J. Fluid Mech. (1970), *vol.* 41, *part* 1, *pp.* 3–11
Boeing Symposium on Turbulence

Some early ideas about turbulence

By G. I. TAYLOR
University of Cambridge

(Received 1 August 1969)

Apart from the well known pictorial studies of eddies in a stream by Leonardo da Vinci, it seems to me that other engineers were the first to make any progress in the study of turbulence. Except for Osborne Reynolds (1883) they limited their work to empirical formulae giving the results of hydraulic experiments in which they measured mean flow distributions resulting from turbulence. Osborne Reynolds seems to have been the first to show that with given boundaries the flow can be steady up to a certain speed and then suddenly become turbulent. This was a great advance and contributed to the urge by Orr (1907), Rayleigh (1915) and many others to study mathematically the stability of steady flow régimes. Meanwhile the meteorologists were attracted to the field. Arthur Schuster, a wealthy man who was professor of physics at Manchester University, wanted to encourage a mathematician to study meteorology and try to introduce mathematical reasoning into what seemed almost pure empiricism. He set aside provision for a readership in dynamical meteorology. I was appointed in 1911.

Several people had already made theoretical models of the general circulation of the atmosphere, and Cleveland Abbé made a collection of them which was published in 1910 by the Smithsonian Institution. Few of these works connected analytical results with things that could be measured. I, as well as others, recognized that much more heat and momentum was transferred at the earth's surface than could be accounted for by molecular conductivity and viscosity acting on the mean temperature and velocity gradients. Wilhelm Schmidt, an Austrian meteorologist, for instance, wrote about what he called an 'austausch', that is, a transfer coefficient in which masses of air act like large molecules conveying their properties from one layer to another. He does not seem to have attempted to set up a model to connect this austausch in a definable manner with mean rates of turbulent transfer.

One of my first tasks as Schuster Reader was to try to find out how far an analogy between transfer by molecules and by discrete fluid masses can be carried. My first attempt was to see whether turbulent velocities, like those of molecules, tended to be isotropic, or whether, like the conventional idea of sea waves breaking on the shore, they were confined to two dimensions. Sometime in 1912 I made a light wind vane capable of recording vertical as well as horizontal movements. Figure 1 shows some records from a similar but better version made for me by Mr W. H. Dines a few years later. You will see that the record at 8 feet above the ground is much more nearly round than at 2 feet. That at 25 feet showed no bias in the transverse direction, so that so far as vertical and transverse wind

Printed in Great Britain

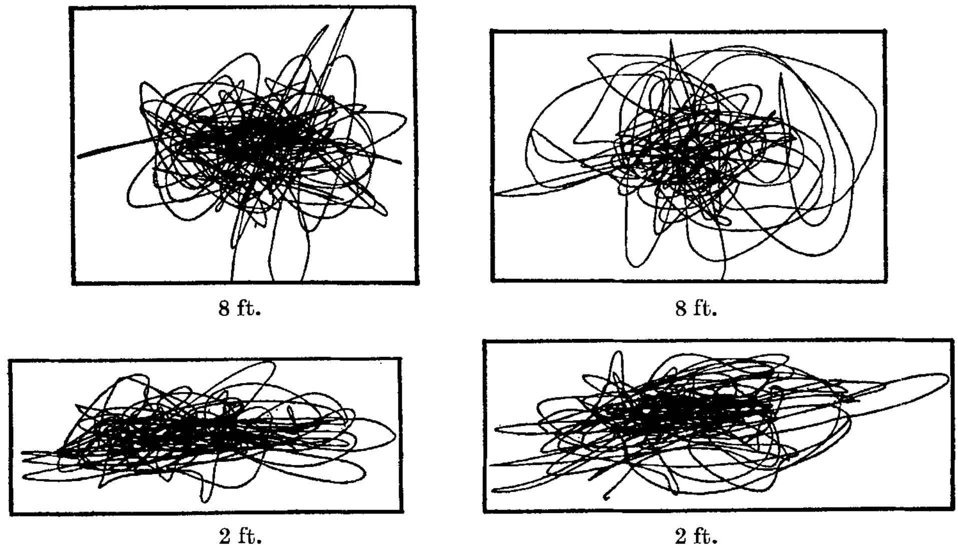

FIGURE 1. Records obtained with double-jointed wind vane.

velocity variations are concerned turbulence seemed to have become isotropic at 25 feet. Then I made a similar investigation for the longitudinal and transverse fluctuations, taking the records of a Dines anemometer and estimating roughly the maximum values of the velocity and direction variations. If the turbulent currents are isotropic,

$$\frac{V_{max} - V_{min}}{V_{max} + V_{min}} = \sin \tfrac{1}{2}\theta,$$

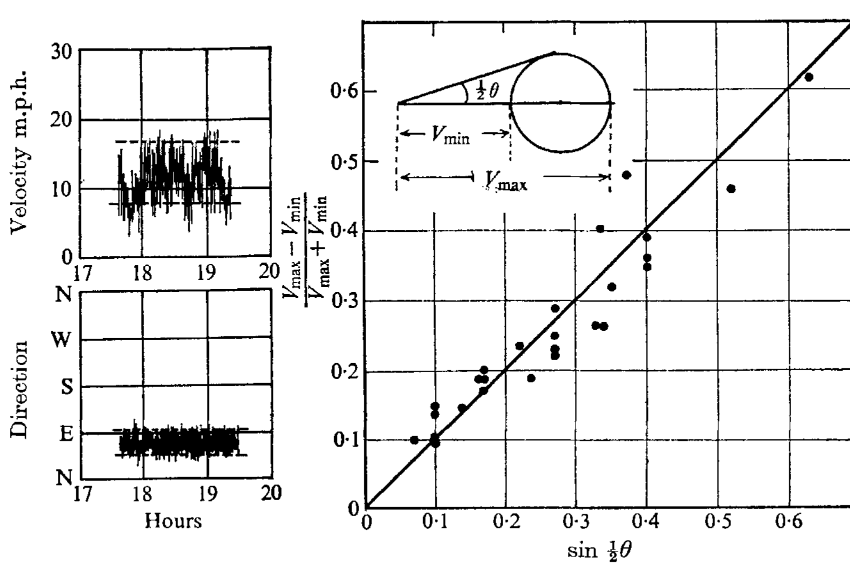

FIGURE 2. Velocity and direction records on Pyestock chimney on 6 July 1917. The limits of variation of records like these are used in the right-hand graph to show that the turbulence is approximately isotropic.

where θ is the range of directions. It was a very rough method but you will see from figure 2 that it did lead to confirmation of this expression.

About this time the passenger ship 'Titanic' was sunk through striking an iceberg and the Government and shipping companies decided to fit out a ship to look for icebergs and report their positions by the recently invented method of wireless telegraphy. I was appointed meteorologist to the expedition and the ship chosen was the old wooden whaling ship *Scotia*.

Here I might perhaps describe the *Scotia* to give an idea of the conditions under which the three scientists of the expedition did their work. She was an old barque-rigged whaling ship, of about 230 tons. She had an auxiliary steam engine which was so inefficient that in spite of the fact that it and its boilers and coal took up about one-third of the space below decks, it would only drive her at about $4\frac{1}{2}$ knots. In a strong wind she could make nearly 9 knots under sail. She had been laid up in Dundee when whaling by the old method of lowering a whale boat, rowing out to the whale, and sticking a harpoon in it, had gone out of fashion. I think she had last been used by Bruce in his 1901 expedition to the Antarctic. She had no bilge keels, so that if nipped in the ice she would have a chance of being squeezed up like a cherry stone pressed between finger and thumb. The consequence was that she rolled very heavily—in fact only two people on board were never seasick, Captain Robertson and Mr J. D. Matthews, our oceanographer. The *Scotia* was built very heavily by shipwrights who, though no doubt excellent craftsmen, seemed to have little idea how to design a ship so as to be a reasonably rigid structure. I had a few books in a shelf above my bunk, and although I jammed them tightly together I never succeeded in preventing them from working loose and cascading on to my head in the night.

On being appointed to the *Scotia* I had decided that the most useful thing I could do would be to study the transfer of heat and momentum and water vapour from the sea. At that time Napier Shaw and Lempfert in their *Life History of Surface Air Currents* had already made a study of the changes in temperature of air masses as they passed over land of varying temperature. I decided to try and use a similar method over the sea, though I realized it might be difficult owing to lack of necessary observations. I would certainly need kites or tethered balloons to carry thermometers and barometers up into the air. Kites for such meteorological work had been used for some time on land and a suitable winch to control the kite wire had been designed for use by Mr C. J. P. Cave. He lent this to me, so I sent it up to Dundee where the *Scotia* was being fitted out and went up to install it.

Here I met my first difficulty. Captain Robertson, who had been captain of the *Scotia* when she was used by Bruce in his Antarctic Expedition in 1901, said that under no circumstances would he go to sea if anyone was going to try to fly kites from his quarter deck. In the 1901 expedition Bruce had tried to fly kites, but his method was like that used by children. A whale boat was lowered and the kite was taken downwind and thrown upwards when the operator on the ship started to pull in the kite wire. The kites always turned over and dived into the sea. This was a consequence, I thought, of the turbulence produced by the immense amount of masts and rigging. Bruce never got kites up but wasted a great deal of time and effort in his attempts. I had been warned of Robertson's objections based on his experience as Bruce's captain, so I made a snatch block into which the kite wire could be passed and the loop between kite and winch hauled to the top of the mizzen mast. To launch the kite it was held by some sailors on the narrow quarter deck and was pulled upwards from their hands when I started the winch. It blew out, clearing obstructions, gyrated wildly as it was pulled upwards through the eddies produced by the ship, and, as I had hoped, settled down as soon as one of its gyrations brought it into the calmer air above the masts. When Robertson looked suspiciously at the winch I was installing in Dundee and asked what it was for, I told him it was for sounding, without mentioning that it was for sounding the air—not the sea. Then on the appointed day for sailing I boarded the *Scotia* in Dundee dock and we went out into the estuary. There we anchored for 24 hours while the crew sobered down sufficiently to work the ship.

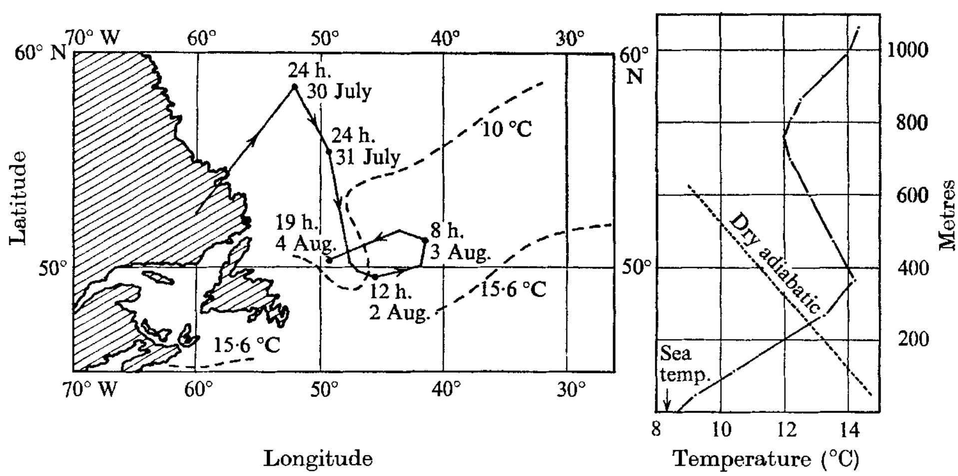

FIGURE 3. The vertical temperature distribution above the sea obtained from a balloon ascent at 19 hours on 4 August 1913. The diagram on the left shows the path of the air mass during the preceding 6 days.

In most of my soundings the temperature rose rapidly with height because the wind had come off the hot land of North America over the cold ice-laden sea of the Grand Banks of Newfoundland, but at heights of 1000 feet or so the rise usually stopped and was followed by a fall at higher levels. Though at the time I could do no more than collect observations, I tried, when I got back, to trace the recent thermal history of the base of the particular piece of the atmosphere which I had sounded. This could only be done even roughly if information was available about the sea temperature at places where the air mass had been at previous times during its approach. At that time a good many ships recorded the surface water and air temperatures and also the wind at bridge level at 4-hourly intervals. Thus if at the time of my kite ascent the wind was, say 10 miles per hour from the south-west, the air which I was sounding had been 40 miles away to the south-west 4 hours previously. To trace it further I had to find a ship near that position at that time which recorded wind and sea temperatures. I had available records for the marine division of the British Meteorological Office, the Deutsch Seewarte and the U.S. maritime authorities but the chance that a recording ship would happen to be near the required positions at the required times was not very great. However, I did manage in a few cases to trace the air paths for several 4-hourly intervals, and by using the sea temperature charts published by the meteorological authorities I was able to trace roughly the thermal history of the air I had sounded and so deduce the rate of heat transfer in the atmosphere.

I got one of my more successful observations at 8 p.m. on 4 August 1913. It was a balloon ascent. The air in this case came off the American continent on 30 July over the cold water flowing southward from Baffin Bay. It crossed over this cold water on to the warm Gulf Stream water about August 2nd, and then turned back to the cold water again. The path of the air mass and the resulting vertical temperature distribution on 4 August are shown in the two parts of figure 3. The double heating and cooling is clearly shown by the temperature distribution. From these results it was possible to deduce a rough numerical value for the effective conductivity due to turbulence.

To complete a theoretical analogy between molecular and turbulent transfer it is necessary to think up some length connected with turbulence which is analogous to the mean free path of molecules. I was driven to imagine a purely hypothetical process to represent the collisions which terminate each molecular free path, and in 1915 I put out the idea that coherent fluid masses move a certain distance up or down vertically carrying all their transferable properties and then mix with the surroundings in which they find themselves. In that paper (Taylor 1915) I also showed that a numerical value for the virtual viscosity can be determined by measurements of the height to which the variation of wind direction from the lines of constant pressure extends. This is the principle of the Ekman layer but unfortunately at that time I had never heard of Ekman who had published his paper 10 years earlier (Ekman 1905).

The other idea, that of a mixture length, was used by Prandtl (1925) who afterwards told me that he had never heard of my 1915 paper. There was however a difference between his conception and mine. He took momentum as a transferable property, i.e. a property which is unchanged in the fluid mass which is transferring momentum, as it might be if the turbulent velocity fluctuation was always at right angles to the mean velocity. I realized that in general momentum is not a transferable property in this sense because air masses are acted on by pressure gradients associated with the turbulence. For this reason I set up a mixture-length theory in which the whole motion, mean as well as turbulent, was two-dimensional, because in that case, though momentum is not transferable, vorticity is.

A mixture-length theory which regards momentum as a transferable property leads to the conclusion that the heat wake from a heated body at constant temperature in an air stream would spread itself in exactly the same form as the momentum wake. In general this is approximately true, but if the spreading wake is nearly two-dimensional, as it is in the Kármán vortex street behind a heated rod, the heat wake differs from the momentum wake in a way that could be calculated by my version (1915) of the mixture-length theory. In fact this was verified 17 years later by Fage & Falkner and described in an appendix to a paper of mine in 1932.

I was not satisfied with the mixture-length theory, because the idea that a fluid mass would go a certain distance unchanged and then deliver up its transferable property, and become identical with the mean condition at that point, is not a realistic picture of a physical process.

While thinking of how a mixture-length theory could be made more definite I considered a theory in which the fluid mass is regarded as a cylindrical mass of fluid which suddenly becomes solid, and moves to another place where it becomes liquid again and mixes with the surrounding fluid. In 1917 I showed that according to classical theory a solid cylinder of the same density as a surrounding fluid which itself is in a state of uniform shearing will move in such a way that it is always moving at the same speed relative to the fluid into which it penetrates. Thus it cannot carry momentum from one layer to another though it can carry heat. This was frustrating, but the analysis applied also to cylinders in rotating fluids and predicted that a solid cylinder of water-density would move straight if towed through water in a rotating field. Experiment showed that this is true (Taylor 1921*a*) and that spheres do not have this property nor do vortex rings. This and the related theory that slow flow in a rotating fluid is two-dimensional which was noticed by me and by Proudman (1916) were just a 'fall out' from an attempt to rationalize a mixture-length theory.

While thinking of these things I became interested in the form which a smoke trail takes after leaving a chimney. Any theory of diffusion which is based on a virtual coefficient of diffusion must predict a mean shape for a smoke plume which is paraboloidal, and it was quite clear to me that near the emitting source the mean outline of a smoke plume is pointed. This led me to think of other ways than mixture-length theory to describe turbulent diffusion. The result was my paper, 'Diffusion by continuous movements', (1921*b*) in which the idea of correlation was introduced into the subject, I think for the first time.

In this problem the correlation was defined in Lagrangian terms, but even then I realized that Eulerian correlations might furnish a useful means for describing a turbulent field. I did not, however, see any way in which it could be used to connect such a field with measurable mean properties, so I did not see any reason for publishing the idea. Some time, about 1924, I believe that the idea was published in Russia but without predictions which could be tested experimentally. It was not until after 14 years, when improvements in hot-wire techniques enabled Dryden at the Bureau of Standards and Simmons at the National Physical Laboratory to measure the decay of turbulence behind regularly spaced grids, that I realized that energy dissipation could be related to the Eulerian definition of correlation. I could only get a simple result, verifiable experimentally, if I assumed that turbulence was isotropic in a statistical sense (Taylor 1935). Then the rate of decay turned out to be

$$15\mu\overline{u^2}\lim_{y\to 0}\frac{1-R(y)}{y^2},$$

where μ is the viscosity, $\overline{u^2}$ the mean value of the square of the fluctuation of velocity in the direction x, and $R(y)$ is the correlation between these variations at points distant y apart in a direction perpendicular to x. The limit of $(1-R)/y^2$ as $y \to 0$ is simply related to the curvature of the $R(y)$ curve at the origin, and can be defined as λ^{-2}. Thus λ is a length which can be obtained directly in two different ways, one from correlation measurements and one from observations of the rate of decay, and these can be compared.

One of the things that can be deduced (Taylor 1935) from the assumption of isotropy is that the correlation $R(r)$ between the u-components of turbulent motion at two points at a fixed small distance r apart will change with the angle θ between the line joining them and the x-direction according to the equation

$$1-R(r) = \frac{r^2}{\lambda^2}\left(\tfrac{1}{2}\cos^2\theta + \sin^2\theta\right).$$

This was verified by mounting two hot wires a short distance apart on a rotatable mounting. This connexion between the correlation R_1 of velocities in the direction of the line joining two points distant r apart and the correlation R_2 between velocities perpendicular to this line was shown by von Kármán (1937) to be a limiting case of the more general relation for isotropic conditions:

$$R_2 = R_1 + \tfrac{1}{2}r\frac{dR_1}{dr}.$$

All these propositions relate either to an existing state of isotropic turbulence or to the rate of decay of turbulence when the instantaneous mean values $\overline{u^2}$ and λ are known. To calculate the rate of decay at all times one must know how λ is going to alter with time, and that has led to far reaching developments in the hands of more competent mathematicians than I so I will leave it for discussion during the symposium.

Now let us turn to the stability of steady motions in an incompressible fluid. This is a question which was brought into prominance by Osborne Reynolds. He showed that motions of Newtonian fluids depend only on the combination *Re* of viscosity, scale, density and velocity which goes under his name. Since viscosity enters in the denominator and velocity in the numerator, and since very slow steady motions do not seem to be unstable when the boundary velocities are fixed, such motions are always either stable for all values of *Re* or become unstable at some critical Reynolds number. Rayleigh and others studied the stability of inviscid fluids and found examples of stable and unstable distributions of velocity. The fact that Reynolds' experiment showed that the flow becomes unsteady at a certain value of *Re*, and later becomes turbulent, led to a great deal of theoretical work to find whether steady flow in a pipe becomes unstable at a critical *Re*. No one seems to have succeeded in that particular problem but in some other cases limited success has been attained by the method of small perturbations. This method, though usually powerful, has to be treated with caution. The Orr–Sommerfeld equation for instance assumes an infinite train of waves superposed on a steady distribution of velocity. To make this a valid conception, body forces which are left out of consideration in the analysis must act on the fluid to maintain the steady state which is disturbed by the waves. Alternatively, the changing steady flow downstream is ignored and Orr–Sommerfeld applied as though the conditions at one point extend unchanged to infinity. For some years after solutions showing waves of increasing amplitude were obtained no one had produced such waves and I was not convinced of the validity of the conclusions from the Orr–Sommerfeld solutions. It was only when Schubauer & Skramsted at the National Bureau of Standards excited the waves and proved that they increase in amplitude that I was converted to believe in their physical existence. Even now the relationship between these waves and the generation of turbulence is not quite clear. There are of course cases where the difficulty alluded to does not arise. The stability of flow under pressure along a parallel-walled channel is one.

It is curious that the Orr–Sommerfeld equation was known for such a long time before solutions of it were found. I think this is mainly because the particular case of stability of uniformly shearing flow between parallel plates was the main target of the approaches of Kelvin (1887), Rayleigh (1915) and Orr (1907), and that particular problem seems to have special difficulties of its own. None of these seemed to make an adequate discussion of the mathematical difficulties which arise in the region where the wave velocity coincides with the particle velocity. Orr (1907) seemed to give up in despair, though he did carry Osborne Reynolds' (1894) theoretical approach to a definite conclusion by calculating the form of disturbance which increases initially at the lowest possible Reynolds number. For the case of pipe flow his result is

$$(\rho D W)/\mu = 180,$$

where D is the diameter of the pipe and W the mean velocity.

In recent years the Eulerian correlations have tended to be abandoned in favour of spectra. The two are however not distinct and the spectral specification is natural to workers with hot wires.

Though the correlation $R(x)$ is defined in relation to simultaneous measurements made at two points separated by distance x it seems to be approximately true that the auto-correlation between measurements at one point at times separated by x/U are a good approximation to $R(x)$. Much work on this has been done at Marseille by M. Favre and his colleagues and contours of equal correlation have been measured.

During the first World War I was much involved with aeronautics, and after it the effect of turbulence in the atmosphere and in wind tunnels on the lift and drag of airfoils was much discussed. It became clear that discrepancies between full-scale measurements and those in wind tunnels are often due to differences in the turbulence of the air streams. For this reason it becomes important to design a wind tunnel which has as little turbulence as possible, and methods for doing this were studied both in this country and in England. With D. C. Macphail, who was a student at Cambridge before and during World War II, I designed and built for the sum of \$1400 a 20×20 in. return-flow tunnel which had a turbulence level $(\overline{u^2})^{\frac{1}{2}}/U$ down to 1/1000. This is the tunnel on which a large part of Townsend's work was done. Simultaneously Eastman Jacobs at Langley Field built one which had an even lower turbulence level. In both, the turbulence was eliminated by screens in the wide part of the channel upstream of the contraction. Much of our work at that time was initiated at discussions in the meetings of the Fluid Motion Panel of the Aeronautical Research Committee—a body with roughly the same duties as the old NACA in this country. One of our projects was to write a book to introduce the ideas on fluid flow which had come up since the last edition of Lamb's *Hydrodynamics*, and Horace Lamb himself, then long retired, agreed to edit it. Members of the Panel undertook to write separate chapters on fields on which they had worked. I wrote the one on turbulence. Lamb died before it was finished and Sydney Goldstein took on the job of editing it and the writing of the introductory chapter. You probably know the book. I think it served a useful purpose and till recently I used to receive small sums yearly as royalty for my chapter.

REFERENCES

CLEVELAND, ABBÉ 1910 Smithsonian Institution, Washington.

EKMAN, V. W. 1905 *Archiv für Mathematik* **2**.

KARMAN, T. VON 1937 *J. Aero Sci.* **4**, 131.

KELVIN, LORD 1887 *Phil. Mag.* **24**, 188.

ORR, W. McF. 1907 *Proc. Roy. Irish Acad.* **27**, 69.

PRANDTL, L. 1925 *Z. Angew. Math. Mech.* **5**, 137.

PROUDMAN, J. 1916 *Proc. Roy. Soc.* A **92**, 408.

RAYLEIGH, LORD 1915 *Phil. Mag.* **30**, 329.

REYNOLDS, O. 1883 *Phil. Trans.* A **175**, 935.

REYNOLDS, O. 1894 *Phil. Trans.* A **186**, 123.

SHAW, W. N. & LEMPFERT, R. G. K. 1906 Life history of surface air currents. *Meteorological Office Memoirs*, no. 174.

SCHMIDT, W. 1925 *Der Massenaustausch in freier Luft*. Hamburg.

TAYLOR, G. I. 1915 *Phil. Trans.* A **215**, 1.

TAYLOR, G. I. 1917 *Proc. Roy. Soc.* A **93**, 99.

TAYLOR, G. I. 1921*a* *Proc. Roy. Soc.* A **100**, 114.

TAYLOR, G. I. 1921*b* *Proc. Lond. Math. Soc.* (2), **20**, 196.

TAYLOR, G. I. 1932 *Proc. Roy. Soc.* A **135**, 685.

TAYLOR, G. I. 1935 *Proc. Roy. Soc.* A **151**, 421.