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An unfinished dialogue with G. I. Taylor

Sir Geoffrey Ingram Taylor, O.M., F.R.S., who died on 27 June 1975 at the age of 89, was one of the great men of our subject. He was a likeable happy man with an uncomplicated character and a razor-sharp mind for which scientific investigation was a natural activity. He was engaged in research throughout the whole of his life – not only the 'working' part of it – and the fruits of his enquiries are described in over 200 papers published between 1909 and 1974. Nearly all these papers have been republished by Cambridge University Press in the four volumes of *G. I. Taylor*: *Scientific Papers*, three of which are on the mechanics of fluids and one on the mechanics of solids. These four volumes are his legacy to us, and will be a store-house of information and a source of illumination for many years to come.

Our current knowledge of the mechanics of fluids and solids owes much to his contributions. Like his contemporary and friend Lord Rutherford, he did not so much ride on the crest of advancing waves as *make* the waves (but unlike Rutherford he would never have said that). Review articles and surveys and books have already shown the fundamental and definitive character of many of his papers. He seemed to have the knack of being first to see things in the right way. For instance, before he wrote his paper on longitudinal dispersion of soluble matter in fluid flowing through a tube, few people had recognized that the differential convection over the cross-section of the tube would cause longitudinal spreading and no one had recognized how that spreading may be described quantitatively; and after seeing his paper, it seems obvious that the longitudinal spreading is asymptotically a diffusion process. It is also characteristic of his papers that, although he put forward this new concept in the context of steady laminar flow in a straight tube of circular cross-section, it is equally applicable to a host of other situations involving the combination of differential longitudinal convection and lateral diffusion. How did he do it? What enabled him to uncover so many nice ideas in our subject and to make so many advances which the rest of us can immediately appreciate? One can answer, as I have written elsewhere, that "his outstanding characteristic is the combination, in one person, of deep physical insight, mathematical ability of high order, and skill in the design and execution of beautifully simple experiments". But there is more to it than that. He had these gifts, certainly, but not everyone with such gifts is able to use them successfully. Taylor was a superbly effective scientist, in whom character and intellect were perfectly matched, and the self-made obstacles to success which beset most of us did not exist for him.

An obituary notice would not be appropriate for the pages of this *Journal*. However, I personally feel impelled to write something about this great scientist and friend whose death impoverishes us all. I lack the literary skill and taste needed for the kind of charming vignette that Taylor himself wrote about Kármán after his death in May 1963 (Memories of Kármán, *J. Fluid Mech.* **16**, 1963, 478), and instead will reproduce here a more ponderous piece of writing compiled jointly by Taylor and myself four years ago. I have long been interested in the sources of inspiration of Taylor's work and the philosophy that underlies it, and several years ago it occurred to me that, since there was little chance of Taylor describing this voluntarily, I might try to extract it by means of a 'dialogue' on paper, my part being to steer the exchange in the required direction by means of questions. Our common friend Milton Van Dyke said he would like to publish such a dialogue in the *Annual Review of Fluid Mechanics*, of which he is an editor. With a little persuasion Taylor acquiesced in the project, without being able to see why it might be found interesting, and so we began, in May 1971. I opened with a question to Taylor by letter, he wrote back his reply, I responded with a further question and a typed copy of the exchange up to that point, and so on. A tape-recorded discussion between us might in some ways have been more illuminating although I doubt it.

The dialogue that follows represents the rather leisurely progress made during the summer of 1971. There were several other topics that I wanted to introduce, but we paused because neither of us was entirely satisfied with the way the exchange was going. I was dissatisfied because I could not get Taylor to expand and 'talk' freely about his views on broad issues; he was confining himself too much to straight answers to the questions. And Taylor, who had been doubtful about the enterprise from the beginning, felt confirmed in his view that this kind of vague discussion did not have enough point. The deadline for publication of the next volume of the *Annual Review of Fluid Mechanics* was approaching, and when Taylor suggested that a better choice for the *Annual Review* would be the text of a lecture that he had given recently on "The interaction between experiment and theory in fluid mechanics" it was my turn to acquiesce. I might have made another attempt later to continue the dialogue, and to turn it into a completed article, but in April 1972 Taylor suffered a severe stroke and further work was not possible.

So here it is, incomplete and different in only minor editorial respects from the form seen and checked by Taylor in August 1971. It will be evident that the idea of introspective discussion is not to Taylor's liking; and that in itself tells us something about the man. Words on paper like these are unfortunately all we have now.

G. K. B. You and I, G. I., have known each other for 26 years, since I came to Cambridge in 1945 as a research student to work on turbulence under your supervision. We have both been in Cambridge during all that time, and have seen a good deal of each other. And I have had the opportunity of seeing the development of your research in fluid mechanics over a much longer period while I was editing the four volumes of your collected papers, the last of which contains papers on various aspects of fluid mechanics written over the period 1910 to 1970. With all this evidence I ought to be able to give a clear and full account of the way in which you work, your approach to new problems, the sources of your ideas, and the factors which have influenced your research. Up to a point I think I could; and I should begin by saying something like what I wrote on the jacket of volume 4 of your collected papers: "He is a happy man who has spent his life doing what he wanted most to do and doing it supremely well." This naturalness of the pattern of your life seems to me to be the key to understanding the way in which you work. But beyond a certain point in such an analysis I remain puzzled and unsure. Perhaps in this dialogue we can fill in some of the gaps and probe in particular into the sources of some of your best ideas in fluid mechanics.

There is one issue which I know I and many other people would like to question you on. It concerns your frequent reference to yourself as an 'amateur' in science. It is well known that you prefer always to work with simple tools and to choose problems which can be tackled by one man working alone, aside from a technical assistant. I do not think that this in itself makes you an amateur. Indeed many of us believe that your ability to get so much from an investigation employing delightfully simple experiments and analysis is the mark of a supreme professional. Could you say what you had in mind when referring to yourself as in some sense an amateur in science?

G. I. T. When I went to Cambridge in 1905 the mathematical teaching was dominated by the requirements of the Mathematics Tripos, Part I, as the examination for the first degree was called. The teaching was done by college tutors and the candidates for Part I were arranged in order of merit in the published examination results. Those who did well in the elementary Part I usually went on to the more advanced Part II, the subjects for which were taught by the University Professors. This system had the result that students concentrated too much on problem-solving and too little on the newer methods of analysis; vectors for instance were hardly used. My own formal mathematical education ended after two years when I took Part I of the Tripos and was placed far down (22nd) among the wranglers, as the first class was called. In 1908 I took Part II in Physics, a course which was almost entirely devoted to the classical physics of continua, electricity, heat, light and sound. Though the mathematical methods I had been taught have proved adequate for many of the physical problems I have studied, they are now regarded as old fashioned and I have not familiarized myself with the modern notations. In that sense I am like an amateur who takes up a subject and works on it without intensive training, using mainly instinctive reasoning. I know that the word amateur is often used in a pejorative sense but I think of it as meaning a person who does something because he wants to, even if he has not been intensively taught how. In that respect I feel I follow at a great distance my grandfather, George Boole, and others like Benjamin Franklin and Ramanujan. I do not really refer to myself as an amateur but only as one who, like an amateur, has not mastered the modern techniques for doing his work.

G.K.B. I have noticed that, like most of us, you tend to go on using the same mathematical tools and notation with which you became familiar in earlier life; for instance, you always prefer to write out the components of a second-order tensor explicitly with co-ordinate symbols as suffixes. This sometimes makes your papers look 'old-fashioned'. And on occasions the use of elementary analytical methods only has even seemed to restrict the research unnecessarily. I remember the striking contrast between the arguments you gave in your 1935 papers to show the relations between the various mean products of velocity gradients in isotropic turbulence and the more general methods devised later by other people. Your special methods, which rely on geometrical ingenuity in making use of the known invariance under different rotations or reflexions, would not be suitable for students, who would get lost in the details, but at the same time I doubt whether a choice between the elementary special methods and the more sophisticated general methods has much bearing on the actual first discovery that such relations existed. You seem, in what you have just said, to be regretting that you have not kept up with the development of modern tools of investigation. But would you really wish to burden yourself with techniques which are mostly used to work out the details of problems after the qualitative features have been made clear? Have there been actual occasions when you have wished to be able to use some advanced technique in your research and when you felt frustrated by ignorance? I get the impression that you see positive advantages in keeping your research methods as simple as possible and in avoiding the use of complex techniques, either mathematical or experimental. To put my speculation in a different form, are you not an amateur – in your sense of the word – by deliberate choice?

G. I. T. I do not try to use modern notations and analytical methods for a variety of reasons. First because the labour of learning them would involve too much time and effort which I would rather employ in thinking about how to give a quantitative discussion of things I can measure in a laboratory. The difficulty I find in modern notations is largely one of memory. I find it difficult to keep in mind the meaning of all the subscripts and superscripts which seem to be necessary when one wants to present a mechanical or physical idea in its most general form. x, y, z come more naturally to me than x_1, x_2, x_3 and still more so than x_i, x_j, x_k and in most of my work I tend to lose the thread of an argument if the gap between the physical conception I have in mind and the symbols used to describe it becomes so large that I cannot readily hop backwards and forwards across it. I have in mind a lecture given by my grandfather George Boole at the age of 19 in which he describes the contrast between the methods of Lagrange and those of Newton in the following words: "By the labours of Lagrange the motions of a disturbed planet are reduced with all their complication and variety to a purely mathematical question. It then ceases to be a physical problem; the disturbed and disturbing planet are alike vanished; the ideas of time and force are at an end; the very elements of the orbit have disappeared, or only exist as arbitrary characters in a mathematical formula. In Newton's investigation this felicitous transformation could not take place. Nature must be combated on her own grounds; the disturbing force is analysed; its effect must be considered in every variety of position – above, below and in coincidence with the ecliptic plane, from syzygy to quadrature and thence again to syzygy, the same influence is followed and the resulting effects determined. The everlasting wheels of the universe are before us and the revolutions can be traced through all the changing varieties of course, circumstance and effect."

Boole's own ideas were very much on the lines of Lagrange, in mine I find the Newtonian methods more congenial though I recognize they limit the generality of the formulae derived. I remember an elderly scientist in the U.S. Naval Laboratory saying to me: "You and I are x, y, z men, all the younger people are i, j, k men." That perhaps is a fair description though it does not explain anything.

You ask if there have been actual occasions when I wished to be able to use some technique in my research and felt frustrated by ignorance. Yes, one was when I was deriving the formulae to which you refer in my 1935 paper. I realized then that if I had been familiar with the rotational transformations of products of vectors I could have put my results in a more acceptable form but I did not want to delay writing up the work while I struggled to learn the necessary groundwork. You must remember that I was working at the same time as Dryden and Simmons and trying to produce a theoretical background to suggest further experiments that they might carry out.

G.K.B. Another characteristic of your research, I think, is your marked preference for concrete propositions; and this may have connexions with the 'amateur' idea which we have been discussing. You obviously like to describe phenomena and processes in terms of what happens in specific cases rather than in terms of abstract ideas. The former evidently gives you more satisfaction than the latter. I am sure that this is what makes your papers so accessible to, and so popular with, practical engineers and applied scientists.

This reminds me of something I have wanted to ask you for many years, and now is an ideal time to do so. When I was going through your very early papers on turbulence in preparation for volume 2 of your collected papers, I could see the gradual evolution of the ideas that culminated in those grand papers in 1935 on the statistical theory. I also thought I perceived some ideas which were expressed in concrete terms and which have later been reformulated in more abstract and general form and seen to be very important. I have in mind particularly those papers you wrote in the period 1915–1918 on turbulent flow in the friction layer of the atmosphere, in which you first draw attention to the problem of accounting for the high rate of dissipation that occurs in turbulent motion of a fluid of small viscosity. Near the end of paper 7 (in volume 2), written in 1917, you wrote: "In order that any appreciable fraction of the energy dissipated in the retarded layer near the ground may be dissipated by means of eddies, it is necessary that the mean value of the vorticity squared shall be enormously greater than the square of the vorticity due to the mean motion. In order that this may be the case the eddy motion must tend to produce small whirls or discontinuities where a very large vorticity occurs in a very small volume. It is only in this way that the effect of viscosity can make itself felt in a fluid of very small viscosity." I think this quotation shows that in 1917 you had conceived the idea of the eddy cascade which enables the energy to be transferred to smaller and smaller eddy sizes until ultimately viscosity has an appreciable effect. In other papers I get the impression that you realized at an early stage that these small eddies produced by the grinding-down process would be more or less isotropic and would have statistical properties with certain universal aspects, irrespective of the large-scale character of the motion. This early recognition of some parts of the universal equilibrium theory of the small-scale components formulated later by Kolmogoroff has not been noticed in the literature, and I am wondering if this is because you stated your ideas and speculations in specific form, in terms of what would happen in actual cases rather than in the form of a hypothesis with general validity.

Can you recall whether the theory that is now associated with Kolmogoroff's name was in fact known to you before 1941, implicitly if not in the form in which he put it forward? And if so, can you remember the development of your ideas about the small-scale components of turbulent flow from 1915 on?

G. I. T. Yes, I did have ideas about the eddy cascade in 1917 but did not see how to express them in mathematical form. The attempt I made to do this starting with a particular form of eddy is described in my paper with A. E. Green in 1937, but it was not successful and I certainly did not get on to the kind of statistical similarity argument which Kolmogoroff, Heisenberg and Onsager developed independently of one another a few years later. As you say, I did realize as far back as 1917 that there must be something that gives small-scale turbulence a statistically isotropic character and that this would be a result of some universal quality in the grinding-down process. I could see that for separations of pairs of points outside the range in which velocity gradients can be regarded as constant there should be a range in which a universal law of grinding down determines the relationship between the correlations and the separation of two points in a turbulent field. However I did not see how to turn this idea into a mathematical description which could form the basis of a theory and could predict things that could be verified or disproved experimentally.

Before 1935 I did not see how statistical definitions of Eulerian correlations could be used for any other purpose than as one element in the mere *kinematic* description of a turbulent field, and I did not want to publish anything until I found something that could be verified experimentally.

G. K. B. I suppose the step which made possible the many useful deductions from the similarity theory by dimensional arguments alone was the recognition that the rate of transfer of energy from one eddy size to the next smaller size is the only relevant physical parameter, aside from the fluid viscosity, in the determination of the structure of the small-scale components of turbulence. Had this idea occurred to you before we first came across Kolmogoroff's work in 1945?

G. I. T. Though in 1937 I had realized the equivalence of the correlation description of turbulence and the spectrum description, my idea of the dynamics was directed to trying to connect the rate of increase of mean-square vorticity with dispersion, because if two neighbouring points on a vortex line are separating the vorticity is increasing, and of course the rate of dissipation of energy is increasing. This idea was expressed in a suitable mathematical form by Karman and Howarth for isotropic turbulence, but it did not lead to the law of grinding down in scale of eddies and I do not think I had any idea that a similarity argument could be used for this purpose till 1945 when you discovered those 1941 papers by Kolmogoroff in a library. You may remember that in 1945 we also heard similar ideas expressed independently, and in very different mathematical form, by Heisenberg and Weizsäcker who were then living under military restraint and who were brought to Cambridge at their request by an officer from the department of military intelligence. Most of the scientific people in this country had been working only on war problems since 1938, and I do not remember that I had thought much about turbulence from that date till you turned up early in 1945. Heisenberg and Weizsäcker had been interned together and had discussed the bearing of hydrodynamical ideas on some other branches of physics but I cannot remember any details. Heisenberg had worked on hydrodynamics – in particular the stability of two-dimensional flow with vorticity – before he took up the work on quantum mechanics for which he is so well known and had retained his interest in it and had read my papers of 1935–8. I remember that we spent some hours in my garden discussing his ideas and my impression is that they were new to me, although I did see the connexion with the theory of Kolmogoroff. Later I heard that similar ideas had been put forward by Onsager. I certainly realized that if a statistically steady state could be established, with large eddies being supplied at a given rate and their energy finally disappearing owing to viscosity, a definite spectrum would result, but I had not thought how this could be expressed as a similarity proposition.

G. K. B. The problem of turbulence has interested you over practically the whole of your scientific life. There are many other large topics in fluid mechanics which you have taken up at some stage, often several years before anyone else has noticed their potentialities. I have the impression that the stimulus that led you to begin research on a new type of problem has usually come from an external source. Some men are led into new fields of research by thoughts generated while reading, perhaps through recognizing an area of ignorance, perhaps through being made curious by the work of another, perhaps simply through noticing a way in which they can exploit some of their existing techniques and knowledge. But I think your starting points have more often been in some external event, frequently a chance and direct contact with a phenomenon which was not adequately understood. For instance, those papers that you wrote on the mechanism of swimming of microscopic organisms – wasn't that work sparked off by Rothschild showing you what bull spermatozoa look like under the microscope?

What would you say were the typical ways in which you were led to take up new problems?

G. I. T. The course of my scientific career has been almost entirely directed by external circumstances. While still at school I came across Lamb's *Hydrodynamics* in my uncle Walter Stott's library and thought I could not understand it I was fascinated by its subject and hoped that I would someday be able to use it in understanding the mechanics of sailing boats, a subject in which I was already much interested from the practical point of view, having built a boat at my home. In 1911 I was appointed to a Readership in Meteorology and this led in 1913 to my appointment as meteorologist of the 'Scotia' expedition in the North Atlantic. Here I measured the temperature and wind distribution above the sea, using kites. This led me to speculate about the way in which heat is communicated from the sea to the atmosphere and hence to develop a turbulent transport theory. The idea of a mixture length had I think just been introduced into a paper of mine in 1915. Some years later it was introduced in a rather different context by Prandtl who afterwards told me he had never heard of my 1915 paper.

At the outbreak of the first world war I went to the Royal Aircraft Factory at Farnborough and was employed in many activities concerned with aeronautics. One of the earliest was to design the form of the keyway slots in engine shafts where torque was transmitted. A young man called A. A. Griffith came to work with me and as a result of many discussions he developed the theory of the Griffith crack to account for the fact that metals are much weaker than consideration of atomic forces would lead one to expect. The fact that though metals may be less strong than expected they get stronger in plastic strain was not in agreement with Griffith's theory and this awkward fact simmered in my mind for twenty years till I realized that a physical idea I had had from the first could be expressed mathematically using as a model Volterra's elastic dislocations.

In hydrodynamics my war activities made me aware of the fact that Lamb's classical treatment was not applicable to aeronautical problems. When the war was over I went back to Cambridge as a mathematics lecturer and I became a member of the Aeronautical Research Committee. Here I became acquainted with Joukowsky's work and Prandtl's boundary-layer theory and these seemed to hold a promise of making an acceptable description of aerodynamic forces. On the other hand it seemed to me that in cases where fluid motions are due only to pressure variations and tangential reactions at solid surfaces are not important, the classical treatment developed in Lamb's work ought to yield experimentally verifiable results. Hence my work on rotating fluids.

G. K. B. The first world war obviously had a big effect on the course of your scientific work. I know that during the second world war you also did a lot of research for different government departments and military agencies, and that some of this work led after the war to papers on related fundamental problems. You seem to find it stimulating to be brought up against practical problems and applications, in war needs, or industry, or yachting. And conversely, your pure science is never far from the practical application. What do you think of this distinction between pure and applied science? Does it have any significance for you?

G. I. T. I have never taken much interest in what appears to me as mathematics which has nothing to do with any tangible objects. This is probably because I have always been slow in that kind of thinking. I am a bad chess player and I am always the last to fill up a cross-word puzzle for instance. I remember Dick Southwell reported hearing me say during a journey we made together in a train "rodent, 3 letters, first letter R, last letter T – now don't tell me". The only one of my scientific papers which could properly be described as belonging to the category 'pure science' as distinct from 'applied science' has the title "A relation between Bertrand's and Kelvin's theorems on impulses". This came to my mind while teaching elementary mechanics.

To most engineers the division between pure and applied science occurs at a very different level from that at which most mathematicians would place it. I suppose most of my work would seem to be on one side of it to one and the opposite side to the other. That is why I am able to excuse ignorance of something I ought to know about by telling a mathematician that I am an engineer and vice versa.

I know of course that mathematical discoveries which appear to have no application when they first appear sometimes become of great importance later. Boole's ideas on using symbols for mathematical operations as though they were algebraic quantities was rediscovered by Oliver Heaviside, an electrical engineer, many years later, and his ideas about symbolic logic seem to have been fundamental in computer science. If Boole had not been my grandfather, I don't suppose I would have taken any interest in his work before it was found useful in applied science.

G. K. B. You may be right in supposing that 'pure' and 'applied' are mainly relative terms. Most of us probably like to see our own work as being neither so pure as to have no apparent application nor so applied as to be devoid of fundamental interest. But I think your work can truthfully be said to combine the best aspects of pure and applied science.

I can recognize your approach and attitude to the more fundamental problems, but the nature of your thinking about the applied aspects is less clear to me. You seem somehow to have enjoyed hearing about engineering needs of various kinds and to have given considerable help to engineers in the development of new devices, while at the same time remaining essentially uninvolved. Most scientists enjoy the challenge presented by first-hand contact with some development problem in technology or evidence of an apparently new phenomenon arising in some applied context, but it is difficult to regulate the demands made on one's time by this contact. I get the impression that most university scientists in U.K. have either too little or too much involvement with applications of their field of research, and this does not surprise me. One needs to invest quite a bit of time in consulting and advisory work in order to be able to make a useful contribution, more I think than most university people are willing to spare. You seem to have the knack of going deeply enough into problems arising in industry or government laboratories to be able to extract the interesting features, without becoming immersed in the detailed technical aspects. This may be simply one aspect of the gift for economy of effort in general which you undoubtedly have. Do you make conscious decisions about the optimum stage at which to withdraw from engineering exercises in which you have become involved? It is alleged that one of our more theoretical colleagues at Cambridge was invited to advise on some practical problem in industry, and was given a lengthy tour of the factory and explanation of the problem at hand; he remained silent throughout, and when at the end of the day his hosts pressed the great man to tell them what he thought, he said, "I am glad it is your problem and not mine." I do not mean *that* degree of detachment! How much contact and involvement do you think is scientifically profitable?

G. I. T. When I have been consulted on engineering problems I have usually tried to invent a simple model which can be described mathematically and works on what I believe to be the mode of action of the machine concerned. If this conceptual design is materialized in a laboratory one may learn a lot about the original machine by watching the performance of the model. If the model does not perform as calculation indicates that it should, one may be more able to trace the cause of faults in the original machine by experimenting with the model than with the original. The model, for instance, may suffer from some kind of instability which was not included in the calculation but can be understood owing to the simplicity of the model, though it might be difficult to pick out by observing the performance of the original. An example of this is provided by my involvement with the paper-making industry. The draining of water from a pulp of cellulose fibres in a Fourdrinier paper-making machine is mainly accomplished by the suction which occurs on the downstream side of the rollers which support the moving wire gauze carrying the paper pulp. I made a simple calculation relating the amount of fluid removed at each roller to the physical properties of the pulp, but this simple steady-motion model involved a downward acceleration of the upper surface of the top of the pulp layer which can be greater than g when the machine is going fast enough. Independent experiments on the stability of free surfaces accelerating downwards can reveal the modes of instability which can occur.

I think that in general engineers feel, quite rightly, that it is their job to make use of any new facts or principles that may come out of consultation of this kind and I do not feel obliged to pursue the matter further unless I am called on again. In one case however I was involved completely, namely in the invention and development of the C.Q.R. anchor, but then I was a user as well as the inventor and naturally followed the thing through completely, including the trying out of modifications, testing in different grounds, making the design drawings needed by manufacturers, and marketing. The last two were in the hands of friends but all three of us knew about, and discussed, everything that was going on.

As you say I do tend to drop out of engineering exercises at a fairly early stage. This is because I am much better at thinking of simple things than in holding more complex situations in my mind and for this reason it is economy of effort as well as personal preference that makes me withdraw before detailed considerations become paramount. In that sense it is a conscious decision to drop out, but sometimes work I have done at the early stages of a project suggests ideas in other fields which I feel more able to follow up than I do to go on with the work which inspired them.

G. K. B. What do you think about the ethical questions which arise in the case of work which leads fairly directly to application? Do you think a scientist has an obligation to consider the social implications of a possible application of his research?

G. I. T. I do not regard scientists in general as any more capable of foreseeing the ultimate results of their work than other people. There are however obvious exceptions. The scientists working on the Manhattan Project at Los Alamos knew that if they succeeded in producing an atomic bomb its destructive effect would be so great that it would kill everyone within a large area and most of us hoped that it would not be used on a city, but we realised that we would have no voice in determining how it would be used. We believed that the Germans were engaged in similar activities and that unless we produced the bomb first we would be at their mercy if they succeeded. We felt that this was a complete justification for the project, but when Germany collapsed without producing a bomb some of us thought about the possible military use against Japan if we were successful. However till the first successful nuclear explosion in July 1945 we did not know whether the bomb would explode, for at that time we only had calculations based on an optimistic hope that nothing in a long series of operations would fail to work and that our calculations were realistic. I think it was this uncertainty which filled our minds and excluded thoughts about what might happen if the bomb actually went off and was delivered into the hands of the government. Attempts were made by some scientists during the short time between the first explosion in New Mexico and the second in Hiroshima to persuade Mr Truman and his military advisers not to use the bomb on a civilian population but they proved unavailing.

I only stayed in Los Alamos for a short time after the first nuclear explosion in New Mexico and did not play any part in the organisation of this appeal. Had I known of it at the time, and taken part in the discussions which led, after my return to England, to the appeal, I would certainly have joined in it. The newspaper report of the attack on Hiroshima was the first intimation I had that the bomb had been used and in a BBC radio broadcast the next day C. G. Darwin described the history of the British contribution to the project and I gave an eyewitness account of the explosion in New Mexico. Neither of us made any comment on the military use of the bomb but confined ourselves to the New Mexico test and the events which led up to it.

Apart from my connexion with the Manhattan Project I do not think that any of my own work has been likely to have any deleterious effect on other people. It has been mostly concerned with analysing quantitatively phenomena which are understood qualitatively.

G. K. B. Turning to a rather different topic, what do you think about the current trends in research in fluid mechanics? There are some areas which have gone ahead rapidly in the last few years – rotating fluid systems is an obvious example – and others, such as turbulence, which have been relatively stationary, either because of declining interest or for lack of fruitful ideas. The clever research worker anticipates these broad movements and gets in – or out – at an early stage. Can you see areas of fluid mechanics which at this moment seem to be ripe for big advances and which a young man would be well advised to take up?

G. I. T. I doubt if I can answer that question. If one could foresee new developments one would probably have gone after them oneself and got stuck. The question might perhaps be put more specifically "In what directions have you set out and found progress barred by lack of skills or facilities which others may possess?" I could mention one or two, but in most cases do not see how the difficulties which stopped my progress can be surmounted. The first is the problem A. E. Green and I attempted in 1937 (*Proc. Roy. Soc.* A **158**, 499–521) of trying to trace the history of a given assigned simple initial fluid motion till (hopefully) it has the character of turbulence. If this could be done even in a non-viscous fluid, it would be of great interest. Several people seem to have tried to go beyond the first steps which Green and I took but in spite of the power of modern computers they do not seem to have gone much beyond the stage reached by us without such aids. Green and I defined the initial motion by velocity components u, v, w in the form

$$u = A\cos ax \sin by \sin cz, \quad v = B \sin ax \cos by \sin cz,$$
$$w = C \sin ax \sin by \cos cz$$

with the incompressibility condition $Aa+Bb+Cc = 0$. We attempted to trace the generation of higher harmonics in Fourier series expansions of u, v, w, in powers of t, the time since the initial simple harmonic was released. It may well be that a computer could produce the Navier–Stokes solutions giving the changes with time in u, v, w at fixed points without computing the spectrum. It would be very interesting to see whether a computer starting with a simple initial motion would develop the characteristics of the Kolmogoroff statistical representation of the grinding-down process.

Another difficult problem which would be worth considerable effort if a way could be found into it is to construct a model of flow through a porous solid. The Stokes flow past a grid of parallel cylinders and various regular arrangements of spheres have been calculated, but none of these are realistic models because in a porous body the individual components, fibres or granules, support one another. The Stokes flow past isolated finite cylinders has been calculated but when they are arranged, say, along the edges of a cubical lattice no one has yet surmounted the difficulty of calculating the flow near the junction points. If such a theoretical model could be constructed it might be possible to calculate the longitudinal and the lateral dispersion of contaminants as well as the hydraulic resistance.

The flow of fluids through a porous medium when the voids are not filled is an important part of the study of ground water flow and it seems to me that work on the dispersion of contaminants in such flows might have important applications in environmental studies. In problems concerning water seeping through a dam the top of the water is usually taken as a surface of constant pressure and the interstices above that surface are considered as empty. What kind of boundary condition would apply if the pores were only partially filled above some saturation surface in the porous medium? Questions of this kind have been studied by E. C. Childs and his colleagues in Cambridge and by J. R. Philip in Australia but I think much remains to be done in this field. Lateral dispersion of contaminants in flow through porous media cannot be studied as a two-dimensional problem because since streamlines cannot cross one another in two dimensions contaminating particles must come out of the medium in the same order as they entered it.

One of the most intriguing problems, which may be partly mechanical, is to understand why a tree grows upwards. Apart from the question of why it grows at all, the nature of the control which makes it grow in the direction of maximum acceleration does not seem to be understood. I understand that plants grown on a steadily rotating table are no more capable of separating the effects of gravity from those of other accelerations than we are and so grow along the lines of maximum acceleration.

G. K. B. The problems that you have mentioned are clearly important ones, but they are more particular and specific than the broad areas that I had in mind. My question was concerned more with what might be called the strategy of research in fluid mechanics and with the shifts in emphasis which occur, sometimes for scientific reasons and sometimes for social and political reasons. I think that at various times during your life you have made forecasts of broad areas which are likely to be fruitful, or at any rate attractive to you personally. For instance, I recall that you made a conscious decision to take up aerodynamics and meterology, when you were first beginning research, rather than the then new and exciting field of atomic physics which J. J. Thompson as Cavendish Professor was pursuing so successfully. Supposing that a young man who has just got his Ph.D. came to you, in 1971, and said that he would like to choose a fruitful area of fluid mechanics and study it for a period of several years at least, and that he would like your advice on the best field to choose. Would you be able to offer him any guidance?

G. I. T. I do not think that the problems I mentioned can be described as particular, except perhaps the one concerned with the development of a sinusoidal velocity field and even that is interesting only because no other particular situations have been suggested in which a definable initial motion might assume the character of a turbulent field. The amplification of finite disturbances discussed by J. T. Stuart is another, but there the emphasis is on the effect of mean flow on the disturbance, as in pipe flow, rather than on the process by which the scale of eddy size continually decreases.

I do not think that flow in porous media is a particular problem, in fact the theoretical discussion of this subject seems to me to be impeded by a lack of any definable model for which the dispersive property of flow in porous materials can be calculated. This subject has assumed great importance recently when people's minds have been directed to the process by which chemical effluents are dispersed in the ground.

I do not remember making any forecasts of broad areas of study which have proved fruitful, but I have gone along paths which are attractive to me personally. All my work, like that of most of us, has been concerned with particular problems. Some of these may point the way to a new range of particular problems, but I do not see how one can plan a "strategy of research in fluid mechanics" otherwise than by thinking of particular problems. As you say, one may be directed along a particular line by social and political considerations but it seems to me that it is by attention to specific problems rather than by generalized reasoning that advances are made in our subject. I realize that by developing methods of analysis which have more general application than to the particular problems which give rise to them one may facilitate the solution of further problems, but in general it seems to me it is through particular problems which can be subjected to experimental verification or compared with natural phenomena that most advances are made.

The only general forecasts to which I seem to have committed myself on paper were ones which I included in the Wilbur Wright lecture to the Royal Aeronautical Society in 1921. I expressed the opinion that increases in the speed and comfort of travel were a bad thing for society because they reduce the size of the world without increasing our understanding of the people in it. They tend to make the place you reach at the end of your journey exactly like the place from which you started. I also alluded to the harmful effects of over-population. At that time people in authority in England were deploring a decrease in the rate of increase in population, and the Reverend T. Malthus was regarded by many as a sort of antichrist or pornographer whose works respectable people did not care to be seen reading.

G. K. Batchelor