfants*. Around 1970, Norman Biggs, who was briefly at Southampton, wrote a few papers showing how

[1] HNN stands for Higman-Neumann-Neumann, that is, Graham Higman, Bernhard Neumann and his wife Hanna Neumann, famous for their work in Group Theory

maps on surfaces could be described by permutations (an idea originating with Hamilton). He then abandoned the subject, and became a leading authority on algebraic graph theory, but David and I were convinced that there was a significant theory waiting to be discovered. We published a number of papers, and supervised research students in this area, but nobody in the UK took much notice: the subject was too inter-disciplinary for those times, involving a mixture of combinatorics, permutation groups, Fuchsian groups, and Riemann surfaces. In the mid-1980s we were excited to discover that Grothendieck, with some of his colleagues at Montpellier, had also been working on these ideas, and had found some surprising links with Galois groups and Teichmüller spaces. He wrote out a sketch of his ideas, but then withdrew from active mathematics, leaving others to work out the details. There was a very important conference at Luminy in 1993, with people like Fried, Ihara, Itzykson and Serre involved, and now it's quite a thriving subject.

These days with research assessment exercises all over the place some people tend to think that only research work is important for Mathematics and its development. You have written a couple of excellent textbooks and have, for instance, produced a translation of Jean-Pierre Serre's " Complex semisimple Lie algebras" [2]. *It seems you do not share that opinion ...*

Research assessment exercises now play a major role in British academic life: each department undergoes a rigorous examination every few years, with major financial rewards and penalties for success and failure. There is no doubt that some mechanism has to be used to direct limited resources towards those most capable of using them effectively, but the current system is producing serious distortions and injustices, and there is a general consensus that something simpler and fairer must be introduced. One effect has been to place too great an emphasis on research papers and grants, and to devalue more scholarly activities such as editing journals, organizing conferences, writing text-books, etc. Fortunately, research output is mainly judged by its quality, rather than its quantity, and it is enough to publish an average of one good paper each year, preferably in a prestigious journal. My view is that one shouldn't allow one's career to be too strongly influenced by these forces, and that one should concentrate on what one does best.

In my case, I have always enjoyed expository writing, including survey articles, text-books and encyclopedia contributions. I've written a couple of undergraduate text-books with David Singerman and with my wife Mary, based on the lecture-notes for courses we have taught at Southampton, and I hope to publish one or two more in the next few years. I also translated Serre's book on complex semisimple Lie algebras, partly to learn the subject properly, since the classification of finite simple groups, around 1980, meant that one couldn't really understand the groups without the Lie algebras; another reason was to study Serre's style, which I've always admired for its simplicity and directness.

This time you are in Portugal to give a talk in connection with the exhibition of a video on Paul Erdös. Paul Erdös is sometimes referred to as a "problem solver"and his work does not appear to command the same respect and admiration as, say, Milnor's, Grothendieck's or Atiyah's. That is perhaps unfair. What do you think?

Paul Erdös was loved and respected throughout the mathematical world. He lived a nomadic life, with no permanent position or home, travelling between conferences and visits to research colleagues, many of whom were glad to tolerate his rather demanding nature (for a few days, at least) in order to achieve the honour of a joint paper with him. He published over 1500 papers, many of them deeply influential, with nearly 500 collaborators, whereas most mathematicians would be proud to publish 100 in their lifetime.

Erdös won several major prizes, such as the Cole and Wolf Prizes (characteristically giving away most of the money for charitable causes), but nevertheless many feel that his achievements were insufficiently recognised at the highest levels. The classic instance of this is the fact that when he and Selberg found an "elementary"proof of the Prime Number Theorem, it was the latter who got a Fields Medal and a position at the Princeton Institute for Advanced Studies, not Erdös. Perhaps his idiosyncratic approach to mathematics, preferring to tackle problems rather than build theories, was out of step with the prevailing view of the subject. His legendary ability to enter new areas, such as dimension theory, and solve difficult problems without absorbing masses of theory, cannot have endeared him to specialists in those areas. One of his main fields of activity was combinatorics, and even today, despite its rich structure and wide applicability, this subject is often looked down upon as lacking in depth (*"Graph theory is the slums of topology"*, in a famous phrase); perhaps the problems are too easily stated for the guardians of jargon, though the solutions (in Ramsey Theory, for instance) are often notoriously difficult to obtain.

Mathematics is fertile enough to allow many different talents to flourish, ranging from "Bourbakiste"system-builders to "Hungarian"problem-solvers. Although Paul Erdös began his mathematical career nearly 70 years ago, it is still rather early to judge his influence; however, I predict that some of his results and techniques (such as

[2]Springer Verlag, 1987

the probabilistic method) will in future be regarded as among the greatest achievements of 20th-century mathematics.

Outside Mathematics what are your interests? I know you are a keen jogger. Did you not take part in the London Marathon several times?

Outside Mathematics, my main interests are now my family (my son Peter is studying History at Oxford, my daughter Elizabeth hopes to study Electronic Engineering, and Mary is still active in Mathematics), and also running. I try to run about 10km each day, preferably at lunch-time, as a break from the morning's work. I compete regularly, in road and track races, from 800m upwards, and I've represented Wales several times. I've run seven marathons, my best time being 2:26 in London, 1990, but now lack of time for training forces me to concentrate on shorter distances. I used to play a lot of chess, and as a student I came 2nd in the British Junior and Welsh Senior Championships; however, taking chess seriously is too much like doing research in mathematics, and far too time-consuming, so I only play casually now.

(Questions and picture by F. J. Craveiro de Carvalho)

Gareth A. Jones was born in Cardiff, Wales, where he lived until the age of 19 when he won a scholarship to study Mathematics at Jesus College in Oxford. After six years in Oxford he obtained a DPhil for work on finite permutation groups. He was supervised by Peter M. Neumann and also benefited from Graham Higman's strong research leadership in Group Theory.

After Oxford he moved to Southampton where he has been ever since and where he is currently Professor of Pure Mathematics.

Professor Jones has written three textbooks, one in collaboration with David Singerman and two with his wife, the group-theorist Mary Jones. He also contributed a long article on *Symmetry* to Walter Ledermann's *Handbook of Applicable Mathematics*.

Gallery

João Farinha

Prof. João Pereira Dias, summarizing the beginning of João Farinha's academic life, wrote: "...in 1934 he graduated in Mathematics in Coimbra with distinction". After mentioning his "ceaseless teaching work", he added: "Recruited as an Assistant in 1950, the School of Sciences showed its trust the very same year by giving him full charge of several courses; and four years later his position at the School was definitively established with the *Very Good* mention given to his brilliant doctoral examination".

Of those 16 years of "ceaseless work", I followed closely the last six, probably the most important: I met João Farinha in August 1944.

Having finished high school, I was going to stand for the university admission examination. Aware of my mathematical deficiencies, I went to look for the most reputed teacher of mathematics in Coimbra, who then lived in a strange *República*: its name was "Lactarium Paradoxorum", possibly because most of its members had already graduated, or were old enough for it.

A 12-year friendship began that day. I recall the warning he gave me and a cousin of mine: "I can teach you, but I can't promise to be very assiduous because I'm about to be married". The frequency of classes indeed suffered from this. My cousin, who was better prepared, passed the examination; I failed.