DEREK WILLIAM MOORE

19 April 1931 — 15 July 2008

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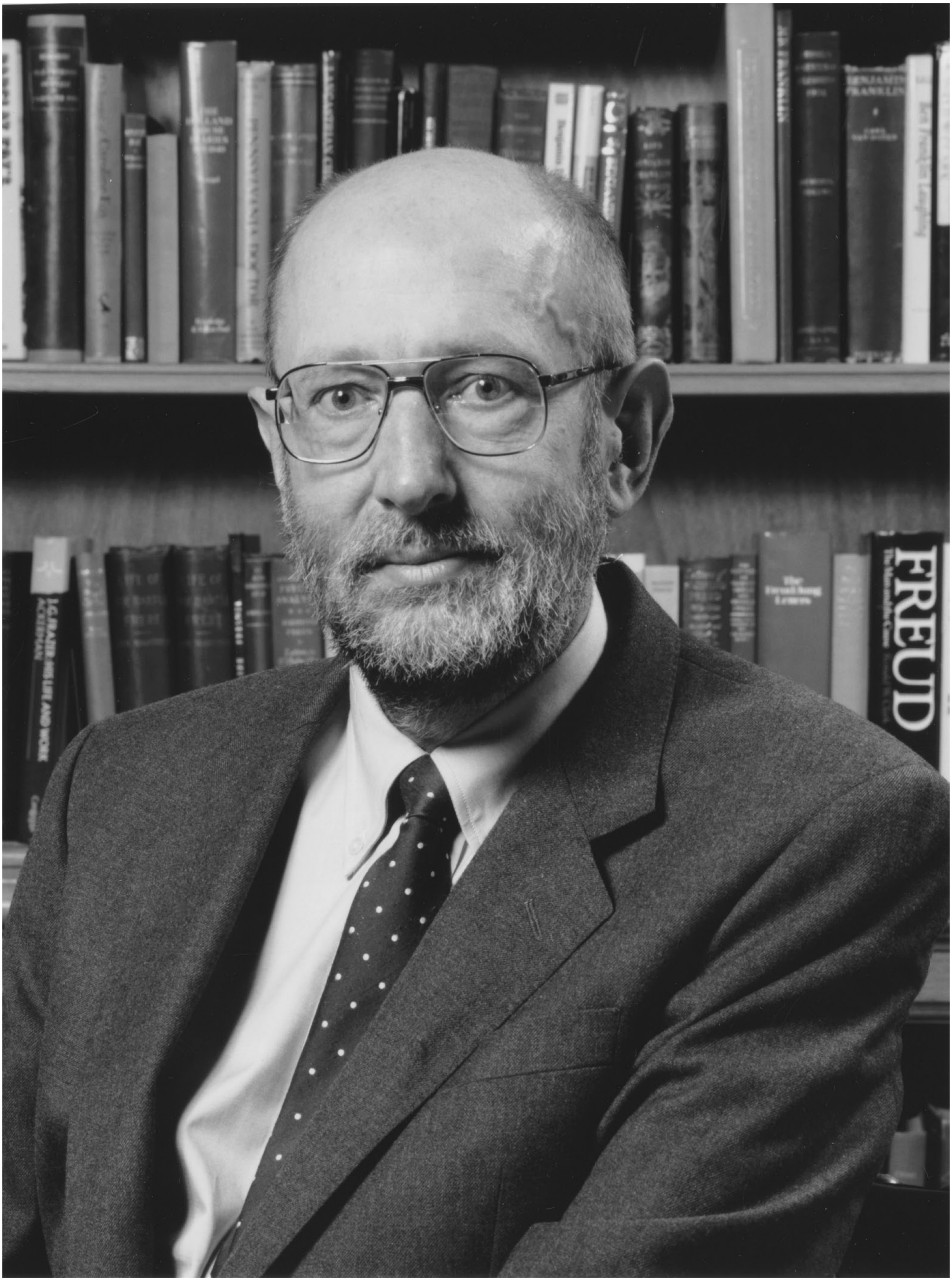

Derek W. Moore

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Elected FRS 1990

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Derek Moore was born in South Shields and studied at the local grammar school, from which he gained an Exhibition to Jesus College, Cambridge. However, before going to Cambridge he did his National Service in the Royal Air Force and was stationed in Yorkshire, where one of his fellow personnel was the poet Ted Hughes.

He entered Jesus College in 1951, studying for the Mathematical Tripos, which he gained in 1954, and for Part III, which he gained in 1955. He then became a research student in applied mathematics and theoretical fluid mechanics under the supervision of Dr Ian Proudman and was awarded a PhD degree of the University of Cambridge in 1958.

Thereafter he held positions at the University of Bristol, the Goddard Space Flight Center, New York, California Institute of Technology (Caltech), and Imperial College, London, where he spent the major part of his career. He was extremely versatile and had a major influence on several branches of fluid mechanics, including the motion of drops and bubbles, mathematical aperiodicity and chaos, rotating fluids, vortex mechanics (in which he became famed for the 'Moore singularity') and the sound emitted from vortices. Other interests included oceanography and astrophysics. An important influence on his career came from his early collaboration with Ed Spiegel in New York, with whom the idea of (what can now be called) the 'Moore–Spiegel oscillator' was conceived and developed. Two other very strong influences came from collaborations with Philip Saffman (FRS 1988), whom he visited regularly at Caltech, and Ted Broadbent FRS, each of whom died in the same year as Derek, namely 2008.

Moreover, Derek Moore had another strong interest in his life: jazz, from which he gained much pleasure throughout his life, in playing, in listening and in discussion and lecturing. Within this memoir the reader will find an appreciation by Peter Batten of Derek's jazz interests and talents (see also Batten 2008*a*,*b*).

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Early years, background and education

Derek Moore was born and brought up in South Shields and was an only child. His father, William McPherson Moore, had been a wireless operator in the Merchant Navy, but later taught the subject at a local college. I came to know and like Mr Moore in his later years, when he lived with Derek in London after the death of his wife, Derek's mother, Elsie Marjorie Moore (*née* Patterson), whom I never had the pleasure of knowing. After studying at South Shields Grammar School, culminating in the sixth form, Derek gained an Open Exhibition in 1948 to Jesus College, Cambridge. However, before entering Cambridge, he was required to do his National Service in the Royal Air Force, serving in Yorkshire. By chance one of his fellow national-servicemen in Yorkshire was Ted Hughes, who also went on to study in Cambridge and moreover was to become distinguished later as a poet and indeed as Poet Laureate.

Derek joined Jesus College in October 1951, graduating in 1954 with first-class honours in the Mathematical Tripos. Intending to pursue an academic career, he then studied for Part III of the Tripos, graduating in 1955. He then had three PhD years, completing his formal studies in 1958.

It was in his undergraduate years in Cambridge that Derek's musical interests really developed strongly, to which I return later in this memoir with a contribution from Peter Batten. Thereafter he became a research student in the Department of Applied Mathematics and Mathematical Physics under the supervision of Dr Ian Proudman. In 1958 Derek Moore was awarded a PhD by the University of Cambridge. Anthony Pearson FRS has commented to me:

> it would not be exaggerating to say that he was shaped by, and helped to shape, the dedicated group of staff and students who ensured that Fluid Mechanics dominated DAMTP for a generation. His contemporaries included Philip Saffman (FRS 1988), Owen Phillips (FRS 1968), Francis Bretherton and Stewart Turner (FRS 1982).

To these names I would add that of Anthony Pearson.

Academic career

Derek Moore then applied for a position at the University of Bristol, where Leslie Howarth FRS was Professor of Applied Mathematics. He was appointed as an assistant lecturer, a position that he held from 1958 to 1960, when he was promoted to a lectureship.

In 1964 he was offered a prestigious Senior Postdoctoral Research Fellowship by the US National Academy of Sciences, to be held at the NASA Institute for Space Studies, New York, together with New York University, where he developed his collaboration with Ed Spiegel. Because of difficulties associated with being granted leave from the University of Bristol, to which I shall return later, Derek resigned and moved to New York, where he lived and worked from 1964 to 1966.

The first of his many visits to Caltech occurred in the autumn, September to December, of 1966; there he worked as a visiting Fellow with Professor Philip Saffman. Many other visits to Caltech (and to Philip Saffman) followed, including 1980–81 and 1986–87, when he was the Sherman Fairchild Distinguished Scholar. (A brief account of Saffman's collaboration with, and high regard for, Derek Moore is to be found in Pullin & Meiron (2013).)

Derek's next permanent position was at the Mathematics Department of Imperial College, London, which he joined in 1967 as a senior lecturer, following strong encouragement from me, from Professor R. S. Scorer, Head of Applied Mathematics, and from Professor H. Jones FRS, Head of the Mathematics Department. Derek was promoted to a readership in 1968 and to a professorship in 1973. He remained there as a full-time academic until he retired in 1996, after which he became a Senior Research Fellow.

Personal life and interests

Little is known, I believe, of Derek's early life. In my experience I realized that he had a very close bond with his father, but he mentioned his mother, Elsie, very little. (However, he was concerned that his mother's prize possessions should be kept in good use, as he gave my wife a set of porcelain storage jars for preserving fruit; they are still preserved!). I came to know William Moore quite well in the 1970s, when he and Derek bought a house in Ealing after the death of Elsie Moore. Derek and William visited us regularly in Wimbledon, and our young daughter, Katherine, who had no living grandfather, developed a nice granddaughter–grandfather relationship with Mr Moore, to the enjoyment of both.

Sadly, William Moore died early in 1979 (28 January). He had become unwell after he and Derek had paid a visit to the cottage in Allendale, Northumberland, which was inherited from Mr Moore's parents. During January he was admitted to hospital, where I last saw him. Derek came to see us on the day when he died, clearly missing his father and saddened by his death. He had no other close relatives as far as I know. He never married, although, as he once told us, he was at one time engaged and, I understand, remained on good terms with his former fiancée.

Derek had affection for the Allendale cottage, which he inherited on the death of his father and which was on the edge of the moors, with its wildlife (including curlews), nearby Hadrian's Wall and the remains of the more recent industrial past (lead mines and shafts). He would often hike the moors, taking care to leave note with a neighbour of his whereabouts (this was the world before mobile phones, although I doubt that he ever had a mobile phone or knew how to use one). He was generous in encouraging friends to stay for a few days, as Christine and I did for at least two visits with great enjoyment; also Ted Broadbent and his wife, Liz, were similarly encouraged to stay there. Derek had friends in nearby Newcastle upon Tyne and visited them when he was staying in Allendale.

It is well known to his many friends that Derek had a sharp tongue and a caustic wit, coupled with humour but not with spite. Here is one illustration: during a lecture in London about dusty gases by Philip Saffman, one mathematician heckled the speaker several times, saying that we should not forget heat generation and refusing (unwisely) to accept the speaker's rational reasons; eventually Derek responded, 'one does not heat a cup of coffee by stirring'! It 'brought the house down'. (Both Ed Spiegel and I were there, and this anecdote is an amalgam of our memories.)

Another interest of Derek's was powerful motor-bikes, and he usually had one in his younger years. (Anthony Pearson recalls riding pillion on Derek's Triumph Speed Twin, of which Derek was very fond, when they exceeded a 'ton' (100 miles per hour) on the A1 at a time when it was not illegal!) This leads me to another anecdote: in Bristol in the early 1960s he went to see his bank manager to seek a loan for a new motor-bike but, sensing inadequate security, the manager gave a severe 'No'. Some days later he and Leslie Howarth went to see the same bank manager about the finances for the annual British Theoretical Mechanics Colloquium, which was to be held in Bristol with Howarth as chairman and Derek as Treasurer. The manager was very ‘frosty’, at least initially, causing Howarth to remark later that the manager was atypically discourteous; Derek never gave the reason!

Another anecdote relates to Derek Moore’s dedication to his profession, not least research. His relations with Howarth, the head of department, were usually good but they were strained when in 1964 Derek asked for two years’ leave to take up the prestigious Senior Postdoctoral Research Fellowship offered to him by the US National Academy of Sciences. Howarth refused permission on the grounds that Derek’s lecturing talents, which were considerable, were needed to teach the Bristol undergraduates. One can sympathize with Howarth’s position, but Derek promptly resigned. To his credit, Howarth retracted his refusal but it was too late. Moreover, on Derek’s return to the UK two years later he did not go to Bristol but joined Imperial College, to its great benefit.

Derek’s interest in music developed in the sixth form at South Shields Grammar School. Here are two recollections of mine: he visited the Jazz Society in Hull on several occasions to lecture about jazz and to play from his recordings, during which he would meet Philip Larkin, the poet, who also had a strong love for jazz. A second recollection is that my friend, Dick DiPrima, who was visiting Imperial College from the USA in the 1970s or 1980s, asked to hear Derek play the tenor saxophone. It happened that he was due to play with a group at Sutton to the south of London, so Dick and his wife, Maureen, and my wife and I went over to Sutton to spend a pleasurable evening with the jazz group of which Derek was clearly a welcome and influential member. A related anecdote is that I know that Derek’s playing of the saxophone was enjoyed very much in the Saffman family circle during his visits to Caltech and by Anthony Pearson’s family circle at their home. An even earlier incident was this: at the Geophysical Fluid Dynamics Seminar at Woods Hole in 1962, Ed Spiegel and Derek took to working all night. However, when they were ‘stuck’ in their work, Derek would play the saxophone for a time; inevitably a complaint came from ‘high up’ that ‘the students were too noisy’. In fact no students were present!

Now follows a more ‘professional’ contribution to Derek’s jazz music from his friend, Peter Batten:

> Derek Moore became interested in jazz while in the sixth form at South Shields Grammar School. He decided to learn to play the clarinet. While on National Service in the Royal Air Force a quiet posting gave him lots of time for practice.
>
> When he went up to Cambridge in 1951 he was able to join a band. Soon he was introduced to the pianist Tony Short, who was a leading member of the University Jazz Club and one of the pioneers of traditional jazz in Britain after World War II. They became firm friends and Derek was invited to play in the University Jazz Band.
>
> Thus began a very active spell as one of the leading jazz musicians in the university, which lasted until 1958. Derek was a permanent member of the University Jazz Band from 1952 to 1956 and appeared occasionally until 1957. His outstanding year was 1955–56, when he developed a remarkable partnership with Dick Heckstall-Smith on soprano saxophone. At the 1956 English Universities’ Jazz Contest he was voted the best clarinet player.
>
> Before a spell in the USA he joined the University of Bristol. For a short time he played the clarinet in an excellent band lead by the pianist Dave Collett, but he had already started to play the tenor saxophone. He loved the recordings of Lester Young and Wardell Gray on that instrument. Very quickly he developed a strong personal style and began to appear at ‘modern’ jazz sessions.

Sadly, the demands of his academic career soon began to limit his opportunities for playing regularly in bands. Nevertheless he continued to practise the saxophone and always welcomed the chance to sit in on a session with friends. Ill health finally made him give up playing in 1991.

But there was another side to his jazz activities. From the beginning of his years in Cambridge he became an avid collector of jazz on record. He soon had a substantial collection, which he was always ready to play for his friends. His taste moved on quickly from a first enthusiasm for early jazz to embrace many styles. His enjoyment and deep understanding of jazz influenced Dick Heckstall-Smith, who became a major figure on the British blues and jazz scene. Many others benefited from hearing selections from his outstanding collection. He also gave several talks about his favourite musicians, but these were sadly far too few: his insight into jazz performance deserved to be more widely shared. In his last years (2008) he collaborated with me (37)* on a long article for *Just Jazz* magazine about the jazz career of his old friend Tony Short.

Jazz was an important part of Derek's life for more than 50 years. It is typical of his generous spirit that he was able to nurture in his friends a similar enthusiasm and enjoyment.

In spite of the connections with Ted Hughes (slightly) and with Philip Larkin (more positively), Derek had no feeling for poetry of any kind. However, he had a strong appreciation of other literature, including philosophy, history, biography—scientific or otherwise—and the novel. He had a great liking for the novels of Barbara Pym, having read and enjoyed almost all of them, although they often dealt with themes, such as Anglican priests and their congregations, seemingly remote from Derek's interests. (As far as I could tell he had no faith, Anglican or otherwise. Such matters were perhaps beyond his experience. For example I recollect his embarrassment when I mentioned having heard Cardinal Hume speak at a meeting of the Wimbledon Council of Christians and Jews. I did not tell him that later I accompanied my wife to the Cardinal's lying in state!) However, he sometimes said that his favourite novel, and in his view the best in the English language, was *Middlemarch* by George Eliot. Who can disagree? Certainly I did not. As to scientific writing or biography I recall his lending me his copy of Peter P. Wegener's book *The Peenemunde wind tunnels: a memoir*, a fascinating account by a 1943–45 'insider' (Wegener 1996). For another reference to Peenemunde see Stuart (2009, p. 115), the memoir of Leslie Howarth, part of whose research at that time was concerned with estimates of the range and accuracy of the V2 rockets being developed at Peenemunde.

In the later period of his life, Derek developed a very good and strong friendship with Professor Susan Brown, a distinguished mathematician of University College London. This gave another enjoyable period of Derek's life and indeed, I believe, of Susan's also. Christine and I were always delighted to see them, whether at our home or at Derek's. I have by me a postcard that portrays Jeremy Bentham, written by Susan on behalf of herself and Derek, to thank us for a party on Derek's 65th birthday and to express appreciation.

Academic work and research

Derek Moore was a talented and dedicated teacher at both undergraduate and graduate levels and was a fascinating seminar speaker. He took all duties seriously, even examining (!), where he took his duties extremely seriously. For example, he was for a time the chairman of a panel for checking examination questions in applied mathematics at Imperial College. If we had

* Numbers in this form refer to the bibliography at the end of the text.

been careless in not recognizing the implications of a question, Derek quickly spotted it and remonstrated in no uncertain terms. To my chagrin I was taken to task at least once.

Derek was an excellent speaker in lecture series, including that of the 1963 Woods Hole Seminar on Geophysical Fluid Dynamics, when he was the Principal Lecturer (5).

He was an excellent listener, and his interest and comments were of great help to young researchers, students, postdoctoral workers and academics generally, so that he was often able to influence others' research in a completely selfless way. It is also true that he was one who listened to the views of others and was not slow to seek or to receive advice when appropriate. For example, Professor Lou Howard and I each told him of the relevance to chaos theory, which was made famous by Lorenz (1963), of his work with Ed Spiegel (8) on non-deterministic (aperiodic) flow, which was published in 1966 with no knowledge of Lorenz's work. Not knowing of the latter research, he was most pleased to see the connection. This topic is discussed later in this memoir.

In short, Derek Moore was an excellent and scholarly academic, whom we were fortunate to have at Imperial College during the major part of his career.

Areas of research

Derek Moore's interests centred on (i) the fluid mechanics of drops and bubbles, (ii) mathematical aperiodicity and chaos, (iii) rotating fluids, (iv) the mechanics of vortices, and (v) the sound emitted from vortices; however, he also had other interests such as oceanography and astrophysics. I shall treat each of his major interests in turn.

Derek Moore's first paper concerned the flow past a rapidly rotating circular cylinder in a uniform stream, especially for the case when the peripheral velocity of the cylinder was far greater than that of the stream. His results, which were of great interest, will be discussed in the section on rotating fluids; however, here I wish to emphasize his concern for mathematical accuracy of the approximations that were made, for accuracy of the numerical schemes and for appropriate comparison with experimental observation. This was the case with his first paper, and this scholarly approach followed throughout his research career, not least in the research (24) that led to what is now known as the 'Moore singularity'. First, however, I shall turn to Derek's work on the fluid mechanics of drops and bubbles.

Fluid mechanics of drops and bubbles

A bubble can be a bubble of gas in a liquid, or a bubble of gas in another (or the same) gas, as in the case of children's play bubbles. In Derek Moore's papers a bubble is of the former kind, a bubble of gas within a liquid. A drop is composed of liquid within another liquid, also within his range of interests.

His first paper (2) was concerned with the rise of a spherical gas bubble in a viscous liquid, producing a formula for the drag coefficient, namely $32/R$, due to the normal stress at the bubble surface, R (which was large), being a Reynolds number based on the terminal velocity of rise (U), the radius of the bubble, and the density and viscosity of the liquid. The flow field outside the bubble was found to be irrotational to a good approximation with the implication, which Moore recognized, that the problem solved was that with a non-zero tangential stress. On the grounds that the flow within the bubble had a negligible effect, this seemed reasonable given that there was 'fair agreement' with observation. However, Moore recognized that a boundary layer might be needed in the liquid at the bubble surface. After the paper's publication, George Batchelor FRS pointed out that another author (V. Levich) had given the result $48/R$ for the drag coefficient but by different means; this caused Moore to reflect that the reason must be the effect of the liquid boundary layer and that (surprisingly) this could give a contribution to the drag coefficient of the same order as Moore's value of $32/R$. This was indeed the case, as Moore found in his second paper on this topic (3), in which he improved on the result of Levich by going to a higher order, namely $(48/R)(1 - 2.2/R^{0.5})$.

Another paper on gas bubbles followed for the velocity of rise of distorted gas bubbles (7), in which the drag coefficient was modified to include the case where the bubble was approximately an oblate ellipsoid, agreement with experiment being 'fair'. A fourth paper discussed the effect of variation of surface tension with temperature on the motion of bubbles and drops. This was written with J. F. Harper and J. R. A. Pearson (9) and showed by order-of-magnitude arguments (supported by detailed arguments for a special case) that the surface-tension gradients would be too small to have an effect unless surface-active substances were present. A fifth paper on the motion of a spherical liquid drop at high Reynolds number was written with J. F. Harper (13), concerning the case of a drop of liquid in another liquid of similar density and viscosity. The boundary layers, both inside and outside, were linear because they were induced by the stress difference and not by a velocity difference, a crucial observation of Moore (3, 7, 13), as was pointed out to me by John Harper. Approximations to the drag coefficient were found and gave good agreement with experiment with (3) for the case of negligible internal density and viscosity as in a bubble.

After a gap of some years, Derek Moore returned to bubbles in 1989 in a paper (31) written jointly with G. R. Baker. They followed an earlier paper of Walters & Davidson (1962), in which consideration was given both experimentally and theoretically to the shape and position of a two-dimensional bubble as it developed under gravity. In their experiments the liquid was confined between two parallel vertical plates, which were closely spaced. Those authors had shown experimentally that a jet is developed at the rear of the bubble pointing into the bubble; however, their theory was valid only for small perturbations from the original shape and could not capture details at larger amplitudes of perturbation. Thus Baker and Moore were stimulated to take the calculations numerically to much larger values of the perturbation. The bubble was assumed to be initially circular and its evolution was calculated by two distinct methods, a point-vortex method and a surface-dipole method. Agreement between the methods was excellent even at large perturbations, giving great confidence in the validity of the results. (Greg Baker told me that a referee objected to the 'unnecessary' use of two numerical methods; Derek was outraged, because agreement between the methods gave a severe test, which should be used more often. The journal editor agreed!) However, the jet at the rear of the bubble was found to be 'taller and thinner' than in the experiments of Walters & Davidson. Surface tension had little effect on this discrepancy. As Baker and Moore commented, it is possible that a very thin film of liquid, which could exist between the bubble (notionally two-dimensional) and the confining boundary might be responsible; a meniscus and contact line could form. Moreover, it was shown that the development of the surface shape is very sensitive to the initial perturbations (31); indeed, Rayleigh–Taylor instability can produce spikes on the upper surface, and this was shown numerically.

Mathematical aperiodicity and chaos

In 1966 Moore and his co-worker E. A. Spiegel of New York University, stimulated by phenomena in astrophysics (the 5-minute solar oscillations), considered the following mathematical problem, which led to a third-order differential equation and to the possibility of aperiodic solutions (8). They considered a system composed of a buoyant element that oscillated vertically in a temperature-stratified (and density-stratified) fluid under the restraint of a linear restoring force, namely a spring obeying Hooke's law. The element exchanged heat with the ambient atmosphere, and the buoyancy force depended linearly on the temperature difference between the element and its surroundings. In this situation the equation of motion for the system was of third order and was formulated in terms of two parameters, R and T, reflecting the astrophysical stimulus, R being analogous to the Rayleigh number of thermal convection and T to the Taylor number of rotating flows. At the time rather little was known of the properties of nonlinear third-order equations; moreover, Moore and Spiegel were unaware of the pioneering work on chaos theory in Edward Lorenz's famous paper of 1963, in which a third-order nonlinear differential system was derived and discussed (Lorenz 1963) and aperiodicity was found. Their lack of awareness may have come from the fact that Lorenz's paper had been published in *Journal of the Atmospheric Sciences*, where it was not noticed by applied (or pure) mathematicians or by astrophysicists. Be that as it may, Moore and Spiegel's paper gives another example of the fascinating phenomena exposed by third-order differential systems; see also Palmer (2009), where reference is made to research parallel to that of Lorenz.

Moore and Spiegel (8) considered several aspects of the problem: (i) they linearized for small deviations and found the criterion in the R, T parameter plane for a monotonic instability or an oscillatory instability; (ii) they considered the nonlinear equation for the adiabatic case, in which thermal dissipation was neglected completely, and showed that there could be one or three equilibrium points with associated finite periodic solutions or a homoclinic orbit; (iii) they considered the case of T/R small and R large, which introduced a simplification, and showed that 'relaxation' oscillations occurred; and (iv) they considered the general case and showed by careful numerical analysis that periodic solutions were possible in certain ranges of R and T, but that in a narrow band in the R, T plane the solution was aperiodic. For example, for $R = 100$, the solution was found to be aperiodic for $T = 30$, 35, 39, 40, but periodic for $T = 45$. An even more unusual aperiodic solution was found for $R = 20$ and $T = 6$. These results established numerically the existence of aperiodic solutions of what we may call the Moore–Spiegel oscillator.

In 1971 N. H. Baker, Moore and Spiegel took the discussion further and in more detail (18). In particular they reconsidered the adiabatic case of paper (8) and then went on to modify the problem by allowing small deviations from the adiabatic situation with small dissipation. In that case they were able to perform an approximate analysis by the method of averaging (Landau & Lifschitz 1960). They studied the problem in terms of two parameters, R (which is taken to be large) and $d = 1 - T/R$. Periodic solutions were obtained in terms of Jacobian elliptic functions. Moreover, by examining the stability of the periodic solutions they were able to determine the range of d (for a given large value of R) for which aperiodicity occurred, thus enabling a better understanding of the reasons for the occurrence of aperiodic solutions in the numerically calculated results, at least for the near-adiabatic system. Indeed, it seemed that an aperiodic solution of a deterministic system was the result of the solution's wandering between a 'skein' of (unstable) periodic solutions whenever those solutions became close enough in phase space. (I am indebted to Ed Spiegel for a clarification of this point. At one time Moore was loath to use the word 'turbulence' in the context of papers (8) and (18), but his view may have changed later.) To examine other possibilities, the authors performed numerical calculations for a more extensive range of the parameters R and T and found still more complexities in the nature of the aperiodic solutions. This showed the great variety of possibilities for the Moore–Spiegel oscillator, and provided a fascinating parallel with results found by others for the Lorenz equations. However, it is noteworthy that Lorenz's equations could be related to a map (see also May 1976), thereby revealing the relation to aperiodicity and chaos in discrete systems; in contrast, Moore and Spiegel, followed by Baker, Moore and Spiegel, found an understanding of chaotic properties through mathematical analysis. The two approaches are complementary, although it must be said that the explanation of Lorenz's equations in terms of a map is perhaps more appealing to the mathematical community.

Rotating fluids

A series of papers (1, 11, 12, 15, 16, 25) were each concerned with the effects of rotation. In the first of these, which is atypical, Moore discussed the Magnus effect, namely the sideways force (or lift) experienced by a rotating cylinder placed in a stream, particularly for the case of a large rotation rate. From a study of the viscous flow equations, he found that the lift was that given by the product of stream velocity, density of fluid and circulation, but that the drag was zero to first order.

In the next paper (11), Howard, Moore and Spiegel discussed the role of the boundary layer between the convective envelope of the Sun and its interior in causing the interior to spin less rapidly, possibly thereby reducing the precession of the perihelion of Mercury. This was a topical issue at the time.

The remaining papers were concerned with a very different set of phenomena, namely those associated with a body moving through a rotating fluid. Much earlier, G. I. Taylor FRS (Taylor 1922) had shown that associated with a body moving slowly parallel to the axis a rotating fluid is a column of fluid (a Taylor column), which is parallel to the axis of rotation and moves with the body as if pushed or pulled. Moore and Saffman (12) considered the effects of rigid (or with the upper wall free) end walls on such a motion. The walls affect the Taylor column, and viscous effects are felt at the walls and on the body as Ekman layers and at the boundary of the Taylor column as layers whose structure had been developed by Stewartson (1966). The drag calculation depends on aspects of the flow that involve these layers, and a formula for the drag is given; if the upper surface is free, the drag is greater by roughly 50%. However, the spacing H between the walls was required to be small compared with R^3W/n, where R is a typical radius of the body, W is the rotation rate and n is the kinematic viscosity. These results were given some qualitative support by experiments of Maxworthy (1968), provided that inertia (nonlinear) effects were small, such differences being perhaps associated with a lack of fore–aft symmetry.

In a later paper (25), by Hocking, Moore and Walton, the distance between the end walls was allowed to be of order R^3W/n, so that the spacing H was of the same order as the length of the Taylor column. Agreement was found in a limit with the result for an unbounded domain (Stewartson 1957), and deviations associated with H in the specified range were calculated. However, agreement with experiments of Maxworthy (1970) was poor; the paper concluded that this was probably due to the inertial (nonlinear) effects rather than the small spacing.

A paper written with Saffman (15) considered the detailed structure of the free shear layers, which occur at the cylindrical boundary of the Taylor column. The axisymmetric body moves slowly parallel to the axis of the rapidly rotating fluid, the Rossby number (U/RW) is small and so is the Ekman number (n/R^2W). Here, as before, U is the velocity of the body, R is a typical radius, W is the rotation rate and n is the kinematic viscosity. Following and extending the concepts of Stewartson (1966), Moore and Saffman elucidated the structures of inner and outer shear layers, using in part the solution of a Wiener–Hopf problem.

A paper by Moore and Saffman (16) discussed the flow associated with a thin disk moving in its own plane through a fluid that was rotating about an axis orthogonal to the plane. They showed that the geostrophic flow outside the Taylor column (given by the Taylor–Proudman theorem) could be calculated by proper treatment of the shear layers (Stewartson layers) bounding the Taylor column. At the same time it was shown that fluid particles that pass through the Taylor column are deflected at the column boundaries as they do so. In an appendix to the paper, Maxworthy illustrated this deflection experimentally and remarkably with a typical angle of 20°.

The mechanics of vortices

What is a vortex? What is vorticity? The reader will excuse me, I am sure, if I explain these terms. At any point of a fluid there is a velocity vector, $\boldsymbol{u}$, which gives the velocity of a particle; if we perform a mathematical operation known as curl $\boldsymbol{u}$, we have the vorticity at that point. Moreover the operation curl is the equivalent of a rotation, and we may regard the vorticity as giving the rotation (or spin) of a particle. This property is very important because we know that if a body is contracted about the axis of spin, it will spin more rapidly. This process occurs in fluids, is known as vortex stretching and is the reason why vorticity is so important in calculating the dynamics of a fluid motion.

At its simplest we may consider a point vortex that would induce a swirl in the surrounding fluid, and for which all the vorticity is concentrated at a point. By extension we could have a line vortex by uniting a set of vortices in a line, which could be curved. Moreover, a sheet of vorticity can be conceived by amalgamating a set of line vortices. (An early researcher in the study of flows associated with such vortex patterns was L. Rosenhead FRS, whose work is described in a memoir by Stuart (1986).) In addition, a vortex ring may be constructed by bending the (curved) line vortex and then joining the ends. These examples involve discontinuities in the vorticity, whereas in contrast it could be distributed continuously in space. Where did Derek Moore's interests lie within this range of possibilities? The answer is that he made contributions in all these aspects; his interests were wide. Moreover, in much of his work in the field of vorticity and vortex motions he and Philip Saffman were close collaborators. However, some papers were by Moore alone, as we shall see, although their collaboration was often influential, as Moore was careful to mention. Thus I shall concentrate on a few particular papers.

In the early 1970s Moore and Saffman studied the instability of a line vortex in a strain field imposed in the plane orthogonal to the axis of the vortex (17, 20). The first of these papers treated long axial wavelengths of the perturbation, whereas the second allows for a range of axial wavelengths. It was found that there is a narrow band of wavenumbers within which the strained vortex is unstable, provided that the perturbation wave does not propagate along the vortex. Thus support was given to the theory of Widnall *et al.* (1974) for the instability of a vortex ring, but curvature effects and the non-propagating nature of the wave gave constraints on the comparison.

If a line vortex is subject to an axial flow, such as occurs in the pair of trailing vortices from the wing tips of an aircraft, further effects arise. These were the subject of a paper by Moore and Saffman (19), but in that paper the axial flow was taken to be irrotational and thus to have zero vorticity. An attempt at treating the more general case of an axial shear flow, that is one with vorticity, was made by Moore (27), at least for a special case. This situation is that of a flow that is independent of the axial coordinate, in which case the azimuthal equation for the swirl decouples from that for the axial velocity, there being a radial pressure gradient to balance the centrifugal force. Moore chose the swirl velocity to be a diffusing line vortex whose solution is known. Then the solution for the axial velocity, which depends on the known swirl velocity, was calculated with the assumption that the similarity coordinate for the swirl was appropriate for the axial velocity. In this case the problem was that of solving an ordinary differential equation but with variable coefficients given by the swirl. The axial velocity field was assumed initially to be dependent on the azimuthal angle. The vorticity of the axial flow was both radial and azimuthal, but attention was focused on the azimuthal component because of large radial gradients. The results showed that, under specified conditions, the azimuthal vorticity was expelled from the neighbourhood of the axis to greater radii, the original vortex being now embedded in an irrotational flow. Quantitative results were given for the radius of maximum azimuthal vorticity. However, the reader was directed to the paper of Pearson & Abernathy (1984), where similar results were found; Moore referenced it in proof, having recognized generously that their paper had priority for the work described.

We turn now to the study of flows involving a continuous distribution of vorticity, focusing on an especially dramatic result. Hill's spherical vortex is a sphere within which the flow is rotational with its azimuthal vorticity proportional to radius, but outside which the flow is irrotational. It is a remarkable property that the tangential velocity components match precisely at the surface of the sphere. It is known that Hill's vortex is an extreme member of a family of vortex rings (Norbury 1973). Moffatt and Moore (22) discussed the problem of the possible instability of Hill's vortex, in which it was found that mathematical analysis associated with numerical studies was significant; however, during the course of their work they learned that Bliss (1973) had studied the problem earlier by numerical means only. Moreover, Moffatt and Moore noted that, subject to certain conditions, Benjamin (1976) had shown that any member of the family of vortex rings has a kinetic energy greater than any neighbouring unsteady solution of the equations, so that a disturbance could not grow at the expense of the parent vortex. The perturbation approach of their paper is consistent with Benjamin's result because the conditions, which were assumed by Benjamin, were not satisfied. Indeed Moffatt and Moore found that the perturbation decayed except in the neighbourhood of the rear stagnation point (22). If the perturbation was roughly of the form of a prolate spheroid, vorticity was swept from the surface and formed a spike of growing length from the rear stagnation point downstream. However, if the perturbation was of the form of an oblate spheroid an indented spike would grow at the rear stagnation point. Diagrams were shown of the shape of the growing spike at different times, thus illustrating this unusual behaviour. In a later development Pozrikidis (1986) followed the nonlinear evolution of the perturbation, and his diagrams give substantial support to the results of Moffatt and Moore.

We now consider work leading to the Moore singularity, which is concerned with the possibility of a singularity developing in a vortex sheet in a finite time from the commencement of evolution. Moore remarked (24) that P. G. Saffman (unpublished results) had argued from ideas of linearized perturbations that a vortex sheet might develop a singularity in a finite time. However, as Moore recognized (24), the role of nonlinearity was likely to be substantial in any endeavour to give firm justification to this idea. Thus this paper was directed towards that end.

Moore based his approach on the Birkhoff–Rott equation (see Caflisch 2012), which is a Lagrangian formulation of the nonlinear equations for the evolution of a vortex sheet in terms of time, t, and the 'circulation' (which is related to vorticity) between two points on the sheet. An initial condition was specified at $t = 0$. The solution for a perturbation of small order E from a flat sheet was expanded in a Fourier series, which was valid so long as the solution remained analytic in the circulation, and a set of ordinary equations resulted for the Fourier amplitudes. These could be solved sequentially, and the numerical results were compared with those obtained by S. Damms (in a personal communication). The results suggested that the time at which a singularity would occur was of the order of $\log E^{-1}$. Moore went on to study the solution for large times in greater detail on the grounds that $\log E^{-1}$ is large when E is small. Therefore the Fourier coefficients were evaluated for large t and also for large Fourier number (n). By the use of generating functions and several transformations, including one of a shift of timescale, Moore was led to a solution for large t. Indeed, subject to certain assumptions about higher Fourier coefficients, this paper gave a rather simple implicit formula for the critical time for the singularity to occur but in which the earlier approximate result, $\log E^{-1}$, was embedded. A comparison with the numerical calculations of Damms is fair; however, there is a small but noticeable difference, as Moore recognized. Thus the more rigorous work of Moore was significant, even though the lack of knowledge of higher Fourier coefficients meant that still more rigour was needed. This was indeed provided by later researchers, including Caflisch, O. F. Orellana, J. Duchon and R. Robert, for which the reader is directed to Caflisch (2012) and the papers referenced there.

Krasny (1986) examined the same problem by a further study of Rosenhead's point-vortex method (see Stuart 1986), thereby obtaining substantial confirmation of Moore's asymptotic analysis of the singularity and also giving justification to the point-vortex scheme. Later still, Cowley *et al*. (1999) studied the problem further and gave greater support to Moore's work, giving also the shape of the vortex sheet as the singularity formed. All this later research activity, which was stimulated by Moore (24), shows the great and continuing importance of Moore's singularity.

Compressibility and the sound emitted from vortices

In several papers, some which were written in collaboration with Ted Broadbent, Moore pursued the effect of compressibility on vortex motions and the sound generated thereby. Broadbent and Moore (23) discussed the destabilization of vortices by compressibility, in effect by the generation of sound waves. Their analysis was related to the theory of Lighthill (1954) for the generation of aerodynamic sound; however, whereas in that theory the flow producing the sound (that is, the source) is prescribed and is unaffected by the sound radiation, Broadbent and Moore's analysis allows for the interaction of the sound radiation on the source in this special case. In that sense, the paper went beyond Lighthill's assumptions, at least for the relatively simple case of a vortex in a compressible fluid. The problem was confined to the plane orthogonal to the axis of the vortex, and viscosity and thermal conductivity were neglected, the flow being homentropic. The vorticity in the core of the vortex was uniform, but that outside was zero. In the incompressible case the vortex is stable, but in a compressible fluid the ability to radiate sound (and energy) means that the perturbations develop the ability to cause the vortex to be modified and thus to be unstable. This is rather surprising, but a discussion of higher-order perturbations would be needed to provide a complete understanding.

Two papers (34, 36) considered problems that are somehow related to that of Broadbent and Moore (23), except that the vortex interacts with an inclined plane, which had a substantial role in the sound generation. Earlier work on this problem had been done by Leppington & Sisson (1997). Broadbent and Moore (34) treated the case of a hollow vortex interacting with a plane, whose normal is inclined at a small angle to the axis of the vortex; however, in the other paper (36) this was improved upon to treat the more realistic case of a Rankine vortex (in which the flow within the vortex has solid-body rotation with irrotational flow outside). Broadbent and Moore treated the situation in which the vortex is created at an initial instant, but because the velocity normal to the plane surface is not zero a compensating potential is necessary. This incompressible flow potential was calculated, following an earlier calculation of Kelvin (1880). But how was the sound calculated? It is in truth related to the theory of Lighthill (1954), in which the acoustic field does not react back on the source of sound. In this case the source is the incompressible flow, which was calculated. Then Broadbent and Moore matched the outer structure of the incompressible potential flow to the inner expansion of an acoustic velocity potential subject to certain assumptions, which were stated (including $Wt \ll 1/M$, where W is the angular velocity at the edge of the vortex core, t is the time and M is the Mach number). Thus the acoustic pressure field was found. Surprisingly, the radiation was at a single frequency, W; moreover, the Rankine vortex was found to radiate far less efficiently than a hollow vortex. The acoustic pressure was found to have a peak on the solid plane with a much broader peak at about 30° to that plane. However, as Broadbent and Moore pointed out, they were unable to complete the formidable task of solving the acoustic equations exactly; in essence this led to a reliance on Lighthill's theory, in which the source is not affected by the radiation; some concern therefore remains about the results found.

Other papers concerned with compressibility included one by Moore alone (26), one with Broadbent (29), and several with D. I. Pullin (30, 32, 33, 35) on a sequence of problems concerned with extending incompressible theories to, successively, a compressible vortex pair, a compressibility vortex pair with heat addition, Hill's spherical vortex and the Stuart vortex array.

Other fields of the physical sciences, including astrophysics, meteorology and oceanography

Derek Moore was a regular and popular collaborator with scientists in astrophysics, including Ed Spiegel, with whom he collaborated on waves in a compressible atmosphere (6) and on the flow of gas in a galaxy (14); Leon Mestel, with whom he collaborated (with Judith Perry also), on expanding quasar envelopes (21), and on radiatively driven shock waves in quasar envelopes (28); and the late Philip Drazin, with whom he collaborated on flow of fluid of a variable density over an obstacle, a topic of Meteorological and Oceanographic interest (10). An article by Moore alone, on Rossby waves in ocean circulation, was published in *Deep Sea Research* (4).

Later life and assessment

In the last years of his life, Derek Moore became increasingly unwell and moved to a care home in Barnet, where colleagues would visit him. One regular visitor was Dr Gerald Moore, who at the College had been very helpful to Derek on matters of numerical analysis. (There were three Moores in the Department of Mathematics—Dan, Derek and Gerald—but they were not related and they were all good friends even though their mail was sometimes confused, perhaps because their interests had much in common.) During Derek's move from his home in Ealing, Dr Stephen Cowley was extremely helpful in so many ways, not least in organizing the move.

Derek was always a most helpful colleague, listening carefully to students and colleagues, young and old, often giving help very generously. Moreover, the reader will have noticed generous references to other researchers, with Derek on at least one occasion attributing priority to a paper that he saw only at the proof stage. For my own part I was always very appreciative of his wisdom and experience.

His research work, which was a mixture of analysis and numerical computation, was always so carefully done both in mathematical analysis and in numerical computation, with 'no stone left unturned', or very few. This is shown clearly in his two most valuable and lasting contributions, the 'Moore–Spiegel oscillator' and the 'Moore singularity'. Those papers were ones of which he could feel justifiably proud.

The reader will have seen the many references to collaboration with Philip Saffman; he and Derek Moore were candidates for election as Fellows of the Royal Society over the same period of several years, and their cases clashed 'head to head' so to speak. George Batchelor and I had been remiss in not staggering their nominations by two or three years. Be that as it may, Saffman was eventually elected in 1988, followed by Moore in 1990. I wish to record that I know that Saffman's wise guidance was very important to the committee in its assessment of Derek's achievements and talents in 1990. An interesting point is that, as well as being close collaborators, Philip Saffman and Derek Moore were born within one month of each other in 1931, and died within one month of each other in 2008.

Finally, while I have been writing this memoir, I have become more and more impressed by the quality of Derek Moore's work and of his generous qualities. It has been a pleasure to bring back memories of a fine friend and colleague. Moreover I know that others who have read the draft of this memoir have also been grateful to have had their memories of Derek refreshed.

Honours

1964–66	Senior Postdoctoral Research Fellow of the National Academy of Sciences, Washington DC
1984	Invited Lecturer at the 16th International Congress of Theoretical and Applied Mechanics
1985	Foreign Honorary Member of the American Academy of Arts and Sciences
1986–87	Sherman Fairchild Distinguished Scholar, Caltech
1990	Fellow of the Royal Society
2001	Senior Whitehead Prize of the London Mathematical Society

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I am indebted to Peter Batten, who was most helpful in preparing his perspective on Derek Moore's life in jazz, without which this memoir would have been incomplete; see also Batten (2008*b*) for his obituary notice in *The Guardian*, and Batten (2008*a*) for the obituary notice in *Just Jazz*. For an unattributed obituary notice the reader is directed to *The Times*, 28 October 2008 and online (http://www.timesonline.co.uk/tol/comment/article5025729.ece?print=yes&...)

Many colleagues and friends have expressed to me their appreciation of, admiration of and liking for Derek as a scientist and as a man of integrity, including Leon Mestel FRS, Anthony Pearson FRS, John Harper, Saleh Tanveer, Robert Krasny, Greg Baker, Dale Pullin and Ed Spiegel.

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