J. Fluid Mech. (1986), vol. 173, pp. 1–14
Printed in Great Britain

Geoffrey Ingram Taylor, 7 March 1886–27 June 1975

By G. K. BATCHELOR
Department of Applied Mathematics and Theoretical Physics, University of Cambridge, Silver Street, Cambridge CB3 9EW

(Received 28 April 1986)

G. I. Taylor was a happy man who spent a long life doing what he wanted most to do and doing it supremely well. He was a natural scientist whose character and activities were perfectly matched, and that allowed the fullest use of his creative talents. This short article is intended to help the understanding of G. I. Taylor's approach to his research by providing some information about the man and his life, in particular the major events in his early adult years which determined the direction of much of his subsequent research. A list of the now rather numerous articles about the man and his family and his work is appended.

Most of us could do with some help in our personal research. No external aid is likely to make an adult mentally sharper and quicker of perception and more imaginative, but there are other ways in which research efforts can be made more productive. It pays to choose a topic with significance and to ask the right questions and to match the technique of investigation to the nature of the problem and to know when further work would bring diminishing returns. In these and a thousand other ways in which approach and judgement are involved we may be able to increase the effectiveness of our enquiries and make better use of our native ability. There is undoubtedly scope for improvement, and all of us, young and old alike, would like to do better. How can we learn?

One answer to this question which has often been given is: go to the masters; study the lives and works of the great scientists, identify the criteria on which they based their choices, consider the attitudes that informed their judgements, see the merits in their mental habits, and be inspired by their example. It is sound advice, and is especially valuable when there are great scientists whose fields of research are close enough to one's own to allow understanding and appreciation of their work in depth.

Thinking of this kind lay behind the choice of character of the international symposium held in commemoration of the 100th anniversary of the birth of G. I. Taylor, one of the greatest of the 'masters' in fluid mechanics and a lovable man with friends all over the world. The Symposium was both a celebration of Taylor's life and a consideration of the value of his scientific legacy. His research began in 1909 and continued until 1972, an extraordinarily long period of time, which gives his later work a contemporary look and his earlier work a classical character. Most of us have an active interest only in some specialized corner of fluid mechanics, but there will be few who have not found a seminal paper by G. I. Taylor which threw a new light on that corner or perhaps even showed that it existed. For fluid dynamicists in 1986, he is probably the most accessible and the most relevant master.

Taylor's contributions have a distinctive character, of which three principal features may be identified immediately. First, they show profound insight and ability to see how things work physically; secondly, they have the elegance and beauty that

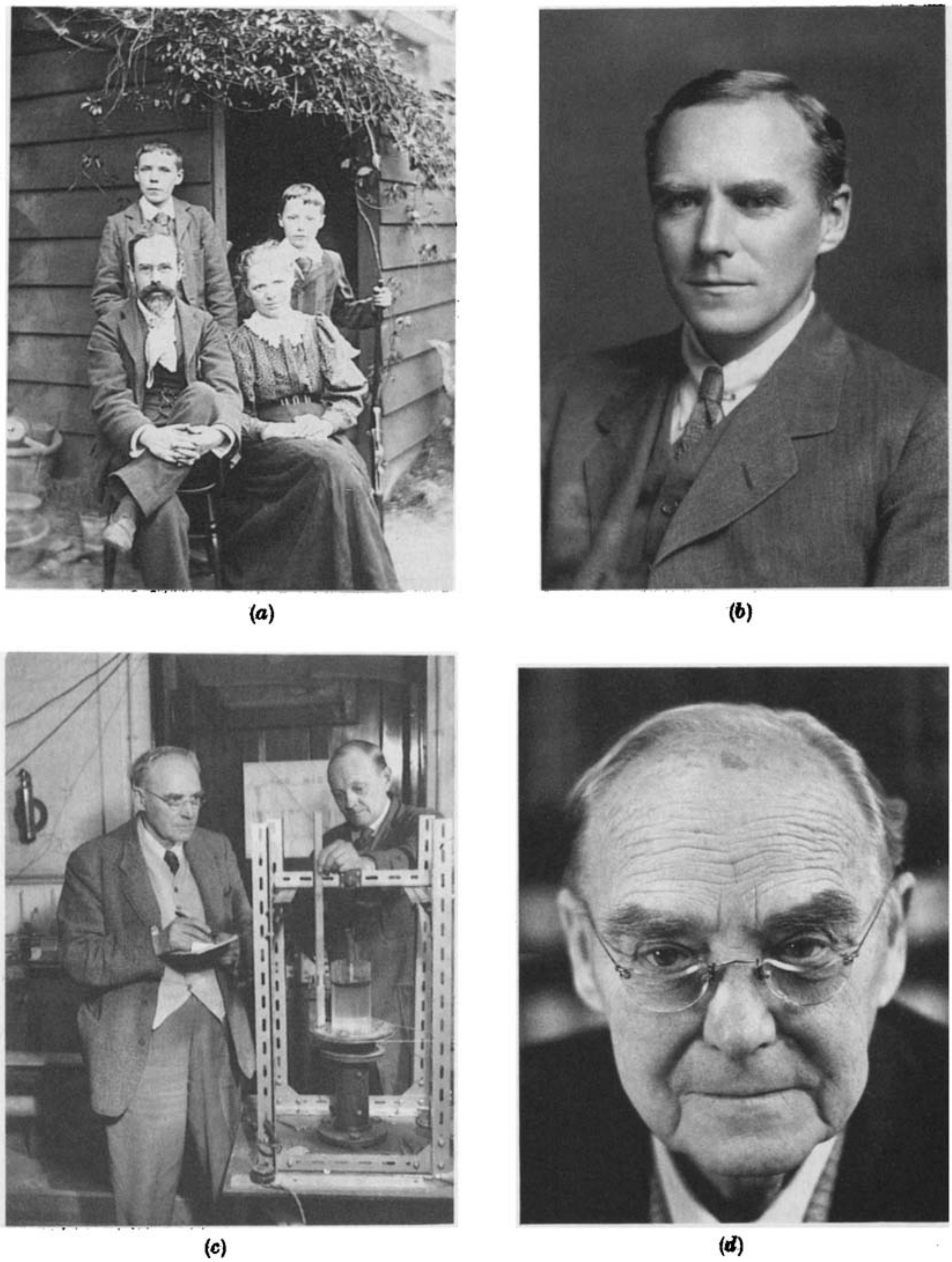


FIGURE 1. (*a*) Edward and Margaret Taylor and their two boys Geoffrey and Julian. Edward Taylor was an artist. (*b*) G. I. Taylor in 1919 (age 33); (*c*) in the Cavendish Laboratory with his technical assistant, Walter Thompson, in 1955 (age 69); and (*d*) in 1973 (age 87).

is conferred by functional simplicity, simplicity of experimental design and simplicity of mathematical argument, both being sufficient, and no more than sufficient, for the purpose in hand; and thirdly, and most important, they exhibit that uncanny knack common to the greatest scientists of recognizing the essential aspects of a phenomenon or a problem that everyone will see later to be significant and of wide applicability. These and some other more personal and less readily recognized features of his research are embraced by the words in the title of the Symposium, 'the spirit of G. I. Taylor'.

Thoughts about what G. I. Taylor did or might have done or how he would approach a problem are in our minds during this centenary year of his birth. Those who knew him personally as well as being familiar with his publications have an advantage here, because the man and his work are inseparable to a quite unusual degree, and knowledge of the one aids the understanding of the other. In this short article I propose to describe some of the formative experiences and events in the early part of his life. The main point I hope to convey is that, from the very beginning, he was a *natural* scientist whose character and activities were perfectly matched, and that freedom from maladjustments and self-concern allowed his creative energy to be used to the fullest possible extent. For those who would like more information about the man and his long and eventful life, I give at the end an annotated list of autobiographical articles by G.I. and a list of memoirs about him.

Geoffrey Taylor was born in London into an interesting and talented family. Most people know that George Boole was his grandfather, but he had several other interesting ancestors. The Boole family tree shown in figure 2 begins with John Boole, George's father, a poor cobbler in Lincoln with a lively mind who made scientific instruments and observed the stars with his own telescope and who introduced George to Latin and Greek and modern languages and natural philosophy. George taught himself mathematics from borrowed books while working as a schoolteacher,† and his later pioneering and very original studies of symbolic logic were similarly carried out in isolation. At the age of 34 he was appointed to a Professorship of Mathematics at Queen's University, Cork, and there he married Mary Everest, niece of George Everest who became Surveyor General of India and one of the founders of geodesy. They had five daughters, one of whom, Alice, was also a self-taught natural mathematician who for her own amusement constructed by Euclidean methods the three-dimensional sections of each of the regular figures which exist in four-dimensional space. Another daughter, Lucy, became a Professor of Chemistry, and the youngest, Ethel Lilian, wrote novels, one of which, *The Gadfly*, is about revolutionary struggles in Italy in the mid-nineteenth century and is recommended reading for millions of schoolchildren in socialist countries.

The razor-sharp quality of G.I.'s mind was thus 'in the family', and in his later years G.I. also liked to see himself as continuing the family tradition of the gifted amateur – by which he meant an untrained person who worked independently and alone and for pleasure – represented by George Everest, George Boole and his daughter Alice. George and Alice Boole had no choice in the matter of the way they worked, but G.I. certainly did, and I am sure that independent work with a minimum of help from others was a deliberate choice and was to his taste. Unlike the typical institutional scientist of today, he (so far as I can determine) never once in his life had a secretary, applied for a research grant, or felt a need to take sabbatical leave. It seems strange that G.I. should think of himself as 'untrained',

† G.I. thought it likely that George Boole acquired his knowledge 'by reading great masters like Lagrange and Newton' (see paper [13] in the bibliography of general articles by Taylor).

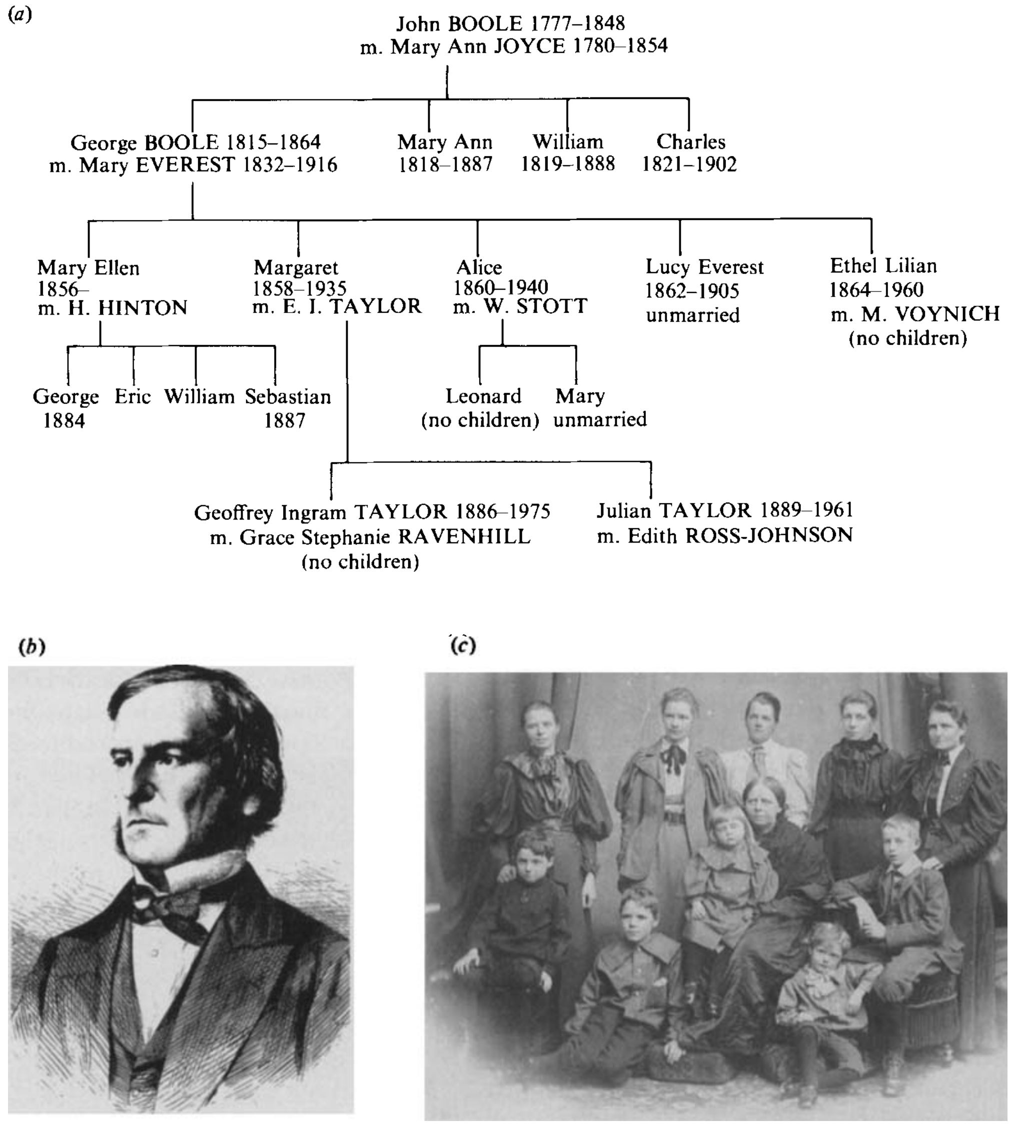


FIGURE 2. (*a*) The Boole family tree; (*b*) George Boole; (*c*) George Boole's wife Mary, née Everest, and her five daughters (from left to right, Margaret Taylor, Ethel Lilian Voynich, Alice Stott, Lucy Boole, Mary Hinton) and five of her grandchildren (Julian Taylor, Geoffrey Taylor, Mary Stott, Leonard Stott, George Hinton), *c*. 1894.

but I believe he had in mind here simply the inevitable tendency for the working tools and techniques of an ageing person to be superseded by new developments. Rather than master tensor notation, electronics or computing, for example, G.I. preferred to do without them and in his sixties and seventies to choose research topics which could be investigated with the tools at his disposal.

G.I. was attracted to science at an early age, apparently as a consequence of his natural ability and the encouragement of schoolmasters rather than any family influence. A few months before his twelfth birthday he went to the Christmas lectures for children at the Royal Institution given in that year by Oliver Lodge on 'The principles of the electric telegraph', and many years later he wrote 'I wish I could again catch the exquisite thrill those lectures gave me. From that time I knew I wanted to be a scientist' (paper [29]). He went to the University of Cambridge in 1905, and studied mathematics for two years and physics for one year. At the end of the third year he obtained first-class honours and a major scholarship awarded by Trinity College which enabled him to stay on for research in the Cavendish Laboratory.

Taylor asked the Cavendish Professor, who at that time was J. J. Thomson, to suggest some possible research projects, and from among them he chose to make a test of the new quantum theory by examining the formation of interference fringes by light waves of very small intensity. J. J. Thomson's idea was that when the intensity is so small that the incident quanta with fixed amounts of energy are far apart the interference pattern seems likely to be modified. G.I.'s simple but effective experiment, which involved exposing a photographic plate for as long as three months in the case of the feeblest source (equivalent to a standard candle at a distance of about a mile), showed conclusively that light behaves as a wave even for the smallest intensity, and this was interpreted as setting an upper limit to the amount of energy in one of the postulated quanta. It is remarkable that G.I. did not get drawn into further involvement† in the exciting new ideas that were transforming physics at the time and that were no doubt being discussed vigorously in the Cavendish. Many years later he wrote 'I did not feel a call to a career in pure physics' (paper [33]). I think it likely that his instinctive feeling of independence made a fashionable field seem less attractive, but it must have required a good deal of self-confidence and courage for him to go his own way.

G.I.'s second published paper – on the structure of a shock wave – arose out of his own reading, perhaps of the note by Rayleigh published in 1908. G.I. established theoretically that a propagating sharp transition layer of permanent type is possible only when the pressure increases across the layer and when diffusion processes operate in the interior of the layer; and he set up the continuum equations for the distributions of velocity, density and temperature in the interior of the layer for a perfect gas with constant viscosity μ and heat conductivity κ, and showed that they could be solved exactly if either $\mu = 0$ or $\kappa = 0$ and approximately if the velocity jump across the layer is relatively small, thereby obtaining the first estimate of the thickness of a shock wave. This was his début in fluid mechanics, the field in which he was to publish over 150 papers during the subsequent 60 years, and it helped to gain him a fellowship at Trinity College in 1910 which provided support and freedom to pursue his research for up to six years.

We all know about the broad fields to which G.I. made massive contributions – turbulent diffusion and dispersion, the dynamical structure of turbulence, dynamical meteorology and oceanography, aerodynamics, the mechanics of explosions, low-Reynolds-number hydrodynamics and others – and it is natural to suppose that he chose deliberately to work in those fields because he saw that they had promise and offered opportunities for contributions of the kind he could make. However, the

† There was another piece of research in physics which is not mentioned by G.I. in any of his later writings and which was apparently not published. Soon after G.I.'s death in 1975 a water-stained incomplete manuscript describing an experimental investigation of the action of crystal rectifiers was found in his garage. The paper records that the work was done in response to a suggestion made by J. J. Thomson 'some months ago', and this provides the only evidence of its date.

evidence suggests otherwise. G.I. himself said that 'the course of my scientific career has been almost entirely directed by external circumstances' (Batchelor 1975) and that he simply reacted to events. One such event in 1911 which had a major influence on his research interests for many subsequent years was the establishment of a temporary post in dynamical meteorology at Cambridge, with money from a wealthy physicist named Schuster who wanted to encourage a more analytical and quantitative study of what was then a rather empirical subject. G.I. was appointed, and chose to investigate an aspect of dynamical meteorology which allowed scope for direct observation, namely the vertical transfer of momentum and heat by turbulent velocity fluctuations in the lowest layers of the atmosphere. He made a number of observations of the mean and fluctuating velocities in the wind at various heights above flat ground with home-made instruments, and began the thinking about the nature of turbulence that was to be a major preoccupation for the next thirty-five years.

His work as Schuster Reader was soon interrupted by another of those external events to which he responded. In 1912 the *Titanic* struck an iceberg and sank with the loss of many lives, and this led the British Government and some shipping companies to equip the old sailing ship *Scotia* to look for icebergs in the North Atlantic and report their positions. Three scientists were included in the expedition, and G.I. was invited to be the meteorologist. He saw it as an excellent opportunity to study the vertical transfer of momentum, heat and water vapour throughout the whole of the friction layer of the atmosphere, and set about designing instruments to be carried by both tethered balloons and kites flown from the masthead of the ship up to heights as large as 2000 metres. The old photographs taken on board the *Scotia* which are reproduced in figure 3 give us some conception of what was involved in this bold and enterprising plan. The expedition spent six months on the Banks of Newfoundland in 1913, and single-handed G.I. obtained a great deal of data which together with records of the wind velocity and air and water temperatures made by ships in the same locality enabled him to make convincing estimates, I believe for the first time, of the turbulent transfer rates. In the famous 1915 paper describing the work he also showed the possibility of representing the transfer rates in terms of eddy diffusivities which varied with height, he introduced the idea of a mixing length (ten years before Prandtl's similar ideas) and used it to relate the eddy viscosity and the eddy conductivity, he derived the spiral variation of wind velocity with height without knowing about Ekman's paper published ten years earlier (and incidentally made a better choice of the boundary conditions at the ground), and he pointed out that Rayleigh's demonstration of stability of inviscid fluid in plane parallel flow without an inflexion point in the velocity profile will not necessarily apply to a fluid of small but non-zero viscosity in the presence of a rigid boundary. It was a tour de force of striking originality which put the study of the dynamics of the lower atmosphere on an entirely new and quantitative basis.

I have referred to this early work on 'eddy motion in the atmosphere' because it shows that G.I. was capable of organizing large-scale experimental resources and did not hesitate to do so when the scientific purpose on hand required that. It would be a mistake to suppose that the 'spirit of G. I. Taylor' is incompatible with or inappropriate for the use of large-scale facilities. It is true that during that long and very fruitful post-World War II period in his sixties and seventies, which is now almost the only period of his life of which we have first-hand knowledge, his lifelong taste for independence led him to choose problems which could be studied experimentally with no resources other than a single technical assistant, a one-room

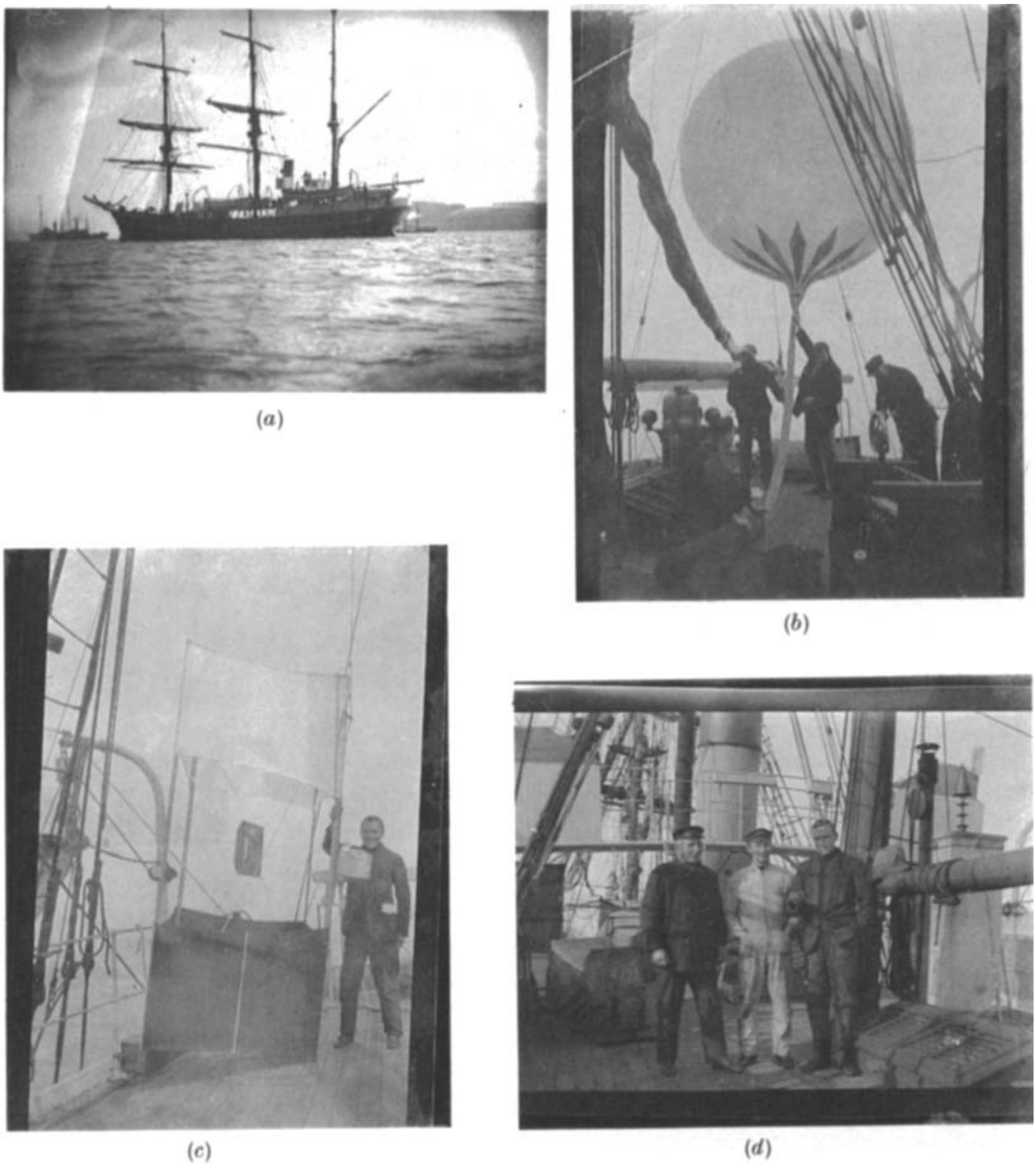


FIGURE 3. The expedition on the *Scotia* in 1913 to observe the paths of icebergs in the North Atlantic following the sinking of the *Titanic*. (*a*) The *Scotia*, an old wooden sailing ship of 230 tons; (*b*) a tethered balloon which carried instruments to measure and record pressure, humidity and temperature up to heights of about 2 km; (*c*) one of the kites flown from the head of the rear mast as an alternative (and, so it was found, better) way of carrying the instruments aloft; (*d*) G.I. and the first mate and second engineer on the aft quarter-deck from which the balloon and kites were launched.

laboratory and simple apparatus. We recognize insight, simplicity and ingenuity in the small-scale experiments of this period, and no doubt the purest examples of these qualities are to be found in these experiments, but they are also displayed in the large-scale projects undertaken by G.I. in his earlier years. One of the people invited to the Symposium, Carl Friehe at the University of California at Irvine, wrote to say that unfortunately he could not attend because he would be involved in setting up a Navy aircraft to measure wind velocity, turbulence and pressure gradient in the lower atmosphere. He added that he had reflected many times on G.I.'s kite-borne measurements from a ship 73 years ago and that 'with all of our modern equipment and its headaches, it is not clear we will add anything more to the understanding of atmospheric turbulence than G.I. did with his incisive experiment'.

The outbreak of World War I was yet another external event which influenced the direction of Taylor's research for many later years. He was recruited by the Royal Aircraft Factory at Farnborough to help put the design and operation of aeroplanes on a rational basis, and thereby was introduced to a new field of activity providing scope for physical adventure, scientific enquiry and inventiveness, a combination which his love of small-boat sailing had already shown him to be ideal. Not being content to question pilots about the way an aeroplane behaved, he learned to fly himself (see figure 4) and, later, to make parachute jumps. Acting as both pilot and experimenter, he made the first measurements of the pressure distribution over a wing in steady flight, and thereby helped to relate wind-tunnel measurements and full-scale performance, no simple matter at a time when the concept of induced drag was not known at Farnborough even though it had recently been formulated by Lanchester.

One of the tasks assigned to him at this time was to analyse the stress distribution in cylindrical propeller shafts under torsion. It was being found that the shafts were seriously weakened by the key-ways cut into them for the transmission of torque. Taylor and a young associate named A. A. Griffith set up a standard experimental procedure for observing the displacement of a soap film which gives the stress concentration at a key-way corner in analogue form and enables the required degree of rounding of the corner to be determined. More important, Taylor was also led to think about the physical processes that limit the strength of solid materials. There was the paradox that attractive intermolecular forces in a material free from cracks suggest a much greater strength than that shown by real materials, whereas a crystalline material in which there are sharp-ended cracks should be unable to resist even a small applied stress. Metals are known to have finite strength which increases as they are deformed plastically, and after a series of definitive experiments carried out in later years Taylor developed the notion of a dislocation, or detached tip of a shear crack, which propagates a certain distance when the applied stress is increased, and was able to account for the observed strength properties of metal crystals.

G.I.'s famous dislocation theory is one of many examples of a fundamental advance arising out of an enquiry with a practical purpose. The fact that these fundamental advances have themselves proved to be very useful for engineering purposes has sometimes led people to describe G.I. as a great applied scientist. It is undoubtedly true that he knew how to provide quick useful solutions to urgent practical problems (like his invention of the CQR anchor, shown in figure 5(*e*), which is a triumph of mechanical and geometrical imagination), and he was immensely helpful to the armed services in the two world wars. However, seeing his work used by others was not an important motivation for him, and research directed towards the solution of a specific practical problem was not to his natural taste. He worked in a quite different way when he was free to do so, as the following quotation indicates. 'The method of scientific thought I personally have pursued has always been to continue along one line till it became too difficult or complicated, or required

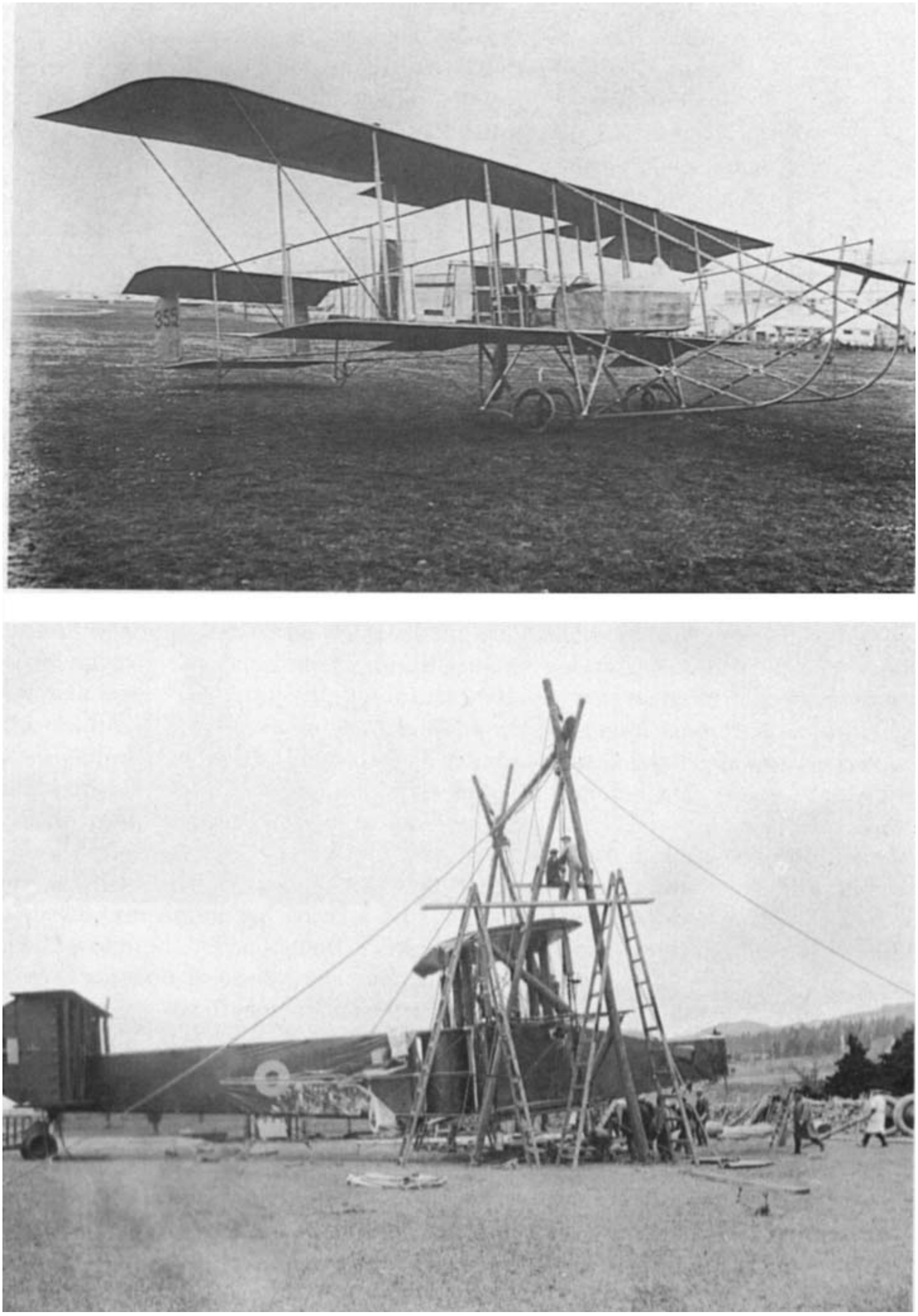
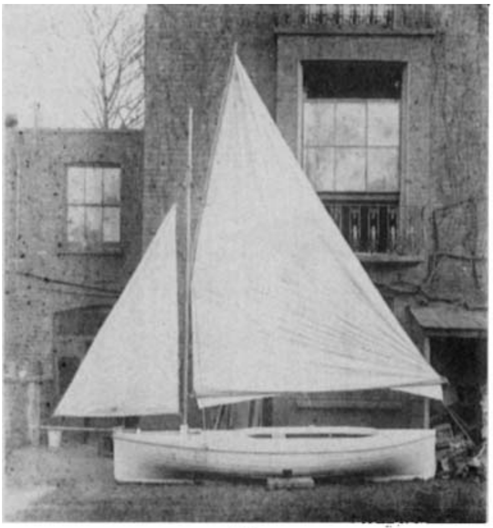


FIGURE 4. While at the Royal Aircraft Factory at Farnborough in 1914–17 G.I. learned to fly on a 1913-model Maurice Farman pusher biplane, like that shown in (*a*), which had a maximum speed of about 60 mph, a landing speed of 42 mph and a stalling speed of 37 mph. Later he wrote 'I am glad to have lived during the time they (aeroplanes) were simple enough for a reasonably intelligent amateur to appreciate them as mechanisms'. In 1919 he acted as meteorological and navigation adviser to the Handley Page group competing for the *Daily Mail* prize to be awarded for the first non-stop flight across the Atlantic. The large Handley Page machine shown in (*b*) was assembled in Newfoundland together with three other competitors, but was not ready to fly before Alcock and Brown succeeded in crossing in a Vickers aircraft.

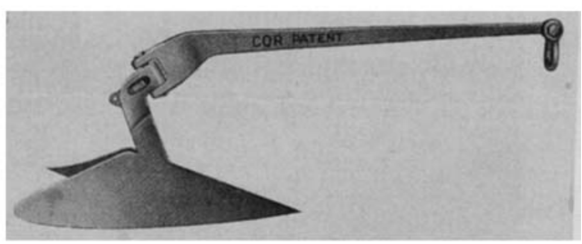


(a)

(b)

(c)

(d)



(e)

FIGURE 5. G. I. was attracted to any activity which provided adventure and a challenge to technical ingenuity; and sailing in small boats was ideal. (*a*) The boat that G.I. built in his bedroom in London as a schoolboy and sailed down the Thames alone; (*b*) G.I. in one of his earliest boats, *Sorata*, sailing in a Norwegian fjord in 1921; (*c*) *Frolic*, in which G.I. made many long cruises in the twenties and thirties, round England, Scotland and Ireland, up the coast of Norway and across the Arctic Circle; (*d*) G.I. and his wife Stephanie leaving the Lofoten Islands in June 1927, in *Frolic*, the other member of the crew being G. McKerrow; (*e*) the CQR anchor, which digs itself into the seabed like a ploughshare regardless of the way it falls, and which G.I. invented in 1933 on learning that the weight of anchors of traditional design with given holding strength presented a problem for seaplanes.

too much effort. Often, however, the pursuit of one line of enquiry raises one to a position from which one can see interesting side lines opening up and I then tend to be diverted down the easier pathways. This is the advantage of being a freelance. It probably limits one's usefulness but it leads to a happy life' (Spalding 1962). It was simple curiosity and the fun of unravelling the workings of nature which guided his research. That his work has proved to be of immense value in engineering is primarily a consequence of the wide applicability of basic knowledge of fluid and solid mechanics.

In 1919 G.I. returned to Cambridge, to take up a teaching fellowship at Trinity College, and in 1923 he was appointed to a Royal Society research professorship which enabled him, I believe to his relief, to devote his whole time to the activity that was so supremely satisfying for him. Again he worked in the Cavendish Laboratory, where he became a close friend of Rutherford who had been appointed, also in 1919, as Cavendish Professor of Physics. In the 20 years between the two wars G.I. was at the height of his powers, and produced a stream of papers on an extraordinarily wide range of topics in fluid and solid mechanics, many of which were referred to in lectures at the Symposium. It would take too much space here to list these topics, or to describe his dramatic and immensely valuable work during World War II on an enormous variety of problems (work which has not yet been adequately documented), or to take note of the astonishingly fertile period after the war when in his sixties and seventies he revealed the scientific gold that exists in tabletop-sized flow phenomena, such as the swimming of very small creatures, flow bounded by porous surfaces, fingering of moving interfaces, waves on thin sheets of moving liquid, and electrohydrodynamics. His investigations of these latter topics, undertaken at an age at which the mind of a normal man is rooted in the past, show well how his open-minded and uninhibited curiosity enabled him to see new interesting problems in unconventional contexts.

I shall mention in conclusion a little piece of work by G.I. which illustrates an important feature of his approach not yet referred to. It was done as a contribution to a discussion organized by the Royal Society in 1954 on 'The first and second viscosities of fluids'. The second coefficient of viscosity, nowadays usually termed the bulk or expansion viscosity, specifies the rate of dissipation of mechanical energy for a given rate of expansion of the fluid. The nature of the microscopic molecular processes that give rise to the familiar shear or deformation viscosity is evident, but those associated with the expansion viscosity are less easily imagined. The record of the discussion is full of erudite contributions from the thermodynamic, rheological and molecular viewpoints which do not convey much understanding of the essential physics. G.I. always found it helpful to have a concrete picture, and so in a four-page note he conceived a fluid medium for which the expansion viscosity could be calculated explicitly, namely an incompressible liquid with shear viscosity containing some well-separated small gas bubbles. The calculation shows one specific way in which a lag in the adjustment of the mechanical pressure to the instantaneous values of the density and internal energy of the medium may arise from the microscopic processes, and there is no longer any need for mystery. I do not think any of the general arguments and conclusions in the record of the discussion are remembered today, but G.I.'s note has given us a simple textbook example of a real medium with a calculable expansion viscosity. We should remember this demonstration of the value of a concrete model when the normal instincts of the mathematician tempt us to prefer general and abstract considerations.

Autobiographical notes and articles by G. I. Taylor

Almost all Taylor's scientific papers have been collected together and published by Cambridge University Press in four volumes:

The Scientific Papers of Sir Geoffrey Ingram Taylor

Volume 1. *Mechanics of Solids*. 1958. 594 pp.
Volume 2. *Meteorology, Oceanography and Turbulent Flow*. 1960. 515 pp.
Volume 3. *Aerodynamics and the Mechanics of Projectiles and Explosions*. 1963. 559 pp.
Volume 4. *Mechanics of Fluids; Miscellaneous Papers*. 1971. 579 pp.

The relatively few scientific works not published in these volumes are listed at the end of volume 4. Some are expository articles prepared for delivery as lectures or for publication in collective works, and some duplicate papers included in these volumes.

No such collection of his semi-scientific articles intended for a broader readership has been made, and his skill and charm as a writer are less well known than his distinction as a scientist. In post-World War II years he was internationally renowned, he received many medals and awards, he was invited to innumerable conferences, and he was frequently asked to address a broad audience and to tell them about his past life or his contributions to some field or his reflections on some general issue. He seldom responded to invitations to express opinions, but he was willing to give factual accounts of his interesting family and his early life and developments in which he had been involved, and judging by the elegance and gentle donnish humour and serenity of the written texts he enjoyed doing so. I believe he had the capacity to be a writer of note, but characteristically he would not have bothered to reminisce on paper unless he had been pressed to do so in his later years. It is fortunate for us that we have these delightful articles, because we learn from them a great deal about G.I. that would otherwise now be completely unknown. In particular we see more of the personality that was so perfectly adapted to creative thinking. A copy of each of the articles listed hereunder in groups according to their subject matter is included in the collection of papers and correspondence of G. I. Taylor held in the library of Trinity College, Cambridge.

The first group of articles are about sailing, which G.I. was passionately fond of throughout his life (see figure 5). This is the only topic on which he wrote voluntarily.

[1] 1920 Navigation notes on a passage from Burnham-on-Crouch to Oban. *Yachting Monthly*.
[2] 1924 Extracts from the log of *Frolic*. *Roy. Cruising Club J.*, pp. 85–105.
[3] 1927 Across the Arctic circle in *Frolic*. *Roy. Cruising Club J.*, pp. 9–26. (*Frolic* was a 19-ton cutter 48 feet long. For his account of this voyage up the coast of Norway to the Lofoten Islands, which G.I. made with his wife and a friend, he was awarded the Royal Cruising Club Cup for 1927. G.I. wrote later that he was 'prouder of this award than any other in my career'.)
[4] 1931 Round Ireland in *Frolic*. *Roy. Cruising Club J.*, pp. 213–225.
[5] 1948 Eiver 1948. *Roy. Cruising Club J.*, pp. 194–200. (The other crew member on this cruise to Brittany was Alan Townsend.)

He was inevitably asked to write obituary notices and memoirs about scientists with whom he had been associated. They are factual and anecdotal, generous in tone, and steer clear of analysis of the subject.

[6] 1935 Sir Horace Lamb, F.R.S. *Nature*, 16 Feb., pp. 255–257.
[7] 1953 William Cecil Dampier 1867–1952. *Obit. Not. Fell. Roy. Soc.* **9**, pp. 55–63.
[8] 1959 R. M. Davies. *Phys. Soc. Yearbook*, p. 15.
[9] 1962 Gilbert Thomas Walker 1868–1958. *Biog. Mem. Fell. Roy. Soc.* **8**, pp. 167–174.
[10] 1963 Memories of Kármán. *J. Fluid Mech.* **16**, pp. 478–480.
[11] 1963 Sir Charles Darwin (1887–1962). *Amer. Phil. Soc. Yearbook*, pp. 135–140.
[12] 1973 Memories of von Kármán. *SIAM Review* **15**, pp. 447–452.

In 1954 the Royal Irish Academy held a meeting in Dublin to celebrate the centenary of the publication of George Boole's famous book *The Laws of Thought*. G.I. was asked to give an account of the life of George Boole, and the published text of his talk makes fascinating reading. (G.I. here demolishes, politely and mildly but convincingly, the class-dominated picture of George Boole's struggles to educate himself presented by E. T. Bell in his *Men of Mathematics*, 1937.) G.I.'s ancestors in the Boole family figure in many of his addresses and general articles other than these three.

[13] 1954 George Boole 1815–1864. *Proc. Roy. Irish Acad.*, pp. 66–73.
[14] 1956 George Boole, F.R.S. 1815–1864. *Notes & Records Roy. Soc. Lond.* **12**, pp. 44–52.
[15] 1964 The life of George Boole. Address at the Boole Centenary Celebrations, Lincoln; typescript only.

G.I. received many invitations to talk or write about scientific developments with which he had been associated, and he accepted a few of them. The published texts are interesting, although one gets the impression that G.I.'s modesty made this kind of writing less congenial to him.

[16] 1945 Trying out the bomb. *The Listener*, 16 Aug.
[17] 1959 The present position in the theory of turbulent diffusion. *Proc. Symp. on Atmospheric Diffusion and Air Pollution*, Academic Press, pp. 101–111.
[18] 1965 Note on the early stages of dislocation theory. *Sorby Centennial Symposium on the History of Metallurgy*, Gordon & Breach, pp. 355–358.
[19] 1970 The interaction between experiment and theory in fluid mechanics. Brit. Hydromech. Res. Assoc.; and *Ann. Rev. Fluid Mech.* **6** (1974), pp. 1–16.
[20] 1970 Some early ideas about turbulence. *J. Fluid Mech.* **41**, pp. 3–11.
[21] 1974 The history of an invention. *Bull. Inst. Math. Applic.* **10**, pp. 367–368. (This note is about the CQR anchor.)

G.I. was attracted to any activity which provided both adventure and scope for his scientific curiosity, and sailing and flying were favourites. His experiences at the Royal Aircraft Factory during World War I evidently made a deep impression on him, and they provided the material for several lectures and articles in later years. The first of the following references was the Wilbur Wright Lecture to the Royal Aeronautical Society in 1921 and is especially interesting, for in it G.I. uncharacteristically airs some opinions and generalities. He discourses for instance on the optimum conditions for research and stresses the importance of freedom for the pure scientist; and in lighter vein he laments the improvements in air transport because that ultimately makes out-of-the-way places less interesting.

[22] 1921 Scientific methods in aeronautics. *Aeronaut. J.* **25**, pp. 474–491.
[23] 1966 When aeronautical science was young. *J. Roy. Aero. Soc.* **70**, pp. 108–113.

[24] 1969 Aeronautics fifty years ago. *Quest* (Jour. of the City Univ.), pp. 12–19.

[25] 1971 Aeronautics before 1919. *Nature* **233**, pp. 527–9; and *Bull. Inst. Math. Applic.* **10** (1974), pp. 363–366.

[26] 1971 Aeronautical experience before 1919. Lester Gardner Lecture at M.I.T.; typescript only.

In the last group are reminiscences which range widely and spread into two or more of the above groups or are short articles on a particular miscellaneous topic perhaps connected with an award or with the gathering to which it was read. These show G.I.'s light literary touch at its best.

[27] 1950 Address to the Accademia dei Lincei on the occasion of the centenary of the birth of Vito Volterra; typescript only.

[28] 1952 Recollections of a scientist. Lecture given at one or more universities in Australia; typescript only.

[29] 1952 A scientist remembers. Hitchcock Lecture at the University of California; typescript only.

[30] 1953 Rheology for mathematicians. Presidential address in *Proc. 2nd Intern. Cong. Rheology*, Butterworths, pp. 1–6.

[31] 1956 An applied mathematician's apology. Address in reply to the presentation of the De Morgan Medal from the London Mathematical Society; typescript only.

[32] 1959 Is there still scope for simple methods in science? Davidson Memorial Lecture at Stevens Institute of Technology, New York; typescript only.

[33] 1963 Scientific diversions. Article in *Man, Science, Learning and Education*, Rice University Semicentennial Publication, pp. 137–148.

[34] 1969 Amateur scientists. *Univ. Michigan Quart. Rev.* **8**, pp. 107–113.

Articles about G. I. Taylor

G. K. Batchelor

1964 Close-up: Sir Geoffrey Taylor. *Trinity Review*.

1975 An unfinished dialogue with G. I. Taylor. *J. Fluid Mech.* **70**, 625–638.

1976 G. I. Taylor as I knew him. *Advances in Appl. Mech.* **16**, 1–8.

1976 Geoffrey Ingram Taylor, 1886–1975. *Biog. Mem. Fell. Roy. Soc.* **22**, 565–633.

T. Griffiths

1972 G. I. Taylor – a profile. *Trinity Review*.

R. V. Southwell

1956 G. I. Taylor: a biographical note. *Surveys in Mechanics*, Cambridge University Press, 1–6.

D. B. Spalding

1962 An interview with Sir Geoffrey Taylor. *The Chartered Mechanical Engineer* **9**, 186–191.