Mathematical Conversations

Brian Launder: Modeling and Harnessing Turbulence >>>

Brian Launder

Interview of Brian Launder by Y.K. Leong

Brian E. Launder made important contributions, both experimental and mathematical, to fluid mechanics and convective heat transfer and pioneered the use of mathematical models to study turbulent flows. He was at the forefront of the development of numerical methods for turbulence models. He has also applied his methods to industrial problems related to turbomachinery and was active in leading research on environmental issues. His influence in engineering is extended through his wide and deep collaboration with his numerous students and other researchers.

He had his university education at Imperial College and MIT. Except for a short teaching stint at University of California at Davis, he taught mainly at Imperial College and from 1980 onwards, at University of Manchester Institute of Technology (UMIST), where he variously headed the department, the Thermo-Fluids Division and the Turbulence Mechanics Research Group. He was Chairman of UMIST's Environmental Strategy Group, Director of the Mason Centre for Environmental Research and Regional Director of the Tyndall Centre for Climatic Change Research.

His scientific contributions were recognized by leading professional and scholarly bodies, and he was elected Fellow of the following professional and scientific bodies: Institute of Mechanical Engineers (IMechE), American Society of Mechanical Engineers (ASME), Royal Aeronautics Society (RAeS), Royal Society and Royal Academy of Engineering. He was Editor-in-Chief of the International Journal of Heat and Fluid Flow, an assessor for leading French institutions and advisor to Stanford University's Center for Turbulence Research. Though recently retired from UMIST, he continues to play a leading role in the Turbulence Mechanics and CFD Research Group.

He took an active part in the Institute's six-month program (July – December 2004) on turbulence. When he came to the Institute for a second time, the Editor (Y.K. Leong) of *Imprints* interviewed him on 16 December 2004. The following is an edited account of an illuminating revelation of his thoughts about a life-long fascination with an awe-inspiring physical phenomenon that is mysterious and gradually beginning to be fathomed and understood, if not harnessed.

Imprints: Do you consider yourself to be an engineer or an applied mathematician? Do you carry out laboratory experiments?

Brian Launder: Yes, I am an engineer but one who always enjoyed mathematics. When I applied to university at the age of 17 or 18, it became a matter of choice whether I became a mathematician or an engineer. I decided that my mathematics was good but not sufficiently brilliant to be a stellar mathematician. I thought, and I think it was a correct choice, that I could contribute much more to engineering at an applied level than by following a mathematics course. On the second question: As an undergraduate, I had a final year project in boiling heat transfer and that was experimental. I was fascinated by that. I wanted to do that for my doctoral study. So I applied to MIT to do my doctoral work and was offered admission to do boiling, but the gas turbine laboratory at MIT offered me a research assistantship which meant that I didn't have to do teaching-assistants' duties to earn my living. I would be paid to do research but, of course, the gas turbine lab would be concerned with gas turbines. From that point on, I forgot about boiling heat transfer and I concerned myself with the types of flow that arise in gas turbines.

I: Would it be correct to say that subsequently you were more theoretical?

L: No, even at MIT my work was experimental. It was certainly related to turbulent flow. So it was there that my interest in turbulent flow arose. But later on, I did move to mathematical modeling of turbulence.

I: Your CV mentions a number of doctorates to your credit, could you tell us a bit more about your graduate training?

L: As I have just mentioned, I decided to go to the USA for my graduate work. I applied to half a dozen institutions there. I had no idea what was a good university and what was a bad university in those days. I got several offers – one from Princeton, I recall, and one from Yale, and then an offer from MIT came through. I was advised by the professor at Imperial College to take the MIT choice. I did my masters and doctorate at MIT and then I came back to join the staff of Imperial College as a lecturer.

Continued on page 11

Continued from page 10

I: It appears that you have more than one doctorate.

L: Ah, yes, let me go on to say that three of the other doctorates I have are what we call higher doctorates. They are awarded on the basis of substantial body of published research - the first of those from the University of London I think I obtained in about 1976 when I was working at the University of California. I have to say that I got it purely to increase my salary. I don't know if they were impressed but I did get a small increase in salary as a result. Of the other two, one was awarded by the University of Manchester and the other by UMIST. In accepting those there was no effect on my salary. Nor did I expect any. I took them to show my commitment to Manchester. I submitted different research for each one and they were submitted at roughly ten-year intervals. It took me about a decade to accumulate enough new research to submit for a possible award of a higher doctorate.

I: Why did you go to MIT and not stay at Imperial College for your graduate work?

L: Well, I was interested in heat transfer and did think of staying at Imperial College, but the head of the Heat Transfer Section Professor Brian Spalding came and gave us a talk when I was in my final undergraduate year. Basically, he said, "Well, don't do your PhD here. The College is being rebuilt, you will lose a year of effort if you try and do your doctorate in this building at the present time." He was very honest and it is characteristic of him. He always says what he believes to be the truth. That stimulated me to look elsewhere. Since I thought Imperial was the best in England, if I wanted to look elsewhere, I naturally looked to America.

I: How did you become interested in turbulence research? Were you interested in turbulence right from your graduate days?

L: As I indicated, I got shifted into gas-turbine problems - problems of aerodynamics. I was given a free choice for my PhD project and I looked through maybe 150 alternative topics that the laboratory offered. The one that I chose I thought was very interesting. As an undergraduate, I learned a little bit about the transition of laminar flow to turbulent flow. I had always assumed what all the textbooks said: that was a one-way process; that is, once you get into turbulent flow, you never go back to laminar flow. But one of the projects that was put forward at MIT reported that the Russians had done some experiments suggesting that you could go from turbulent flow back to laminar flow if you accelerated the flow sufficiently quickly. This seemed a fascinating question to me. So I decided to look at it in more detail. The Russian paper had been complicated because the flow was supersonic and effectively they were looking at the flow around a projectile where there was a Prandtl-Meyer expansion wave, and effectively, in passing through the expansion wave, the turbulent boundary layer got peeled off and the laminar boundary layer grew up beneath. I was asked to look whether in subsonic flow one could get such a phenomenon. That was what I did for my master's and my doctoral theses.

I: Did it ever occur to you to become an aerospace engineer?

L: It did occur to me. Indeed, I did make some exploratory enquiries, when I was getting near the completion of my doctorate, about the possible positions. I didn't pursue that for two reasons. Firstly, I was a holder of a Fulbright grant and if you hold that grant, you have to leave the USA for two years after completing your doctorate. I did think of going to Canada, but mainly I wanted to get back to England because my grandfather at that time had terminal cancer and I wanted to see him before he died. So having been offered a position by Imperial College, that seemed the best possible choice.

I: The Clay Mathematics Institute in the United States is offering a million U.S. dollars for the understanding of the Navier-Stokes equations in fluid mechanics. Do these equations apply to all forms of hydrodynamic motion?

L: If one has a fluid that one calls Newtonian, that is to say there is a linear relationship between stress and strain, the Navier-Stokes equations apply to a large part of such flows. Of course, if one is in special regimes like free-molecule flows, then they aren't applicable. What makes them especially difficult to solve, however, is the non-linear convective transport term in those equations coupled with the viscous term. Those two together are very challenging. Also, if the convective term becomes more dominant, then steady solutions, that is to say, solutions independent of time, no longer become stable. You get a transition to a phenomenon that they call turbulence. Occasionally, over a limited range, you can have periodic solutions, but the more usual form is this chaotic motion that is called turbulence.

I: Are the solutions analytically obtainable in principle?

L: Not by formal mathematics. Nowadays, computers are large enough that for a limited range of Reynolds number, one can with the computer solve the Navier-Stokes equations numerically. Of course, there are some analytical solutions for laminar flow.

I: Is turbulence a matter of boundary conditions?

L: No, even if you have perfectly calm inlet conditions, if

Continued on page 12

Continued from page 11

the Reynolds number is high enough (high Reynolds number means that you have only got very weak damping by viscous forces), in that event you will find that small perturbations will grow and you will rapidly end up with turbulent flow even if you have a laminar flow coming in.

I: It's not that it is asymptotically so?

L: No, it is certainly not asymptotic. If you have a high Reynolds number flow (the Reynolds number is a measure of how strong the convection terms are compared with the viscous terms) and you have a laminar flow entering the domain, the smallest perturbation will trigger a transition almost immediately to a turbulent agitated motion. It's not asymptotic, it occurs almost instantaneously.

I: Is it also due to an initial condition, like a high Reynolds number?

L: No, it's simply the ratio between how fast the flow is going, what is the size of the range you are looking at, and what is the viscosity of the fluid. One is looking at things which are not related to boundary conditions.

I: In the real world, the phenomenon of "turbulence" seems to be the rule rather than the exception, and the human mind seems to be able to survive, if not thrive, under conditions of "turbulence". Why is turbulence scientifically so intractable?

L: (*Laughs*) I'm not sure how far I can answer this question. To start with your beginning question, turbulence is the rule rather than the exception in the world we live in. Why is that? It's because the two most important fluids to us (air and water) are, on the scale of fluid viscosity very small in value. If we lived in a world of castor oil, we would find that most of the fluid motions that concerned us would be laminar rather than turbulent. Above a certain Reynolds number, the flow does become chaotic. Unless one is going to make a major numerical solution of unsteady three-dimensional turbulent flow, one is forced to look at the averaged equations and, because of the non-linearity of the equations, one finds extra terms (what are known as Reynolds stresses). So, one is forced to adopt modeling to determine those unknowns.

I: Is there a fair amount of modeling for turbulence?

L: It depends at what level one works. I mentioned that one can, over a limited range of Reynolds number, solve the turbulence equations. If one wishes to do what I call "light modeling" of the equations, one can adopt what we call a "large-eddy simulation". This is in some way like a direct numerical simulation except that one recognizes that turbulence has fluctuations on a scale smaller than the numerical grid one is using and one has to include a model to represent that subgrid scale fluctuations. At that level, a simple turbulence model is the usual choice. Most of the effort is in the numerical solution. The model is just a small part of the numerical scheme that ensures that turbulence is destroyed at the required rate. If one goes to the level of modeling that I work at, however, (this is called Reynolds-averaged modeling), then there is a huge amount of input into the turbulence model because all of the statistical fluctuations are contained in the model of turbulence, whereas at the level of large-eddy simulation, most of the transport associated with turbulent flow comes from the simulation itself and it is only a small amount associated with the model.

I: Are the models you mentioned verified by experiments?

L: Indeed, one verifies them either by experiment or increasingly nowadays, by referring to large eddy simulations or direct numerical simulations. There are models that inevitably don't cover all turbulent flows. They will have a range of applicability. Modelers try to make the range of applicability as wide as possible. A one-flow turbulent model is of no good to anyone.

I: Feynman once said that turbulence is the most important unsolved problem of classical physics, and Heisenberg was reputed to have said that he had only two questions to ask God: "Why relativity?" and "Why turbulence?" How much nearer are we to a clear understanding of turbulence?

L: I'm not sure I can respond to this question in a meaningful way. I think that "understanding" is such a personal state, it's almost like religion. For myself, I feel remarkably assured when I am in tune with direct numerical simulation. If we can numerically solve the Navier-Stokes equations, it's very nice to see that the flow that comes out is what we see when we do a careful experiment for identical conditions. Now, at the level of modeling that I do, I find understanding in looking at the relevant equations (the Reynolds stress transport equations) and gaining insight from the different roles taken by certain terms under particular force fields or strain fields which explain why turbulence behaves the way it does. For example, why is it that, when we have a rotating flow, due to the resultant Coriolis force one initially gets some augmented turbulent mixing on the high-pressure side of the flow and diminished turbulence on the opposite (low pressure) side? One can see this directly just by looking at the equations ... qualitatively at any rate. One can also understand qualitatively why, on the high-pressure side there is a cut-off level beyond which further increased mixing does not occur whereas on the low-pressure side mixing is continuously reduced until the flow becomes quasi-laminar. Yes, I find great insight and understanding of turbulence by looking at those equations. Of course these

Continued on page 13

Continued from page 12

insights are usually not precise enough to give quantitative answers. One needs to provide a model for the unknown terms, to complete the mathematical specification of the equations and then, by solving them, one can get, hopefully, accurate results.

I: So you think that understanding of turbulence has increased tremendously over the last fifty years?

L: Undoubtedly so. I don't say there are no unresolved problems but there has been tremendous progress. Of course, as in many fields in the mathematical sciences, with the coming of the computer and the development of techniques to exploit the ability of the computer, it is inevitable that one sees many changes.

I: Is there a master plan or program conducted by researchers around the world to solve the turbulence problem like what the physicists are doing for, say, a theory of everything in string theory?

L: Turbulent flow covers so many fields from blood flow motion in the human body to atmospheric turbulence. You've got such a large range of scale. People's own interests don't span that range. I said atmospheric turbulence; gosh, stellar turbulence also has a large group working in it. So some areas may have this "grand plan"; it may seem grand but it is relatively focused and limited compared to the range of turbulence. No, there is no "theory of everything" in turbulence. Nor will there ever be, I suspect.

I: Will an understanding of turbulence provide the answers to problems in meteorology?

L: Of course, at one level. We are dealing with air which is a Newtonian fluid and it gives rise to turbulent motion. It is, in principle, described by the Navier-Stokes equations. No matter at what level we attack the problem, whether it is direct numerical simulation, large eddy simulation or the sort of Reynolds average modeling I do, what is so different is the scale. One is looking at thousands of kilometers; yet the smallest motion of turbulence is still as small as they are in the experiments I get involved with – they are fractions of a millimeter. There is really no way one can adopt the same approaches. So while people working in atmospheric turbulence do make some use of the approaches that we adopt, they are almost like a special large eddy simulation. A special feature of the atmosphere is that its horizontal extent is very much greater than its vertical extent, for example.

I: Recently I read in the *Scientific American* that some experiments done by a group at the Delft University of Technology in the Netherlands detected some kind of small eddies or currents that are supposed to be "building blocks" of turbulence. What do you think of that discovery?

L: Well, I'm not sure what words they used. Workers in turbulence, as in other fields, are always wishing to promote what they are doing by making it sound very general. Terms like "building blocks" are frequently ones that come to mind as an attempt to make a very complicated subject understandable by people without any specialized knowledge in the field. At Delft, there are some very strong people in turbulence. One is my first ever PhD student Professor Hanjalic. He has been doing some fine work there, particularly on Rayleigh-Benard convection, in which one takes a pair of horizontal plates at some fixed distance apart. If one then heats the lower plate natural convection is started – that's what we term Rayleigh-Benard convection. You get something like that in the atmosphere. I mention Hanjalic because you asked me about the applicability of our model to atmospheric phenomena. He is originally Yugoslav and lived in Sarajevo that has a long valley. It's a city that suffers desperately from pollution. He has done a CFD study of the Sarajevo valley to help local authorities decide whether they should put a chemical plant with potential emission in one position rather than another. Clearly one wants the effluent gas from the plant to be carried far away. On the atmospheric micro-scale, one is using just the same methods that we were using in engineering.

I: The designs of airplanes and ships are presumably related to the study of turbulent flows in air and water. Yet nature has provided its own designs in the form of creatures that fly and live under the oceans. Has there been any attempt to look for possible answers in the designs of nature?

L: That's an interesting question. The answer is yes, we have. Let me cite a couple of examples. In the late 1970s and early 1980s, we (the community, not just myself) empirically discovered, principally from research funded by NASA, that by putting small longitudinal ridges in the surface you can actually reduce the drag to levels below what you would get on a smooth plate. (This seemed remarkable and it has been established and understood now.) Then people said to themselves, "Okay, we have gone this far. Can we do better?" because they were getting drag reduction of the order of ten percent. Somebody called Bieter Bechert, who was a professor in Berlin and is retired now, looked at the performance of sharks. The great white shark seems to have an ability to swim at speeds faster than what people feel it should be able to, given its size and power and so forth. Bechert's idea then was that the shark must have some drag-reducing feature on its skin. He then took some shark's skin and examined it in detail and discovered that the shark had something like riblets - these devices that had been discovered empirically. But, of course, what the shark had weren't shaped exactly like those that have been evolved empirically. So Bechert launched a major research effort mimicking the shark's skin. Alas, I have to say that Bechert came to retirement before those experiments reached any firm conclusion.

Continued on page 14

Continued from page 13

I: Could he do better than Nature?

L: Looking at the results that he published, I don't think he did better than Nature. Looking at it another way, one can say that Nature had the same idea. I said I would quote two examples. So, as a second example, an area where it is certainly the case that one is looking at Nature is in the development of what I call "micro-sized vehicles". There is a lot of interest, partly from the point of view of surveillance but not only from that point of view, in building flying machines that are really the size of just my fist. Obviously, they will not contain anything except sensors: a small camera, sound recording equipment, or whatever. If you go down to that sort of size, you can't have a fixed wing. It is just not feasible. So various designs of flapping wing devices are being explored. Naturally, one looks extensively at the various solutions that Nature has come up with over the millennia in order to get good ideas.

I: But then birds are not the same as inanimate objects. Birds are "dynamic" but aeroplanes are not.

L: Well, there is a lot of research in this field (it's not an area where I do research), in bringing in basically dynamic response capability into inanimate objects. Certainly it is the intention that smart devices should be embedded in, say, the wings or some other part so that if things are not exactly ideal, this can be sensed perhaps as a perturbation in pressure and there can then be feedback control to change it. However, what man creates in his instruments will be inferior to what Nature, even in a bird or an insect, has evolved over millions of years. Nonetheless, there is movement in that very direction.

I: Do you work on any projects in industry?

L: Well, I have done a wide range of industrially-driven problems sponsored by industry. Let me just mention a few. One has provided research, both experimental and computational, for at least ten years - it's on blade cooling. It may not be generally known that the jet engine becomes more efficient the higher the temperature one makes the air flow coming out of the combustion chamber. The problem is that when it meets the turbine blades, the blades will melt at that sort of temperatures they want to use. So, one puts cooling passages in the inside of the blades. Because turbine blades are very small it is pretty challenging to develop effective cooling systems inside them.

I: Is the cooling done by coolant or by air itself?

L: It is done by air that has been compressed in going through the compressors and is then taken off before it goes into the combustion chamber. The air is heated up by the sheer compression but it's heated even more in the combustion chamber. This high pressure cool air is, I'm guessing here, around 600 degrees Centigrade; that's a high temperature, but it is much cooler than the temperature on the outside of the blades. So that's what protects the turbine blades. So that's one area.

I'm also looking at the trailing vortices that are being created behind an aircraft wing. These are created at the tip of the wing as the aircraft flies through the air, and as you know, it can be dangerous for following aircraft to get caught up in these vortices. We are looking at ways to cause the vortices to die out faster. Finally, I mention research that I'm doing on nuclear reactors where one is trying to work with the people designing the next generation of nuclear reactors to improve better ways of cooling.

I: Has your research resulted in any patents or immediate applications in technology and industry?

L: Although I've been talking about my industrial work, I actually operate at a fairly fundamental level. So it isn't that I discover something that can be patented. So there's no patenting of my modeling work but there is a lot of industrial take up of it. My colleagues and I have advanced mathematical modeling so that it is used in industry. The turbulence model (not just my own work but work of modelers around the world) has altered what has gone into the computer programs that industry use. Much of the industrial computing actually makes use of what are known as the industrial codes. There are now three, perhaps four, major CFD computational fluid dynamics vendors. Certainly the vendors have imported my group's models into their codes. That is sometimes a terrible struggle, I have to say. Some code vendors have got models I produced thirty years ago and they're still using them. They complain that they don't work and I've been producing new models over the last thirty years. It's hard to get them to throw away what's in their codes and put in the new models.

I: It's very surprising to me that, given that engineers are well-known for going for patents generally, you are approaching your work from a more intellectual point of view.

L: It's probably what I'm best at and certainly what I enjoy. You will find people in the field who are more financially driven. I mentioned at the beginning the professor at the time when I was an undergraduate. He was probably the first person to get a serious CFD industrial program (that is to say, commercial); he was a commercially concerned scientist. Another colleague from my early days now has one of the most successful current CFD companies. These people just have different interests and different skills. They are more into the numerical discretization of the equations. I'm more into the physics. Like most academics I suppose, turbulence modelers are much more interested in our subject and the

Continued on page 15

Continued from page 14

associated ideas than turning ideas into money.

I: The work you did on blade cooling must surely be patentable and could have been sold to aircraft companies.

L: Yes, but what we produced then is a software for computing a particular arrangement. What one could devise is an entirely different type of cooling arrangement - that would, I believe, be patentable. But that isn't what we were directed at. We were simply showing industry that we can compute this type of flow because we do both experiments and the CFD work. That enables the industry to say, "We'll use your computer code with its modeling and use that to design for ourselves a more efficient cooling system."

I: Do you have any PhD students?

L: I don't have any at the moment, and the reason is that I'm 65 and until a few months ago, I was going to retire when I reached that age. But my university has offered me an extension of my contract for two years. However, it seems to me irresponsible to take on PhD students at an age when I won't be there to supervise them over the final eighteen months of their doctoral research.

I: What about in the past?

L: Oh, I've had over 40. Yes, that's the biblical number to signify 'quite a lot'. I still have some post-docs working with me - three post-docs at the moment. I also interact with academic colleagues helping them in preparing research proposals and offering advice when it's sought. I'm also under contract to write a book for Cambridge University Press. So, if I can get rid of all the administrative work that clogs up my days (and evenings and weekends!) I hope to make some further contributions to the modeling of turbulent flows. In that respect, the opportunity to contribute to the turbulence program here in Singapore has been a real pleasure.