Research as a life style

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INTRODUCTION TO THE AUTHOR

George K Batchelor is well known to the fluid dynamics community. His two books *The Theory of Homogeneous Turbulence*, first published in 1953, and *An Introduction to Fluid Mechanics*, 1967, are widely used around the world. Professor Batchelor's research in fluid mechanics is broad, covering topics as diverse as turbulence theory and colloidal flows, and is always of the highest quality. His writing is concise yet lucid and accessible, and his ideas are often breakthroughs.

He was born in Melbourne, Australia in 1920, and studied mathematics and physics at the University of Melbourne. In 1945, he went to England to do research on turbulence under the supervision of GI Taylor, and received the PhD degree in 1947 from Cambridge University. Staying on at Cambridge, he became Professor and Head of the Department of Applied Mathematics and Theoretical Physics, a position he held for 24 years until his retirement in 1983. His outstanding research has resulted in numerous honors for him, including being elected Fellow of the Royal Society at an early age (in 1957), and membership in the national academies of science in the US, Sweden, Poland, France, and Australia; honorary doctorates from several universities; and receiving the Timoshenko Medal of the ASME (1988), and this year, the GI Taylor Medal of the Society of Engineering Science.

But perhaps Batchelor's greatest achievement is as the founder and first editor of the *Journal of Fluid Mechanics*. Since its inception in 1956, this journal has set the pace for scientific publications in fluid mechanics. It is difficult to commence as a distinguished journal in a crowded field, and even more difficult to keep it up for 40 years, but Professor Batchelor did it single-handedly. The readers of *Applied Mechanics Reviews* are indeed very fortunate to have Professor Batchelor share his words of wisdom in this Retrospective.

Mohamed Gad-el-Hak and Arthur W Leissa

Thinking is one of the greatest joys of humankind
(from Galileo according to Brecht)

Introduction and Preface

World War II drew me into fluid mechanics, and that has remained my main field of research for just over 50 years. Now that my creative years are near their end it is an appropriate time for some reflections on what I have learnt from this research experience. During those 50 years I mixed with and got to know well many scientists and engineers actively engaged in research. It seemed to me that they had a characteristic set of values and principles and preoccupations–in short, a life style–which is worthy of study. This life style of research workers interests me, and I propose to write here primarily about the doing of research rather than about the research itself. My purpose is like that of the writer HG Wells, of whom it was said he relished the unfamiliar perspectives that science opened on the human condition.

Scientists and engineers engaged in intensive research lead uncommon and, many would say, strange lives. Their objectives are vague and uncertain; success is difficult to evaluate; the pursuit of elusive new ideas is both frustrating and stimulating; and making progress does not mean there is less to do in the future. I propose to take a look at these features of a life in research, and to ask what we are doing and why we are doing it, using my own years of research in fluid mechanics as a source of examples and illustrations. For examples of a life of research at the highest level, I shall draw on the life of GI Taylor, the most original and creative scientist known to me.

Inspiration and communication of research are two important issues for consideration, especially the latter. What proportion of all the published papers on fluid and solid mechanics are read and understood? Contrary to popular opinion, it is not usually the equations which need to be understood for the effective communication of science, it is the words. Literacy is important, as well as being the door to the world of literature; why is it not taught to graduate students?

We should also take note of the remarkable internationalism of science and its consequences. There is a degree of trust among scientists, especially in the context of communication, which probably exceeds that among participants in any other international field of human activity.

Honesty compels us to recognize that there may be a price to pay for the intellectual rewards of research, namely the distancing of a scientist with a problem in his head from his nearest and dearest. These rewards may become so attractive to scientists that they become obsessed with the pursuit of success. In our survey of group characteristics, we should not overlook vanity and competitiveness and the familiar weaknesses of men who set themselves an overriding goal.

The kind of research I am writing about is of course curiosity-motivated research as usually carried out by individuals. Nowadays, much research is teamwork, carried out by groups with experimental or computational facilities. Curiosity-motivated research appears to be less common than it was a few decades ago, possibly because the remorseless financial pressure from sponsors drives groups of research workers into fashionable areas with promise of early industrial application. However, wise governments recognize that the material contribution to society resulting from new basic knowledge gained by gifted individuals may also be of value. This is essentially what most university research workers get paid for, and naturally we hope we are earning our pay. However, I have to say *may be of value* because it is difficult in practice to see the connections between the results of curiosity-motivated research and the products used in society.

As a final remark in my rapid introduction to a large topic, I note that most of us derive great satisfaction from successful curiosity-motivated research, with any tangible personal rewards being a bonus. This satisfaction is like that of any creative artist who struggles to give expression to his vision and for whom the main driving force is the work itself. As the Cambridge physicist Nevill Mott put it in a moment of candour, 'I never did think the world needed more science, but research grabs you.'

Getting started in research

Although this is not primarily an autobiographical article, I propose to describe briefly my own introduction to research so that a reader will be aware of the basis of my views. A friendly critic of a draft of this article said he found my view of research a little *rhapsodic*; I admit the charge, and regard my happy experience as responsible. As an undergraduate at the University of Melbourne, I studied mathematics and physics and enjoyed doing so. I had only a vague notion of what was meant by *doing research,* but I was certain I wanted to learn how to describe physical processes in mathematical terms and to use that knowledge to make discoveries. After graduating in February 1940, I therefore went to see one of the professors in the Department of Physics to seek his advice on what I should do. I was attracted to what was then called *modern physics*, no doubt *because* it was described as *modern*, and I asked him whether nuclear physics would be a suitable subject for my proposed research. My advisor said that nuclear physics was likely to be a neglected subject for the duration of the war, and he advised against it. As things turned out, he was wrong, but what he said was a reasonable conclusion from the public knowledge of that time.

However, he also made the constructive suggestion that I should visit the Aeronautical Research Laboratory, which had recently been set up in Melbourne to provide a research and development service for Australia's embryonic aircraft production industry. I did so, and found that one of the staff there (Gordon Patterson, later Director of the Institute for Aerospace Studies at Toronto) would be happy to supervise the work of an MSc student on aerodynamic problems, the first being the calculation of the corrections needed to allow for the effect of the walls of a wind-tunnel of octagonal cross-section on the measured lift and drag forces on a model. I took up this problem and got an MSc for some approximate irrotational-flow theory which nowadays would be supplemented by simple computations. More importantly, since my intended plan of going to England to work for a PhD had to be postponed during the war,[1] I joined the staff of the Aeronautical Research Laboratory. The practical aerodynamic problems that were assigned to me there led to consultation of books about wing and aerofoil theory, boundary layers, shock waves, turbulence, and other areas of fluid mechanics, all quite novel and interesting to me. Gradually, the idea of undertaking research in fluid mechanics after the war took root in my mind, and since the aspect about which least seemed to be known was turbulence I decided that would be my PhD topic.

GI Taylor at Cambridge was the acknowledged British authority on turbulence, and so I wrote to ask if he would be willing to supervise my research when the war ended. He agreed, and thereby made me a very happy man. One of my colleagues at the Aeronautical Research Laboratory, Alan Townsend, also intended to go to Cambridge when the war ended in order to complete his graduate studies, which initially were in nuclear physics, and since I knew him to be a first-rate physicist and an electronics wizard I suggested that he too should work on turbulence under GI Taylor and that we should collaborate. He said he would be glad to do so, although he wanted first to ask two questions: one was *What is turbulence?*, and the other was *Who is GI Taylor?* My answers were evidently satisfactory, for the outcome for both of us was a marvelous decade of turbulence research which began in 1945. Those were exciting days in which there seemed to be many interesting and useful things to do on turbulence, both experimentally and theoretically. Contrary to what I had expected, GI Taylor himself was not engaged in research on turbulence because he wished to explore further a number of interesting topics associated with practical problems arising out of war-time defense needs. While always being willing to hear what we were doing on turbulence, he allowed us to go our own way. This suited Townsend and me very well, and I believe we positively enjoyed and benefited from the need to be independent.

The end of the war in 1945 was only the beginning of the gradual disappearance of material shortages in Britain. There was rationing of food, clothing and fuel, and houses were decaying, but Cambridge was my scientific Mecca and it was pure bliss for me to be there. There was magic in the old courts of Trinity College where Newton had lived and worked and in the Cavendish Laboratory where Maxwell,

[1] I should explain here that there was no PhD program in Australian universities before WWII and that most qualified graduates who wished to take up research went overseas, mainly to Great Britain.

Rayleigh, JJ Thomson, Rutherford, and Taylor had made their discoveries. There were opportunities to listen to lectures by people with famous names, and in the evenings there were meetings of societies, some for the reading of papers on current research and some for discussion of the politics of the reconstruction of Europe. Nor did these activities seem to detract from the time I had available to read and think about turbulence. Never before or since this early post-war period of work for a PhD have I experienced such a glorious sensation of boundless intellectual opportunities and freedom to try to make something out of them. I imagine–and hope–that many beginning research students have similar feelings today. Truly, research students are the princes of our scientific community.

As may be seen, I was extremely fortunate with the choice of fluid mechanics as my research field, fortunate with my PhD supervisor who became my mentor and friend, and fortunate with my collaborators, Alan Townsend in particular. Later, I was privileged to supervise the work of some outstanding research students. There have been periods of my life when I have devoted a large proportion of my working time to activities other than research, in particular to teaching, administration, and editorial work, but research has always been my underlying chief love and preoccupation. Now that I have had about half a century of living with research, I ask myself what I think about this way of life. I want to try to answer that question in an impersonal way which I hope will be of interest.

Research is more than an occupation

Any assessment of the nature of scientific enquiry as a human activity must recognize that research is more than an occupation or a career. One becomes *hooked* on research, which can be, and usually is, a demanding and compelling search for knowledge which dominates your life. Put romantically, curiosity-motivated research is a voyage of discovery.

There is no end to lands awaiting discovery, nor does the voyager tire of discovery. In more prosaic terms, one can never have too much of it and there is no such thing as enough. There cannot be many other occupations which give enormous satisfaction and excitement and also qualify as work. And in its power and its urgency there is nothing to compare with research.[2]

However, research is not all pure pleasure. Every research scientist will have experienced the agony of not being able to understand some phenomenon or process or mathematical result. This search for enlightenment on a specific problem can be both exhilarating and agonizing, a paradoxical mixture of pleasure and pain which perhaps is peculiar to scientific research. The nearer one is to a resolution of the problem, the sharper the anguish, and the more reluctant one is to stop and do something else. There may be a prize ahead, for if the resolution comes in the form of a new development, unknown up to that instant, that is a moment of ecstasy worth all the hours of travail.

There may also be a penalty for this obsessive behaviour. Research cannot be done to a timetable, and the scientist on the track of a new development will rearrange his other activities, if possible, so as to avoid having to stop at what he is sure is a crucial stage, likely to be the prelude to the desired clarification. Well, scientists have spouses and children and friends, and it is asking a lot of them that they should have to order their lives around the obsessed scientist who lives in another world while the chase is on. We should ask ourselves: when time for research is in competition with time spent with one's spouse and children, do we choose the family–always–often–seldom–never? Research scientists often make poor marriage partners! And the obsessed scientist is more likely to be a male, I believe, since in my view women are more balanced and less likely to allow one activity to dominate their lives.

If you fancy I am exaggerating these demands made by a scientific spouse, I recommend you look through a few scientific books to see whether the authors have dedicated their books to some person and, if so, what have they said? You will find, more often than not (at any rate in North America), that the authors have taken the opportunity to confess their selfish behaviour and to beg for forgiveness from their wives. Here is a typically fulsome dedication which comes from a current textbook on fluid mechanics: "This book is dedicated to my devoted wife, whose continued love, patience, and forbearance made its completion possible." And here is a more down-to-earth declaration from the preface of a book by one of my colleagues at Cambridge: "My wife needs no thanks as she is even more pleased than I am to see this book complete." It says a great deal for the tolerance of all these neglected wives that the book in question is usually not even understandable. No wonder that an author sings his wife's praises in his dedication!

I do not know how to reconcile these two aspects of the life of research, firstly, the pure bliss accompanying a bright idea or clarification that comes as a consequence of long periods of concentration, and secondly, the guilt accompanying demands made on family and friends and the loss in personal relationships. Perhaps the best a scientist can hope for is a compromise rather than a reconciliation.

A research scientist may make himself unhappy by his insistence that the only worthwhile goals in life are more discoveries and new developments. If his ability does not match up to his research expectations, he must make a painful adjustment. Research is a cruel game for those who are not original and independent in their thinking, because a lack of success is inevitable and is evident to one's colleagues. Practitioners of other fields of creative work feel the same restless discomfort, unless they are outstandingly successful. The Australian writer Thomas Keneally, who wrote the remarkable book *Schindler's List,* put these words into the mouth of one of his characters, a writer, in another book: 'I was unhappy somehow with my own writing, with my publisher, and with my purely literary prospects. This is typical of people who follow my craft. There are only a few novelists of

[2] I use the word research here to mean the creative aspects of what scientists and engineers do. There are creative aspects of other intellectual fields, and no doubt similar remarks may be made about these other fields. However, this article is written by a scientist for scientific readers and I make no claims concerning its applicability to other intellectual fields.

my acquaintance who are pleasant human beings meant to belong in families.' *Pleasant human beings meant to belong in families*–that hits the nail on the head!

The struggle against aging

Age sharpens these psychological problems, and here there may be real tragedy. The erosion of the brain cells is continual, and nothing can prevent the ultimate decline of mental faculties. Nothing in one's previous life prepares one for growing old, and unlike most other human conditions the degree is certain to get worse. What can an old scientist do that compares in satisfaction with what he formerly did? How can a successful scientist ever be content to retire from research? One of my acquaintances in England, a distinguished aerodynamicist, committed suicide around 1960 because, so it is believed, he could not face the decline in his intellectual abilities that comes with increasing age. I know of some other Fellows of the Royal Society who preferred suicide to the anguish of not being able to continue to be creative. The best known of these men was the pure mathematician GH Hardy, whose attempt at suicide was unsuccessful and who gives some hint of his anguish in his well-known little book, *A Mathematician's Apology*. It seems incredible that these men could not think of any activity other than research which would give them sufficient reason for staying alive. Or are we not being sufficiently imaginative about the devastation in the life of a successful scientist when he no longer gets good ideas? Either way, it is clear we are unwise to teach our students and postdocs that research is the only thing worth doing, for when they become old they will find that research is the one thing they cannot do.

Some scientists have thought it better to stop early–preferring the contentment earned by past successes in research to the possibility of further success and the risk of becoming acutely disappointed when the good ideas ran out. Not many active scientists have told us about their views on the impending crisis. I have come across the views of two who chose to stop early. One is Lord Rothschild, a distinguished biologist and research administrator who wrote in his autobiography[1] "I had one success: to know when to stop. At the age of 48, I realized that though I could continue, with assistance, to do quite interesting experiments and knew how to present the results in a way which ensured their publication, my work was becoming monotonous and not too interesting. I therefore decided to stop." The other was Eric Ashby, a distinguished plant scientist who moved at the early age of 46 from research and teaching into university administration, initially as Vice-Chancellor of Queen's University, Belfast. He explained the rationale for the move as follows: "I decided ... that I was more interested in people than in ideas, and in teaching and educational issues than in pure science."[2]

It would be interesting also to learn the thinking of those who chose to soldier on, well aware that the fruits of research were becoming fewer and smaller, but not in danger of being driven to suicide. Becoming accustomed to the fact that the loss of short-term memory ultimately makes fruitful theoretical research virtually impossible is a painful episode in the lives of many scientists. It is then vitally important to develop other interests and, in particular, to find ways of utilizing a lifetime of research, *eg*, in the writing of books, without trying further to add to the store of new knowledge. Perhaps George Bernard Shaw had creative work especially in mind when he wrote, rather cruelly, that "He who can, does; and he who cannot, teaches."

I seem here to have developed a perfect pitch for another one or two of these new *Retrospectives* in *Applied Mechanics Reviews!* But with all seriousness, I believe discussion and debate of the problem of aging for an active scientist should be encouraged. It is an important issue and the conventional modesty of scientists should not stand in the way of candour.

Originality

Dedication, and even a measure of fanaticism, is thus a common characteristic of people who are successful in scientific research. There are other such characteristics, and the most important one is probably originality. Originality appears to be an ability to think independently and imaginatively–*lateral* thinking, as it is called by Edward de Bono, a British writer on the nature of creative thought. It is an elusive quality which cannot be taught or learnt, at any rate not directly, although it is easily recognized. Scientific originality seems not to be strongly correlated with wisdom or philosophical depth, and it is not needed for success in human leadership. Sometimes it appears to be an isolated quality, like a special gift, or a knack. The greatest scientists have an uncanny ability to identify and understand the essential aspects of a phenomenon or a problem that everyone will see later to be significant and of wide applicability; and this insight cannot easily be distinguished from originality.

The most original scientist I have known was my mentor, GI Taylor, who demonstrated a gift for scientific originality and insight throughout a working lifetime of over 60 years. He died in 1975 at the age of 89, and was one of the great men in the field of mechanics in this century. He had heredity on his side, for his grandfather was George Boole, the founder of symbolic logic, and one of his aunts was likewise a self-taught mathematician of high ability. Taylor was a likable, happy man with an uncomplicated character and a razor-sharp mind for which scientific investigation was a natural activity. He was not a natural teacher or lecturer, but what I know of doing my kind of research was learnt from his published papers. He published over 200 papers on the mechanics of fluids and solids, and I wish I had time to tell you about some of the gems of insight contained in these papers. A reader wishing to know more about this remarkable man and his work will find it in my recently published biographical study.[3]

I have spoken hitherto about the nature of good ideas in the abstract, and I shall now describe briefly three of Taylor's papers in order to try to convey the nature and significance of good ideas in a more concrete way. Taylor was capable of the highest level of originality, and no amount of conscious imitation will enable us to be as original as he was, but I believe we can learn useful lessons from a study of the way he went about his research and the nature of the ideas he sought–and usually found. Good ideas are essential for progress in research, and it may be illuminating to see what they look like as conceived by GI Taylor. Since space is limited, my three selected good ideas must be simple ones which are free from technical detail. I shall try to make them understandable to you and will be disappointed if I fail.

GI Taylor[4]: Expansion viscosity of a fluid

My first example of a nice idea conceived by GI Taylor is concerned with the effective expansion viscosity of a fluid, and shows scientific imagination of a kind which I believe is characteristic of his work. It is shown in textbooks on continuum mechanics that two material constants are needed for the phenomenological description of the stress in a moving fluid. One is the shear viscosity, defined as the constant of proportionality in the linear relation between the deviatoric parts of the stress and rate-of-strain tensors. The second material constant may be defined in a similar way as the constant of proportionality in the linear relation between the departure from equilibrium of the isotropic part of the stress tensor and the isotropic part of the rate-of-strain tensor, the constant in this case being referred to as the expansion viscosity.

In April 1954, the Royal Society held a discussion meeting in London on *The first and second viscosities of fluids* in which Taylor took part. The so-called second coefficient of viscosity (which is another name for the expansion viscosity) was evidently a rather mysterious concept then, and the record of the discussion is full of learned contributions from the thermodynamical, rheological, and molecular points of view. The shear viscosity represents transfer of momentum between adjoining elements in the moving fluid owing to the random movement of molecules and the action of intermolecular forces, but there was no similar understanding of the mechanical processes represented by the second coefficient of viscosity. Taylor perceived that in these circumstances it would be helpful to have a concrete example of a medium for which the second viscosity was non-zero and could be calculated explicitly.

Now the essential characteristic of such a medium is that expansion is accompanied by dissipation. Taylor saw that a dilute dispersion of small spherical compressible gas bubbles in an incompressible viscous liquid has this property, and that the rate of dissipation is calculable. When a time-dependent compression is imposed on such a dispersion the bubble radii change, and it may be shown easily that the total rate of dissipation in the liquid surrounding each bubble of (instantaneous) radius a is given by

$$16\pi\mu a(da/dt)^2.$$

On the other hand, the expression for the rate of dissipation per unit volume of a continuous medium with expansion viscosity κ undergoing expansion at the rate E is κE^2. Hence on relating E to the rate of change of bubble volume and equating the two expressions for the dissipation associated with the volume V containing N bubbles, we find for the effective expansion viscosity the expression $4\mu/3\varphi$, where φ is the volume fraction of the bubbles and μ is the shear viscosity of the liquid.

The details of the calculation here are not important. The point is that the bubble dispersion is a realistic medium with an effective expansion viscosity which can be calculated by simple methods and whose physical meaning may be examined. We see that the expansion viscosity is non-zero here essentially because the adjustment of the pressure in the bubbles takes time, being resisted in the case of this particular medium by viscous stresses generated by the radial motion of the water near each bubble. With this concrete case to guide one's thinking about the processes represented by the expansion viscosity, the mystery disappears. I doubt if any of the general arguments and conclusions presented at the discussion in 1954 are remembered today, but Taylor's deceptively simple little note has provided a textbook example–still the only one available–of a real medium with a calculable expansion viscosity. Taylor's contribution here displays creativity which is the better for being so simple.

GI Taylor[5]: CQR anchor

GI Taylor had a remarkable geometrical and mechanical imagination, and many of his pieces of apparatus display this gift. He was not primarily an engineer who gets his satisfaction from designing something useful, but on the few occasions when he thought carefully about how to meet a specific need the results were striking. My second example of a good idea by Taylor is an invention, rather than a gain in understanding of a mechanical phenomenon and in this respect is untypical of Taylor's work, but the imagination which lies behind the invention is characteristic of him.

Taylor was a keen sailor, and the boat in which he made some adventurous voyages around the North Sea in the late 20s was a large one, 45 feet in length and drawing eight feet of water. In confined waters, and especially with an on-shore wind, it was necessary to wind the anchor up quickly so as to get the boat under way before drifting towards the shore, but the anchor was heavy, 120 lb, too large for one man to handle. It happened that Taylor was a member of a government committee on sea-planes in the early 30s and there he was made aware of similar problems of the anchoring of sea-planes in estuaries. Thus in two contexts there was need for a new design of an anchor which had a greater holding-force for a given weight. Could GI Taylor help?

Taylor came up with a radically new design which was probably the first major advance in anchor design since the time of the Greeks. In later years, Taylor described how he arrived at the new design, but it is not easy to follow his creative thinking and I shall simply show you how the anchor works in practice. Figure 1 shows a sketch, in which you should note the pin C which allows relative rotation of the blade D and the shank A. The clever feature of the design is that when the anchor chain is pulled the anchor takes up an orientation symmetrical about a vertical plane at a position just below ground level, where the resistance to movement is great. The three photographs in Figure 2 indicate how this position is taken up by the anchor. The top one of these photographs show the anchor in a typical position after having fallen on the sea-bed. Then, when the pull on the anchor chain begins, the point of the double blade digs in like a plough-share and heels over, as in the second photo. Finally, the blade sets itself in line with the shank and penetrates to a depth such that the whole of the double-blade is buried, the orientation of the anchor then being symmetrical about a vertical plane. Tests showed that the new design has a holding-force to weight ratio which is 4 to 5 times as large as that obtainable with the best traditional design. Taylor and his collaborators gave to the new anchor the trade name CQR (= secure).

GI Taylor[6]: Instability of fluid in a vertical tube

This gift which Taylor had for being able to visualize three-dimensional geometry and to see how mechanical principles operate in three dimensions was employed often in the design of apparatus for his experiments. Always, the apparatus was simple and beautifully adapted to its purpose, as can be seen most clearly in the papers he wrote after his formal retirement as a Royal Society research professor in 1951. A reader who wishes to see what can be achieved by the ingenious design of experiments should watch the film on *Low-Reynolds-Number flows* produced by Educational Services Inc in 1967 under Taylor's direction. Taylor was an inspired choice as director of one of these films, despite having little personal interest in teaching, because he responded to the challenge to design apparatus which would illustrate the principles of fluid flow with negligible inertia forces. Figure 3, for instance, shows a tee-to-tum, a little toy designed for the film which illustrates lubrication theory when its tilted blades are spun in the appropriate sense before being dropped on to a smooth horizontal surface. The design is perfect for the purpose, and any of us could surely have invented it, although, as it happened, Taylor did and we did not.

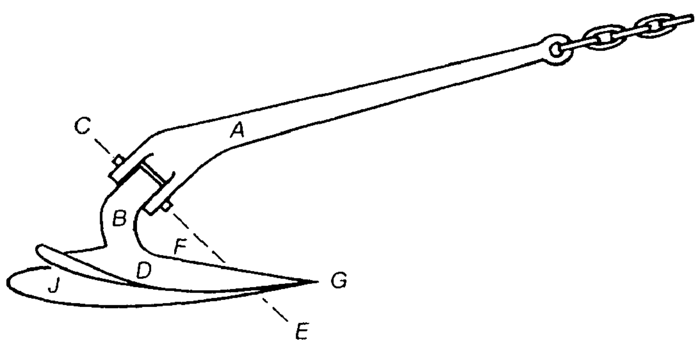

Fig 1. Sketch of the anchor showing the lettering referred to in the text.

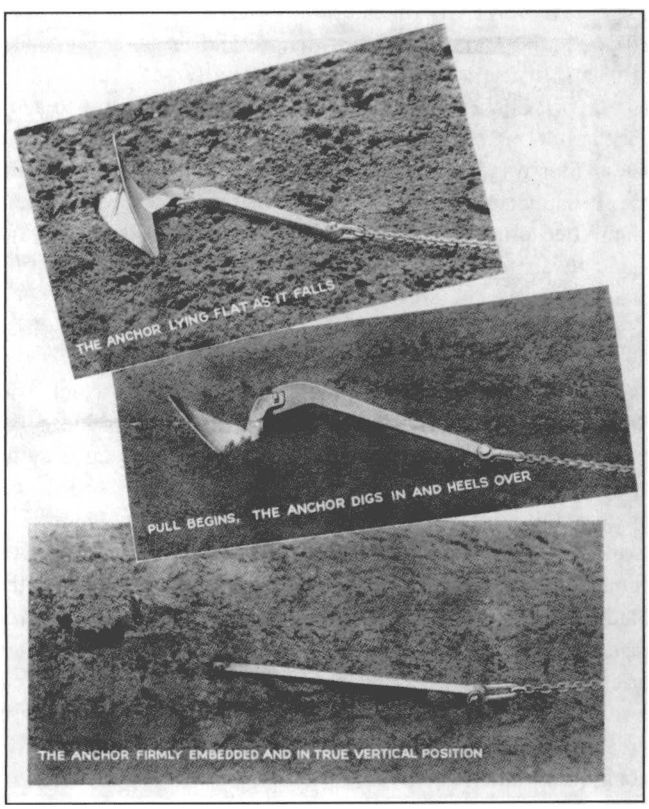

Fig 2. Photographs of the anchor showing the way in which it digs itself into the sea-bed like a plough-share regardless of the way it falls.

I can illustrate further Taylor's gift for the design of experiments by referring to a nice investigation of the critical conditions for instability of stratified liquid of viscosity μ in a vertical circular tube of radius a. By conventional analysis of the behaviour of a small disturbance to stationary fluid with a linear density gradient, he showed that the critical value of the density gradient above which a small disturbance will grow exponentially is $68D\mu/ga^4$, where D is the molecular diffusivity of the physical quantity which determines the liquid density (concentration of solute, for example). Taylor got great satisfaction from experimental verification of theoretical results, and he would not have regarded a theoretical relation by itself as interesting. How, then, did he confirm the above theoretical stability criterion using simple laboratory equipment? How would a reader go about this experiment? The answer is given in a little known paper by Taylor[6].

GI Taylor's strategy of research

It would be natural to suppose that so successful a scientist as GI Taylor usually made a conscious choice of the field of mechanics which he would next explore. Surely his success was due, in part at least, to the shrewd perception that certain broad fields were ripe for investigation? Well, such a speculation may be natural, but, as Taylor himself made clear, it is incorrect. In several articles written in his later years, he points out that during the whole of his life he had simply responded to external developments and that forward planning

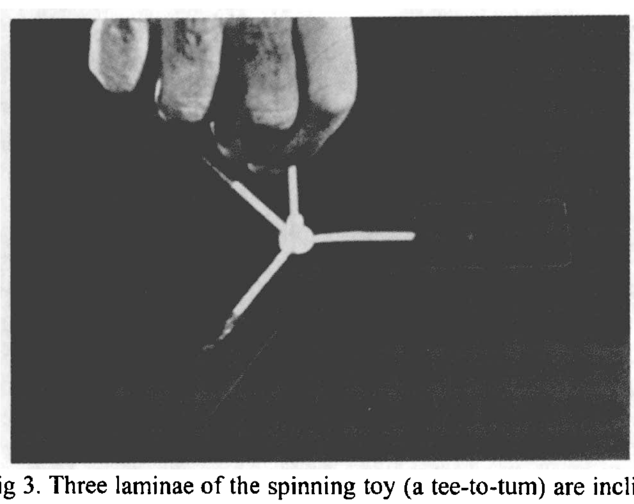

Fig 3. Three laminae of the spinning toy (a tee-to-tum) are inclined slightly so that the front edges are higher.

had not played any part in his career, either in his research or more generally. There are other respects in which Taylor's thinking is a little surprising, and in 1971, when he was 85, I persuaded him to put on paper his replies to a number of general questions concerning what one might call the strategy of his research. I confess the results were not very successful, I think because Taylor was unaccustomed to this kind of introspection. Nevertheless, the questions and answers together made an interesting article[7] in the *Journal of Fluid Mechanics*. Taylor suffered a severe stroke in 1972 and died in 1975 before the dialogue was finished.

I do not have enough space here to summarize the whole article, but I should like to reproduce a paragraph near the end in which Taylor makes an explicit statement of the role of *particular problems* in a research strategy. Here is the paragraph in question:

> "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 key sentence here is *It seems to me that it is by attention to specific problems (which can be subject to experimental verification) rather than by generalized reasoning that advances are made in our subject.* Many of us with mathematical training have always supposed that particular cases and concrete problems are in some way inferior in intellectual status to generalized reasoning, but Taylor says otherwise and he proved his point in an outstandingly successful lifetime of research in mechanics.

Encouraging inspiration

I propose now to look at the way in which scientists at a humbler level get the ideas that are manifestations of their originality. I want to enquire whether there are some especially fruitful sources of inspiration. This question concerns us all in some degree, and has interested me for many years. I emphasize that I am not using the word *inspiration* here in the exalted sense that goes with genius. I am referring to the more modest revelations and flashes of insight that most research workers experience from time to time. Our perception of what constitutes a good idea is usually determined by our own intellectual capacity. Here is a statement by Proust from his novel *Rememberance of Things Past* which puts this more bluntly and which might have been written for scientists: "Clear ideas, for each of us, are those which lie at the same level of confusion as our own." Modest though these occasional good ideas may be, they give us a great deal of satisfaction when they stand up to sober scrutiny. They make research the marvelous game that it is, and confirm us in the view that it is what we should be doing. The question for consideration is what conditions seem to favour the genesis of good ideas?

This question is seldom raised among scientists, which is strange, bearing in mind the enormous importance of inspiration for each of us personally and for the advancement of science generally. It is also strange that sociologists go to no end of trouble to enquire into our personal habits and our opinions on matters of no great moment, whereas they seem not to have surveyed scientists to ask how they get their ideas. In the absence of such an enquiry, let us ask ourselves about the conditions that assist clear thought.

It is natural to consider first what outstanding scientists have said on the question, but there is a problem here in that the gifted few get inspiration so easily that they do not know why the rest of us find it excruciatingly difficult. The distinguished atomic physicist Ernest Rutherford said, in one of his many immodest moments, that he could have done physics at the North Pole. I believe his point was that he did not think he was in any way dependent on his working environment, although there must have been an unstated proviso that he had access to a workshop in which he could make his apparatus. GI Taylor, who incidentally was a close friend of Rutherford, used to do all his calculating and writing sitting on a sofa in the drawing room of his home, and came to his room in the Cavendish Laboratory mainly for experimental work. Like Rutherford, he could have done physics at the North Pole, although unlike Rutherford he would never have said so. Well, isolation may not have mattered to Rutherford and Taylor, but for most of us it has a devastating influence. There are few who can sustain a long research enquiry without contact with others. We need to be close to colleagues, not necessarily ones who know in detail what we are doing, although that helps, but preferably people who appreciate the general purpose of our work. We need their understanding and occasional encouragement and the stimulation that comes from the presence of people engaged in similar work. So, unlike Rutherford and Taylor, I would specify the working environment represented by an institution as the first of several factors that normally have a major influence on the generation of new ideas.

Judging an institution

That leads to the question, what makes an institution a good place to work in? It should be possible to describe objectively the features of an institution that are favourable for independent and original thinking by its members, although I do not know of anyone having tried to do this systematically. Most people, particularly those looking for jobs, tend to rank institutions in terms of the number and quality of the current staff in their own specialty. This simply reflects the common view that the more good colleagues with kindred interests we have, the better we shall get on with our own research. It is no doubt true at any rate for younger people, but it does not tell us anything about the institution itself.

For more useful answers to our enquiry, we need to think about the value of the various things that an institution provides, namely, supporting facilities, working conditions and practices, and the less tangible attribute summed up by the word *atmosphere*. It is obvious that good experimental work is impossible without a workshop and the help of skilled machinists and electronic and photographic technicians, and computing facilities are likewise essential. It should also be recognized, although not all administrators would agree, that writing and thinking work is greatly assisted by the existence of a library of specialist books and journals under the same roof. Administrators may also dispute the desirability of a comfortably-furnished coffee-break room, but I believe they would be mistaken to do so. The intellectual stimulation that comes from colleagues engaged in generally similar research cannot be planned, and it is important that there should be daily opportunities for spontaneous exchanges of information, opinions, and ideas. The Department at which I work in Cambridge encourages this informal communication by providing coffee tables with laminated tops on which people may write or draw, and I believe this simple device has endeared the Department to several generations of young scholars.

A feature of an institution which provides a measure of its intellectual health and vitality is the holding of regular lectures and seminars at which people with related interests gather to hear, and comment on, informal presentations of current research. In my view, the occasions on which people meet and listen to accounts of work by a colleague can be high points in the life of an institution. The symbiotic relationship between a person who wishes to tell others about the good work he has recently done and an alert and critical audience of people interested to argue about the latest developments brings out the best in both parties. This is the time when sparks may fly and there may be a real sense of the boundaries of knowledge being extended. There is a heightened awareness on such occasions, and in my experience one may *see* things for the first time as a consequence of something said by the speaker. A good deal of research, at any rate of the theoretical kind. consists of realizing what later often seems to be almost obvious, and a research seminar can be a fertile medium for such realizations.

The insights and ideas that come to one while listening to a seminar talk or a conference lecture tend to arise in our minds unexpectedly and by chance. There are other occasions when we are trying hard to understand something specific and are consciously seeking inspiration. We may have in our heads certain facts derived from observation or experience or previous calculation, but they are not all compatible with prevailing theories. Something is wrong with the picture in our head. We go over all the data again and again and check all the logical steps in the argument, and unless some new idea or interpretation comes to mind the torment continues.

People have their own ideas about what to do in this kind of situation. I have a friend who says he finds a leisurely hot bath helpful, especially if taken first thing in the morning; and you will remember that Archimedes got a marvelous new idea out of his bath. Some like to go and talk over the problem with a colleague, not so much in the expectation that he will be able to clear it up but more because light may dawn if the colleague asks probing questions. Personally, I prefer solitude, especially in the period before falling asleep at night. Not all the bright ideas that come to one at night still look good in the cold light of day, but some do. If success is achieved under some such conditions, it will of course have been due primarily to the depth and clarity of the picture of the whole problem that was first formulated in our mind. All that external conditions do is to make it easier for the ideas to be conceived. You still have to work at it–unless, that is, you happen to be a Rutherford or a GI Taylor.

Role of scientific journals

A description of the life of a scientist engaged in research which made no reference to the role of communication by the printed word would be seriously incomplete. Interesting and useful brief developments may be communicated orally within an institution, but for communication of complete pieces of research we rely largely on the printed word in the form of papers published in scientific journals.[3] The scale of this distribution of papers is impressive. It has been estimated that there are more than 30,000 scientific journals in existence, and that during the past 20 years they published as many new papers as had previously been published in all previous history. Underlying this torrent of words in print is the remarkable ubiquity of that unit of communication, the scientific paper. It seems extraordinary that, for very many years and in most fields of science, a paper recording the results of, say, two to twelve man-months of work has been by far the most commonly used vehicle for the dissemination of new knowledge. In fluid mechanics, in particular, there has been very little change in the form and nature of published papers over the past century or more, and papers by Reynolds and Rayleigh and Stokes would be at home in one of today's journals; only the more mannered and controlled style of writing would distinguish them.

Publishing a paper in a scientific journal as a means of disseminating new knowledge has been standard practice for many years. The fact that it works so well is due in large measure to the reviewer system. The practice of seeking ad-

[3] Developments in *electronic publishing* of scientific journals are being made very rapidly at the present time, and it is difficult to forecast the future. I write about journals as they are at present, but they may soon have to change. However, the critical assessment of scientific papers will always be needed. In February of this year UNESCO organized a large-scale conference on the current position of Electronic Publishing in Science and produced a large number of recommendations, the first of which reads as follows: "The Conference overwhelmingly recommends that strict peer review should be applied to all scientific material submitted for publication in electronic journals." I agree, and it is the process of 'strict peer review" that I shall be discussing.

vice on the suitability of a paper for publication from qualified reviewers is of such long standing, and is so widely used, that we take it for granted. It is a remarkable practice, unique to science in its scale and universality, and it is the effectiveness of this system that enables a journal to maintain high standards. Reviews play an essential role in the selection of papers for publication, and editors would be helpless without them. Is there another field of human thought or endeavour in which the relevant community give their time and mental effort constantly, without thought of reward, and anonymously? The secret of the success of the system of peer review of papers submitted for publication no doubt lies in the fact that each scientist acts as an author on some occasions and as a reviewer on others, and understands what he is expected to do in either of these two roles. Taken as a whole, I believe the system of regular publication of large numbers of papers selected on the basis of peer reviews is one of the success stories of the world of research in physical science.

I interrupt the thread of my article for a moment to say that a very different view of the peer review system is presented in a provocative article entitled *Conduct and Misconduct in Science* by David Goodstein, sometime Vice Provost of Caltech (and evidently published in *Engineering and Science,* Winter 1991). The nub of Goodstein's case is contained in these two sentences taken from his article: "Peer review is quite a good way to identify valid science.... Peer review is not at all suitable, however, to adjudicate an intense competition for scarce resources such as research funds or pages in prestigious journals." In this statement, he lumps together referees for an application for a research grant from a national agency and referees for a paper submitted to a scientific journal. Goodstein points out that the referees for a grant application may be in competition with each other, since the total amount of money available for grants is limited, and he may be correct in thinking that this sometimes leads to some loss of objectivity in the grading of the applications. But the referees for a paper submitted to a journal are not subject to the same pressures. The number of papers published per year is not limited in any precise sense, publication is a more international business than research grants, and good papers always find outlets. Goodstein goes on to allege that "referees are able, with relative impunity, to delay or deny funding or publication to their rivals." I do not believe there is any evidence for the truth of the part of this assertion that is concerned with publication in journals, nor do I think it is a plausible proposition.

Well-prepared papers

The universality of the scientific paper as the preferred form of communication of new knowledge suggests we should think carefully about its construction and preparation. The contents of a paper are of value only to the extent that they can be understood by others. It is therefore vitally important for the general progress of science that papers should be written clearly, precisely, and attractively, so that readers are helped to comprehend the new developments presented in them. Clearly and precisely–everyone would assent to that, at any rate in principle. The desirability of the writing being attractive is less often referred to, but it is just as important. Reading a paper is a voluntary and demanding task. and a reader needs to be enticed and helped and stimulated by the author. Contrary to popular opinion, the words in a theoretical paper need to be understood no less than the equations for the effective communication of science.

An author has two powerful incentives to make the message in his paper accessible and interesting: the first is that by doing so he will contribute to scientific progress, and the second is that he will contribute to his own reputation. One might suppose he would therefore do his best to make it a minor work of art. As any editor knows, the truth, alas, is usually otherwise. There are of course many papers which show clear signs of having been prepared with care and craftsmanship, but the average level of composition in papers submitted to journals is disturbingly low. Any careful reading of the typescript of a newly-submitted paper should suggest to an author ways in which a word or a sentence can be improved; but the state in which many typescripts are submitted suggests there has been no such scrutiny. How could an author read over his typescript and remain content with the following real example? 'Let the field direction be in a direction parallel to the x-direction.'

Why then do authors not respond to the two strong incentives to try to make each of their papers a literary and intellectual gem? Part of the reason, I believe, is that most scientific workers feel an even stronger incentive to get on quickly to the *next* paper. The peak of satisfaction in scientific work comes at the moment when one suddenly understands something. After that moment of discovery, there comes the less exciting task of exploring the consequences of the new insight and writing it up for others to read. It requires patience and dedication in any circumstances to carry that demanding task through to completion, and if the author already feels the excitement of the chase after another discovery, writing up the previous one is unlikely to get his undivided attention. This association of *writing up* with the relatively dull post-discovery phase poses a psychological problem for scientists. It is not found to the same extent in literary disciplines, because there the creative act lies more in the writing itself and the excitement is sustained to the end of the composition.

If the present poor average standard of composition in scientific papers is to be raised, and if the preparation of a paper is to be turned into a minor art form, as is desirable in view of its dominant role in scientific communications, we shall need to proclaim openly and often the importance of good writing. And we shall need to find ways of showing young scientists how to present their work just as we teach them other relevant skills. It is sometimes maintained that the inclusion of courses on the humanities in all undergraduate curricula would make engineers and scientists more literate, but I believe the help should be more specific and more intensive. I look forward to the time when instruction in the preparation of papers is included in the training of research students and is regarded as a vital part of that training. The fact that literacy is also the key to the world of literature is a bonus for the scientist who learns to write well.

Some conclusions

I set out to describe the main features of a life spent in the grip of research, and to ask myself whether after 50 years it was worthwhile. Clearly, I think it was well worthwhile; and I shall conclude now by giving you my personal views on some of the relevant issues in summary form.

1. Research is an intrinsically optimistic and pleasurable activity, with success and improved understanding always expected in due course.

2. For those who have some scientific originality, no activity can compete with research for excitement and pleasure and satisfaction. And there is no such thing as having enough of it. A scientist is likely to be regarded as obsessive about his research.

3. The demands made on one's spouse and family and friends by the need for long periods of preoccupation and isolation are often excessive. (The fact that GI Taylor, who had no children, lived for his research but always had time for leisure activities such as sailing might seem to be a counter example, but he worked extremely quickly and seldom needed long periods of concentration. As in many other respects, the experience of an outstanding scientist may not be directly relevant for the rest of us.)

4. Old people who carry in their minds expectations of the same degree of success in research that they had in their earlier days may find adjustment difficult. I can find no answer to the question: how can a person be content to retire from research?

5. A scientific paper is easier to understand and more pleasing to read if it is written with clarity, precision, and elegance, but relatively few are written in this way.

6. I believe that the pursuit of natural knowledge is a civilizing and ennobling activity. This civilizing influence of research is too large an issue for discussion at this stage of my article, but I shall leave readers with one illustration of what I have in mind. A scientific enquiry may be carried out by an individual, but at any moment there are others engaged in the same or related enquiries in other laboratories in the same and in different countries. Through having common objectives and principles by which new knowledge is assessed and disseminated, scientists concerned with a particular field like fluid mechanics form an international community of great unity and moral strength. I believe that the understanding, trust, and goodwill between members of this scientific community transcends geographical and political boundaries and constitutes one of the most important forces for international harmony and friendship in the world today.

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