Keith Moffatt: Magnetohydrodynamic Attraction >>>


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Interview of Keith Moffatt by Y.K. Leong (matlyk@nus.edu.sg)

Keith Moffatt has, in a long and distinguished career, made important contributions to fluid mechanics in general and to magnetohydrodynamic turbulence in particular. His scientific achievements are matched by his organizational and administrative skills, which he devoted most recently to the Isaac Newton Institute for Mathematical Sciences at Cambridge.

Educated at Edinburgh University and Trinity College Cambridge, he first taught at Cambridge University and was Fellow of Trinity College from 1961. Except for a brief stint as Professor of Applied Mathematics at Bristol University (1977–1980), his career has been centered at Cambridge University, where he has been Professor (now Emeritus) of Mathematical Physics, Head of the Department of Applied Mathematics and Theoretical Physics (1983–1991), and Director of the Newton Institute (1996–2001).

He has been a visiting professor at the Ecole Polytechnique, Palaisseau,(1992–99), Blaise Pascal Professor at the Ecole Normale Superieure, Paris (2001–2003), and Leverhulme Emeritus Professor (2004–5). He has served as Editor of the Journal of Fluid Mechanics and as President of the International Union of Theoretical and Applied Mechanics (IUTAM). For his scientific achievements, he was awarded the Smiths Prize, Panetti-Ferrari Prize and Gold Medal, Euromech Prize for Fluid Mechanics, Senior Whitehead Prize of the London Mathematical Society and Hughes Medal of the Royal Society. He also received the following honors: Fellow of the Royal Society, Fellow of the Royal Society of Edinburgh, Member of Academia Europeae, Fellow of the American Physical Society, and Officier des Palmes Académiques. He was elected Foreign Member of the Royal Netherlands Academy of Arts and Sciences, Académie des Sciences, Paris, and Accademia Nazionale dei Lincei, Rome.

He has published well over 100 research papers and a research monograph *Magnetic Field Generation in Electrically Conducting Fluids* (CUP 1978). Although retired from the Newton Institute, he continues to engage in research and to serve the scientific community. In particular, he is a founding member of the Scientific Advisory Board (SAB), which has helped our Institute (IMS) to find its direction during the crucial first five years and establish itself on the international scene. When he was at the Institute during the annual visit of the SAB, Y.K. Leong interviewed him on behalf of *Imprints* on 6 January 2006. The following is an edited version of the transcript of the interview, brimming with reminiscences and good-humored chuckles, and capturing the excitement of discovery in an important and very relevant field of scientific activity.

Imprints: You already had a first-class honors degree in mathematical sciences from Edinburgh when you went to Cambridge to do a BA. Were the first two years in Cambridge decisive in your choice of research area for your PhD?

Keith Moffat: Yes, in fact my first year in Cambridge was decisive. In those days, it was still quite common for a graduate from a Scottish university to go to Oxford or Cambridge and take the BA. This was the tradition that I followed. I enjoyed fluid mechanics at Edinburgh University but I was also exposed to quantum mechanics, and I thought that my career would be in this subject – that was what attracted most graduate students in those days. It was related to nuclear research and everything else. In my first year in Cambridge, I attended more courses in quantum mechanics at graduate level, but realized in the course of the year that I didn't want to pursue research in that field. I yearned to go back to the fluid mechanics that I had enjoyed so much at Edinburgh. So after one year at Cambridge I took that

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decision and went to see George Batchelor to talk about the possibilities in fluid mechanics.

I: That was in the mathematics department?

M: Yes, Batchelor was in the Mathematics Faculty, but he actually occupied an office in the old Cavendish Laboratory. He was quite close to physics as well. I remember very well my first meeting with him in his office absolutely crammed with books and papers. I had attended his course in fluid dynamics and I liked the subject, and turbulence was the natural area to go into. He was one of the world authorities at that time on the theoretical side. It was obvious that it was a very challenging subject, and it still is!

I: Was your interest in fluid turbulence largely due to the influence of George Batchelor?

M: Yes, he was the authority, and he had other very able people working with him in a strong research group – people like Ian Proudman and Philip Saffman; and G. I. Taylor still exerted a benign influence in the background. There was an atmosphere of great vitality in research in fluid mechanics. George put me on to a problem in turbulence. This was my second year in Cambridge when I started research, although I was still doing my BA.

I: Is it compulsory to do the BA in Cambridge?

M: Yes, I had to take the BA, although Batchelor took me on as a research student on the basis of my Edinburgh degree. He was an Australian and came from Melbourne University. He had a very open attitude (for Cambridge!). He regarded my degree from Edinburgh as quite adequate as a preliminary to research.

I: Did you do any experiments?

M: Not at that time. I was entirely on the theoretical side. I did some very simple experiments later in my career, but not on turbulence. One of the attractions in fluid mechanics is that you are concerned with phenomena that can be seen. You can easily visualize and that appeals to me. I like to do simple experiments. I like to watch, as we all do, the flow of water, for example, and the vortices that develop and the interactions of these vortices; it's fascinating. When an experiment can be easily done – a tabletop sort of experiment – then I will do it, often for demonstration purposes for students. It's interesting how often when you are preparing a demonstration for students, it raises more questions and leads to research problems.

I: As a child, were you already interested in observing physical phenomena?

M: I think most children are interested in what they see around them, they are curious about the behavior of mechanical things. It's one way to get children interested in science, trying to understand what we see around us. But, no, I think my real appreciation developed much later in life.

I: To be more specific, were you fascinated by the flow of water as a child?

M: Well, I always enjoyed water, I must say. Coming from Scotland, we were frequently on holiday either at the seaside or in the country where we have wonderful rivers and mountain streams. Yes, I would sit for hours watching the swirling flow.

I: Is magnetohydrodynamics mainly applied to astrophysics?

M: That is certainly one important field of application, but by no means the only one. Magnetohydrodynamics (MHD) has applications equally in geophysics, notably to the dynamo problem of generation of the Earth's magnetic field. Then there's the intensely practical problem of controlled thermonuclear fusion: the challenge is to contain a very hot ionized gas using a magnetic field. Many MHD problems arise in this context concerning existence, structure, and stability of magnetostatic equilibria. There has been huge activity in this area dating from the 1950s and 60s. Then there's the whole area of liquid metal MHD, relevant for example to processes of flow control in the continuous casting of steel, and other metals and alloys. And in the developing of new materials, there's a process called crucible-free casting: you have to contain a sample of liquid metal in extremely pure form and you can do this by using magnetic levitation. There are many important practical applications of this kind.

I: You mention magnetic levitation. Some trains work on that principle.

M: It's a similar principle. But there you are levitating a solid structure. To levitate a fluid with its infinity of degrees of freedom, there are delicate problems of stability.

I: Are there any other practical problems?

M: Well, there's a host of stirring and mixing problems using magnetic fields. If you use an alternating magnetic field, for example a field rotating at high frequency, you can generate rotational flow in a container, and by carefully crafting the field, you can generate quite complex flow fields. If you are interested in mixing, this is a valuable technique, which is more sophisticated than using a spoon! Again, there has been a lot of work in this area since the 1960s.

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I: Do magnetic fields have destructive effects, like radioactive particles?

M: No, they don't seem to have. On the contrary, the magnetic field of the earth protects us from very damaging radiation from outer space and it's very fortunate that we have a magnetic field serving as a protective blanket. I don't know what happens if the human body is subjected to an extremely strong magnetic field; it's not altogether known what the effects might be. It's better to avoid it. Experimental work in MHD can be dangerous not because of the strong magnetic fields that are used, but rather because you are dealing with very volatile substances. Even mercury is dangerous: it wasn't recognized in the fifties, but it's now well-known that the vapor from mercury is very poisonous, and so MHD laboratories using mercury have to be very carefully designed to meet health and safety regulations. Liquid sodium and potassium alloys are very high conductivity metals, which are used for experiments and are extremely dangerous, extremely inflammable. So you must avoid any possible leakage in an experiment.

I: Does every substance have a magnetic field?

M: Well, at the atomic level of microscopic fluctuations, yes. But in MHD one is dealing only with fluids that are good conductors of electricity, either liquid metals or hot ionized gases.

I: How much progress has been achieved in fluid turbulence, at least in MHD?

M: Progress in turbulence at the fundamental level is extremely slow. You sometimes take one step forward and two backwards! This applies even to the most fundamental theoretical development in turbulence, the theory of Kolmogorov (1941) which essentially boils down to inspired dimensional analysis. Even Kolmogorov recognized a fundamental flaw in his theory, and he published a revision (his updated thoughts) in 1962, some 20 years later. At that stage, he himself undermined his own theory! One of the 'firmest' foundations of turbulence from that point on became very shaky. This is typical of the history of the subject.

I: I think it was Feynman who said that turbulence was the major unsolved problem of classical physics.

M: I thought this went back to Einstein. You may be right, it may be Feynman. He was certainly concerned with turbulence in some of his writings. I think it is true to say that at the fundamental level, turbulence is still not fully understood. There are many approaches – mathematical, physical, engineering – and these are very different. You hope that there is some common ground at the center where real progress can be made. As regards MHD turbulence, the news is good; in fact, I think the greatest advance in understanding did come in magnetohydrodynamic turbulence and it came in the 60s. It came through what is now described as mean field electrodynamics where the turbulence is on small scales but you are concerned with evolution of the magnetic field on a much larger scale, so you have scale-separation, allowing you to average over the small scales and focus on what happens on the large scale. This works fairly well for MHD, and the application is very important both in astrophysics and geophysics.

I: Is it a statistical approach?

M: There is an averaging involved in it, so to that extent it is statistical, but it's a fairly rudimentary sort of statistics. You take care of non-linear effects through this averaging but there is great subtlety in the process. The great leap forward was in this area. I was lucky to be involved through recognizing the relevance of a quantity called helicity in turbulence: this is the correlation between velocity and vorticity. It relates to distinguishing between right-handedness and left-handedness. The physicist would describe it as a measure of the breaking of chiral symmetry, and it is an extremely important concept in MHD turbulence. This realization developed in the late 60s and gained acceptance through the 70s; that was the great breakthrough. So in this area at least, we can look back on the last 50 years and say "Yes, we have a big increase in understanding". But still now, when we look at pure turbulence, the undiluted problem with no magnetic effects, I don't think we have any such great increase of understanding.

I: You mentioned Kolmogorov's work on turbulence. Did Batchelor try to elucidate on his work?

M: Yes, this was his early work, just after the war, in 1946/7 when he came to Cambridge and worked under G.I. Taylor, although he was from the beginning very independent. He unearthed Kolmogorov's papers from the bowels of the Cambridge library, studied them very closely, gave his own lucid interpretation, and gave them very wide publicity. It was through Batchelor's work that the theory became widely known in the West.

I: Do you think that in the next decade or so there will be conceptual breakthroughs in turbulence or do you think that computers will play an even greater role in understanding turbulence?

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M: We must always remain hopeful! I think the answer is: "both". Important progress in turbulence now does come, no one can deny it, from advances in high-power computer simulations. We need these, but computer simulations alone do not lead to real understanding. They have to be coupled with theoretical and experimental work. You really need a three-fold interaction: computer simulation, theory and experiment. There will always be a place for careful theoretical analysis. Computer simulations often throw up new developments: for example, it was computer simulations that detected the prevalence of concentrated vortex filaments in turbulent flows. This immediately led to a new theoretical modeling and the search for an understanding of why these filaments are such a pervasive feature of turbulence. New theoretical insights then suggest new computer simulation experiments; and so on. We need both. As far as the next breakthrough is concerned, for the next 10 years (a good time-scale, I hope) there is one very big challenge and it relates to understanding the way that these concentrated vortices interact when they are non-parallel. In a fully 3-dimensional flow, they tangle with each other in a very complex manner. The big question is whether the associated solution of the Navier-Stokes equation remains smooth, regular for all time, or whether a singularity will typically develop within a finite time. This is an unsolved problem, which dates back to Leray in the 1930s. It is one of the seven millennium problems posed by the Clay Institute for which a prize of one million dollars is on offer. This calls for a mathematically rigorous solution, and that needs new theoretical ideas. The problem is that the computer can never demonstrate either a singularity in finite time or regularity for all time. At best, the computer can provide an indication of a trend, but then theory has to take over to establish that the trend is genuine and that it really does go to a singularity, or to regularity, or whatever. There is huge interest in this problem, which is central to turbulence because if it turns out that singularities of vorticity are a generic feature of incompressible flow, then there must be some means of resolving these singularities. My view is that compressibility must be taken into account on the very small scales at which such singularities occur. Compressibility effects are usually ignored and thrown out at an early stage in turbulence analysis.

I: Do the Navier–Stokes equations apply at the atomic level?

M: No. You do need to adopt a continuum approximation. Obviously that does break down when you get down to the level of fluctuations of density at the molecular level. Even so, the Navier–Stokes equations are still valid down to the level of microns, but not to the level of molecules or atoms.

I: One would have thought that at the continuum level it would be easy to solve the equations.

M: Yes, but it isn't. Within the continuum framework, the equations are nonlinear and dissipative. Also, the incompressible Navier–Stokes equations are non-local in character, because of the long-range influence of pressure. All these things conspire to make it very, very difficult.

I: Have the Navier–Stokes equations been modified?

M: Yes, they have been modified in a number of ways, depending on the context. For example, two-dimensional Navier–Stokes is relatively easy; but in three-dimensions, all hell breaks loose. You can creep towards 3 dimensions – two-and-a-half dimensions, for example, where you take into account some 3-dimensional effects but not all. That's generally where progress is made.

I: I may be simple-minded, but going beyond 3 dimensions may help.

M: Oh, going beyond 3 dimensions to 4? That is possible. There are other examples in physics where you go to $4 - \varepsilon$ dimensions, where ε is formally a small parameter; then having done the calculation, you boldly set ε equal to 1, and you are back to 3. Attempts of this kind have been made in turbulence but so far have had very limited success.

I: What about fractional dimensions?

M: That has a bearing. There was hope in the 70s that new ideas from chaos theory would help to crack the problem of turbulence, but I think that was fairly short-lived. Certainly particle paths are chaotic in turbulent flow, and ideas from chaos theory are relevant to mixing, but they don't solve the dynamical problem of turbulence.

I: Historically, there seems to be a British tradition in applied mathematics (classical physics) that can be traced to Maxwell, Stokes, Reynolds, Taylor and Batchelor. Do you consider yourself to be a successor of this tradition, and how much of it is being continued?

M: Well, it would be pretentious to claim to be a successor of the tradition established by these illustrious names, but I am certainly a beneficiary! I was greatly influenced by Batchelor, and I had a close relationship with him until he died in 2000. I knew G.I.Taylor well also at Trinity College, till his death in 1975, and I had frequent opportunities to talk with him informally in the college. Taylor told me he had attended a lecture of Lord Kelvin in 1904; this is another name I would add to your list – a very famous name in

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classical fluid mechanics (I would also add Lord Rayleigh). Kelvin knew Stokes very well and they had an incredible correspondence that extended from 1846 until the death of Stokes in 1903. Through G.I.Taylor, I have this remote link with Kelvin and Stokes! Three years ago, we commemorated the centenary of the death of Stokes in Cambridge and I immersed myself in his papers in fluid mechanics and gave a lecture on this subject. I do have a strong feeling for the achievements of Stokes and his relationship with Kelvin. Of course, Maxwell comes into the picture, and I feel an affinity with him too. (Like Maxwell, I was born and educated in Edinburgh.) He was, of course, a very, very great figure in science, increasingly regarded as being in the same league as Newton and Einstein. What a tragedy that he died so young! That was right there in Cambridge, where he was first Head of the Cavendish Laboratory and Fellow of Trinity College. As regards Reynolds, he was a Professor at Manchester, famous for his experimental observation of the transition to turbulence in a pipe and the fact that this apparently occurs at a critical value of a dimensionless parameter that later became known as the Reynolds number. So yes, I guess he's part of this great British tradition in fluid mechanics. I'm certainly happy to have been nurtured in this tradition.

I: What about its continuation?

M: I think it is still strong in Cambridge and the UK. We can't claim Kelvin at Cambridge because he spent his whole career as Professor of Natural Philosophy in Glasgow, although he had been a student at Cambridge and made frequent visits there, particularly to interact with Stokes. It is a peculiarly British tradition. The strength of fluid mechanics in the UK is a consequence of Stokes, Kelvin, Rayleigh and Taylor. Taylor didn't have that many students but his influence in the UK remains strong, particularly in my department (DAMTP) in Cambridge where a dominant theme is fluid mechanics and its many applications. I think this will continue but the nature of the investigations changes and, of course, the computer revolution plays an important part. You can't do research in fluid mechanics nowadays without being involved in computational work at the same time.

I: Are you still able to attract good students to do fluid mechanics?

M: Personally, no, because I reached the retirement age in Cambridge three years ago. I've had a very good research student from Poland these last three years (Michal Branicki) who has just completed his PhD, but it's not normal to take on new research students after retirement. It's possible but unusual. Most research students would wish to be with younger members of the faculty. There is a good continuing recruitment of research students into fluid mechanics in the department, certainly. Its applications are traditionally in the physical sciences and engineering, but the range of applications now embraces biological sciences, geophysics, and astrophysics as well; so it's very broad!

I: It used to be that the understanding of the term "applied mathematics" in the UK is different from that in the US. How much of this is it still so?

M: Well, I talked with Avner Friedman about it this morning. I agree with you that it used to be different, but the use of the term is now converging. Even within the UK, people would differ on what they mean by "applied mathematics". Even the distinction between pure and applied mathematics has been eroded, and quite rightly so. People don't like to use the term "pure mathematics" anymore, because some areas may be quite pure in one epoch and turn out to have important applications in the next. One of the functions of the Newton Institute is to surmount interdisciplinary barriers, particularly between pure and applied mathematics. It's one subject – mathematics and its diverse applications. For me, applied mathematics is mathematics applied to the physical and biological sciences. But some would extend the term to cover the social sciences also. Financial mathematics, for example, is that applied mathematics or isn't it? It's what you are practicing. The boundary between theoretical physics and pure mathematics has certainly been eroded. There is a very strong interplay between the two fields now and they are mutually beneficial.

I: The physicists do not seem to be very happy about that.

M: But there are some brilliant exponents like Michael Atiyah who started in pure mathematics but who gradually embraced theoretical physics. Maybe that's what the physicists are not too happy about, but it's a fact of life. Theoretical physics is a close partner of applied mathematics. My department back home is the "department of applied mathematics and theoretical physics", reflecting that these two disciplines are separate but related; the boundary is flexible!

I: I think that in the US applied mathematics is more about applications outside the physical sciences.

M: Perhaps you have in mind applications to economics and the like. That is possible, but Avner would dispute this, I think. Perhaps the interpretation of the term has changed in the United States. It is difficult, I agree with you. Someone in the States working in fluid mechanics would be more likely to be attached to a department of engineering than a department of mathematics. It is regarded as being more within the ambit of engineering. In Britain, we succeeded in keeping this kind of applied mathematics – fluid and solid

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mechanics – within faculties of mathematics. In this way, top mathematics students can be more easily attracted to the subject.

I: Did you have many graduate students during your career?

M: I did have a steady stream of graduate students and this is one very rewarding aspect of an academic career. I have been very fortunate to have had many good graduate students in my time. They are all good but some of them are absolute stars – people like Juri Toomre, Andrew Soward and Michael Proctor, who have done extremely well in their subsequent research careers. It is rewarding to work with graduate students, when mentoring develops progressively into collaboration. That is why it is so satisfying and it keeps one young. The new students coming in, of course, get younger and younger! The fresh excitement with every new graduate student is something very rewarding.

I: You succeeded Sir Michael Atiyah as the Director of the Isaac Newton Institute for Mathematical Sciences in Cambridge. What is your most satisfying accomplishment during those five years as Director?

M: I inherited from Sir Michael a very exciting development – the Newton Institute was 5 years old when I took over – the appointment as Director is for five years. The primary role of the Director is to maintain a high level of scientific visitor research programs and I think I did this during my 5 years, and this still continues. But as well as maintaining the high scientific level, the Director has the responsibility to maintain the financial health and viability of the Institute, and that is not easy. I was very much concerned with the financial health for the longer-term future of the Institute. I was in constant debate, and indeed argument, with our National Research Council (EPSRC) to maintain an adequate income level, and equally with Cambridge University and its Colleges for internal support. I was also constantly seeking to raise money from external private sources, and I was reasonably successful in this. During my time, we won one of the Queen's Anniversary Prizes for innovation at the Newton Institute, on behalf of Cambridge University. And to celebrate the millennium year 2000, we produced a series of 12 posters to demonstrate the enormously wide scope of applications of mathematics. They were reprinted by World Scientific in Singapore and have been widely distributed in Southeast Asia as well as in Europe. We have reproduced them in this little booklet. It gave a fair spectrum of the applications of mathematics. That was quite exciting and involved a lot of work during 1999 and 2000.

I: Were they distributed to the schools?

M: Yes. They were designed for display in the trains of the London Underground with the general title "Maths goes Underground". Each month a new set of posters appeared in the trains. After that they proved popular and there was great demand from schools. So we reprinted and distributed to all schools and universities in the country. They were all over the place.

I: Our own Institute for Mathematical Sciences is modeled partly after the Isaac Newton Institute. What do you think are the similarities and differences between these two institutes?

M: I was first aware that IMS was to be modeled to some extent on the Newton Institute when your Deputy Prime Minister, Dr Tony Tan, visited the Newton Institute in 1998. He came with Louis Chen, looked carefully at what we were doing, and we had a long discussion. This is how I became involved in IMS. There are similarities – the idea of holding programs and bringing in visitors from overseas, this is at the heart of the business of any visitor research institute: short-term programs of up to 6 months duration, with as many distinguished visitors as you can attract to come and engage in research, and interact with the local community. That is very much the spirit of the Newton Institute also. As regards the differences, the Newton Institute has a wider catchment area – the whole of Europe is at its doorstep. Many of the participants and many of the young postdocs and graduate students come from Europe. It's now very easy for Europeans to fly into Stansted Airport near Cambridge from anywhere in Europe. So there is a very large community there. It's not only Europe, of course. We have many visitors from the United States and from all over the world, but primarily you look to your local community. Of course, Singapore has a strong local community but it is relatively small. You have a wider Asian community. I think that IMS must regard itself as a beacon for that community, and extending to Australia. If you look at the globe, you can see that Singapore can be an attractor in a certain area. You have currently tremendously strong growth from China in particular.

I: But the local scientific level is lower …

M: Well, perhaps, but the ambition of IMS must be to raise that level to reach equality with the institutes in Europe and the United States. I think it's doing very well in that respect. The level of the programs here has been high. I think it is more difficult to maintain that level of activity here given the geographical isolation and the fact that you've got a smaller community in Singapore itself. The first 5 years is always easy. The second 5 years and the third 5 years – to maintain sustainability – will be more difficult. There is quite a

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problem there of just keeping it going at the required level of intensity.

I: Has this to do with the "critical mass" needed?

M: Yes, it is a question of critical mass. There is also the question of diversification. "Mathematics and its applications" has to be interpreted in a broad sense. Any area of mathematics and its applications may be a potential area for a good concentrated program. The programs here tend to be of a shorter duration. It's difficult to get people to come and stay for more than a month or two. They'll come for a couple of weeks or one month for workshops, but to stay for a longer extended period is quite difficult. To maintain a research activity for up to 6 months is not easy. Two months seems to be workable and a good compromise. If IMS runs 4 or 5 programs in a year, each of two months' duration, I think that's excellent and can work very well.

I: Do you think we should be focused on certain topics rather than spread out over a large number of areas?

M: Well, despite what I just said, I do think it's good to focus here on topics that are most relevant to Singapore – local problems. It's interesting that environmental problems are emerging as one of the key areas. Environmental fluid mechanics is important in relation to problems of pollution, and problems relating to natural hazards. That tsunami was so close to Singapore that it must have been a matter of great concern here. Phenomena relating to extreme weather conditions are of ever-increasing concern, and these fall well within the scope of mathematical investigation. And then there is the whole vast field of biomedical science – another area in which Singapore can make great contributions. This is an area also where mathematics can play a vital underpinning role.