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Advice to a young researcher: with reminiscences of a life in science

J. Michael T. Thompson

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Advice to a young researcher: with reminiscences of a life in science

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This paper provides an informal guide to young researchers in science and engineering as they progress for their first 10 or so years from the time that they first started thinking about doing a PhD. This advice is drawn, with examples and anecdotes, from my own research career which started at the Cambridge Engineering Department in 1958, and progressed through 48 years at University College London to a part-time chair that I now hold in Aberdeen. I hope it may encourage and help tomorrow's scientists on whom the Earth's future very much depends.

1. Introduction

This Festschrift for my 75th birthday is kindly being organized as a Theme Issue of *Philosophical Transactions of the Royal Society A* by Isaac Elishakoff, a distinguished professor at the Florida Atlantic University, and at his suggestion I am including here a few informal reminiscences from my lifetime of scientific research. One way of structuring these memories, I realized, would be to assemble some frank and informal advice for young university scientists during their early careers. I have adopted this approach, following chronologically the progress of a notional researcher for 10 years from when he or she starts thinking about doing a PhD. As leavening features on this structure, I have incorporated anecdotes and stories that serve to illustrate the topics under discussion.

   The resulting article might entertain and amuse my friends and colleagues, while the potpourri of advice (certainly not a systematic treatise!) might prove instructive to the young. It has been fun to write, and I hope it will prove enjoyable and useful to my readers.

2. Pleasures and rewards of research

I write as a lifelong researcher, now semi-retired, seeking to help talented young students who might take, or have just started on, the same track. A genius needs no such guidance, and should read no further. As Edward Bulwer-Lytton succinctly expressed, *talent does what it can, genius does what it must*.

I have enjoyed every minute of my research and the free lifestyle that it engenders. The joys and rewards of research are indeed well described by George Batchelor, a top researcher in fluid mechanics and founding head of the Department of Applied Mathematics and Theoretical Physics in Cambridge, who enthused in his very readable article [1, p. R20]:

> 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.

3. Am I good enough?

(a) Intelligence versus enthusiasm and perseverance

So what do you need to be a successful researcher in a scientific discipline? Clearly, a certain amount of intelligence is needed, consistent, let us say, with getting (in the UK) a first or upper-second class honours degree. Beyond that, other factors, such as enthusiasm, hard work, diligence, perseverance and creativity, become equally or even more important.

In his book *Contrary Imaginations: A Psychological Study of the English Schoolboy*, Liam Hudson [2, p. 108] says about this matter:

> Originality in most spheres would seem to depend, among other qualities, on persistence: on the pursuit of a given train of thought far beyond the limits that the ordinary citizen can countenance.

Later he continues [2, p. 124]:

> The relation of IQ to intellectual distinction seems, in fact, highly complex. As far as one can tell, the relation at lower levels of IQ holds quite well. Higher up, however, it dwindles; and above a certain point, a high IQ is of little advantage.

An earlier researcher, quoted by Hudson [2, p. 128], puts it in another way:

> High but not the highest intelligence, combined with the greatest degree of persistence, will achieve greater eminence than the highest degree of intelligence with somewhat less persistence.

Well, after these various remarks about the makings of a good researcher, my recommendation to you is 'give it a try!'

(b) A good memory may help

I have myself witnessed the simple fact that a good memory might help a lot! During a holiday tour of the USA with my family (wife Margaret, and children Richard and Helen) in 1980, we visited John Hutchinson, a researcher in shell buckling, at his home near Harvard. In the evening, we played a game of Pelmanism (also called concentration or pairs). All the cards are laid face down on the table and players turn over two cards at a time, the object being to turn over pairs of matching cards. We soon learned that this was not a good idea. John simply remembered everything: any card that had once been turned over, he remembered it. This may go some way to
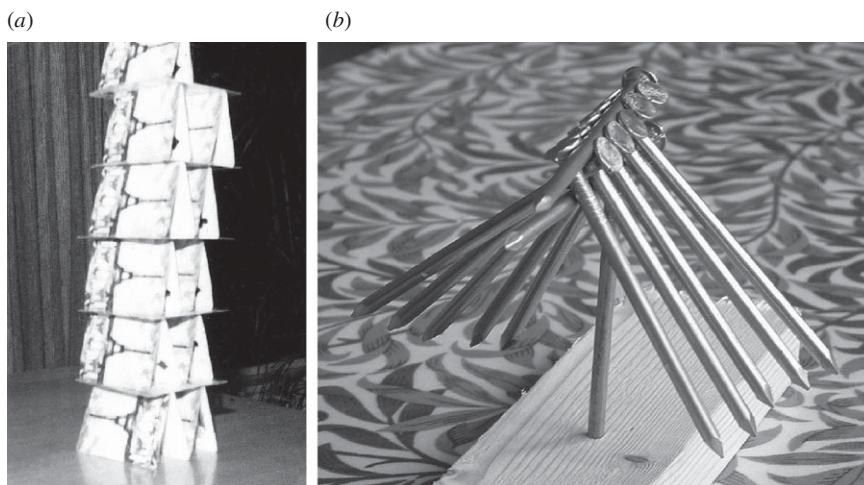
Figure 1. All a matter of balance. (*a*) A house of cards, built by my daughter Helen in 1980 during a visit to John Hutchinson of Harvard. (*b*) Answer to a nail-balancing puzzle, described in §5*g*, posed to me at my badminton club.

explain John's achievement, at the time of this essay, of having 27 214 citations (see §7) to his research in the solid mechanics of fracture and elastic–plastic stress fields. This is one of the highest numbers that I have encountered, having covered many Nobel laureates, ex-presidents of the Royal Society and leading cosmologists.

Despite this total failure at memory, I am happy to say that the honour of the Thompson family was fully restored when we reverted to building houses of cards. We all had a try, including John's son Leif, but my daughter Helen (aged 12) built, with consummate ease, a house that was an order of magnitude higher than anybody else had managed. It is shown in figure 1*a*.

Helen is now happily married to my former researcher, Allan McRobie, who won a prestigious University Research Fellowship from the Royal Society while in my group at University College London (UCL). He is now a reader in the Engineering Department of the University of Cambridge, from which I graduated in 1958, and has worked on the crowd-induced vibrations of the wobbly Millennium Bridge in London [3]. Having developed a late passion for science, Helen is now a senior lecturer in biomedical science in the Faculty of Science and Technology at Anglia Ruskin University (in their Cambridge campus), where she is studying the DNA profiles of black squirrels [4,5]. Richard, meanwhile, is a director (IT) at the head office of a leading financial institution in London's Canary Wharf. There, as a newly elected Technology Fellow, he heads up a team in London and New York responsible for state-of-the-art high-frequency trading systems. He claims the advantage of once *being* a central processor, when helping me with research during his school days (figure 3*b*).

(c) Medawar's intelligence test

The distinguished biologist Peter Medawar (1915–1987) was an Oxford graduate who spent 11 years at UCL as the Jodrell Professor of Zoology. His brilliant research on graft rejection, vitally important for organ transplants, was recognized by the award of the Nobel Prize in 1960. In his informative and instructive book [6], entitled *Advice to a Young Scientist*, he gives his views on desirable characteristics of a researcher. He reinforces Liam Hudson's views with the remark that 'almost obsessional single-mindedness is required by almost any human endeavour that is to be well and quickly done'. He also gives the following as a test of intelligence.

Some faces in El Greco's paintings seem unnaturally tall and thin (figure 2), and a person in a gallery suggests that this might be because El Greco suffered a defective vision, making him see people this way. Could this be a valid explanation? Medawar's view is that anyone who can see
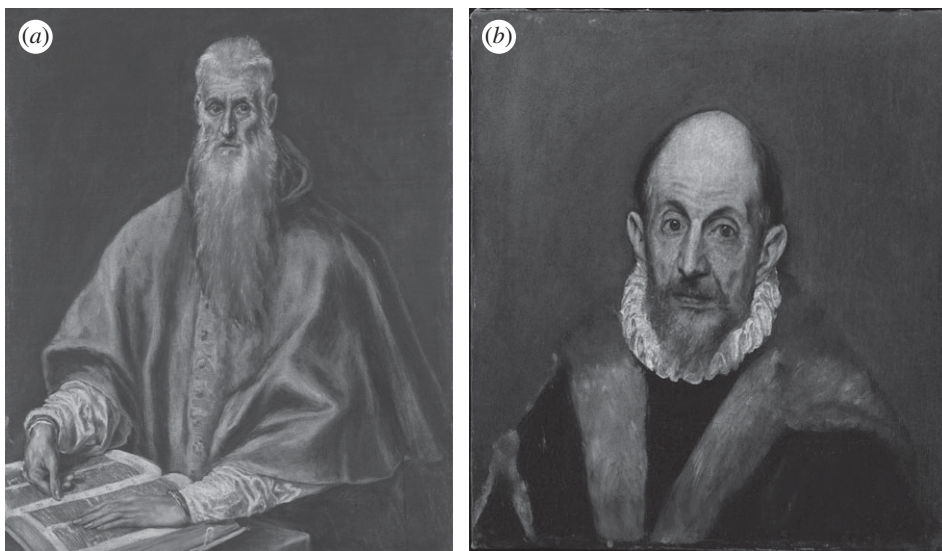
Figure 2. (*a*) 'Saint Jerome as Cardinal' by Domenikos Theotokopoulos (1541–1614) reproduced courtesy of the National Gallery, London. Known as El Greco (The Greek), the painter was born in Crete and settled in Toledo. He was a significant painter of the Spanish Renaissance. (*b*) 'Portrait of a Man', by El Greco, possibly a self-portrait; reproduced courtesy Metropolitan Museum of Art (Purchase, Joseph Pulitzer Bequest, 1924, 24.197.1, www.metmuseum.org).

instantly that this explanation is nonsense is undoubtedly bright. Conversely, anyone who still cannot see it as nonsense even when it is explained (as below), must be rather dull.

The explanation is as follows. Suppose, firstly, that a painter sees double. Drawing a football, which he sees as two balls, he paints one ball on his canvas. He looks at his canvas, sees two balls and puts his brushes away. He has not made what we would perceive as a mistake because he sees the ball, and his picture, through his same defective eyes. In the same way, even if El Greco did see things as tall and thin, his drawings would have the correct aspect ratio and would look normal to viewers in the gallery. I gather that in El Greco's day a spectacle-maker (clearly rather dull) did indeed plan to make a pair of glasses to cure El Greco's presumed astigmatism!

So having passed this hurdle, you are all set to become a researcher.

(d) Scientific method and common sense

A lot has been written about the scientific method, but many agree that it all comes down to systematically applied common sense. So my advice to a starter in research is 'just get on with it'.

In this respect, it is illuminating to read about Batchelor's conversations with G. I. Taylor, which apparently threw very little light on the source of Taylor's much admired originality [1]. Here, I will just quote from Medawar [6, p. 93]. The italics are my addition.

> The generative act in science, I have explained, is *imaginative guesswork*. The day-to-day business of science involves the exercise of common sense supported by a strong understanding, though *not using anything more subtle or profound in the way of deduction than will be used anyway in everyday life*, something that includes the ability to grasp implications and to discern parallels, combined with a resolute determination not to be deceived either by the evidence of experiments poorly done or by the attractiveness, even lovableness, of a favourite hypothesis. *Heroic feats of intellection are seldom needed*.

If you want to have a serious look at the ideas of scientific methodology, you could try reading Popper [7].

(e) Cultivating good ideas

I am indebted for this section on creative problem-solving to a personal communication (2012) from Michael Ashby, Royal Society Research Professor, and a principal investigator at the Engineering Design Centre at Cambridge.

> Where do good ideas come from? They don't just 'happen'. Rather, they emerge from a fascination with a problem, an obsession almost, that sensitizes you to any scrap of information that might, somehow, contribute to finding a solution. Combine this with reading and discussion, loading up the mind, so to speak, with background information and with solutions to related problems that, you sense, might be relevant. The human mind is good at rearranging bits of information, seeking patterns (and links), often doing so subconsciously; we have all had the experience of waking in the night with the answer to a problem that, the previous evening, had no solution. It is like finding a route across previously unmapped territory. The route is what is wanted, but to find it you have to map, at least approximately, the territory as a whole. Or (another analogy) it is like building a scaffold out of many scaffold-poles to reach a remote and awkward roof-top. Only when the last pole is in place can you reach the top; till then it was inaccessible. If there is a moment of real creativity it is probably the insight that provided that last pole. But it would have been no help if the rest of the scaffold were not already in place. To repeat: good ideas don't 'happen'. They emerge by giving the mind the means to find them.

The power of sleeping on a problem applies equally well to routine manual jobs, such as shaving or gardening, where your brain is in an idle mode and your sub-conscience becomes a powerful assistant in cracking a problem. So now it will be up to you to employ your natural youthful curiosity towards generating new ideas, asking difficult questions, and towards developing a healthy scepticism of all that has gone before.

4. Getting started on your PhD

(a) Finding a place with funding

The usual route into research is to stay at university after your first degree and work for a doctoral degree, which usually takes 3 more years. Indeed, it is a young scientist at a *university* to whom this article is primarily addressed. Whatever the field of study, be it chemistry, physics, engineering or mathematics, this degree is invariably called a doctor of philosophy (usually written as PhD, though at Oxford as DPhil). If successful, you will be able to write Dr in front of your name, and some people may even address you as Dr Knowall!

Many people 'stay on' at the university from which they have just graduated, but it might be a good time to make a change of place, and perhaps even subject. As Medawar [6, p. 13] wrote about the choice of subject (my italics):

> It can be said with complete confidence that any scientist at any age who wants to make important discoveries must study important problems. Dull or piffling problems yield dull or piffling answers. It is not enough that the problem should be interesting: *almost any problem is interesting if it is studied in sufficient detail*.

Unfortunately, the choice of place and field may not be entirely optional. Rather, it might be a matter of hunting around to find a university that will accept you (with financial support) to work in a particular research area. In the UK, the government channels money into universities via the various (scientific) research councils for 'studentships' which a university can then award to the most talented students. Under ideal conditions, a student can be given freedom as to what he or she should study. In my case, at the Cambridge Engineering Department in 1958, this involved talking with several lecturers to find one who suggested an interesting and intriguing research topic, and in whom I perceived a nice friendly supervisor. In the event, I made a good choice of
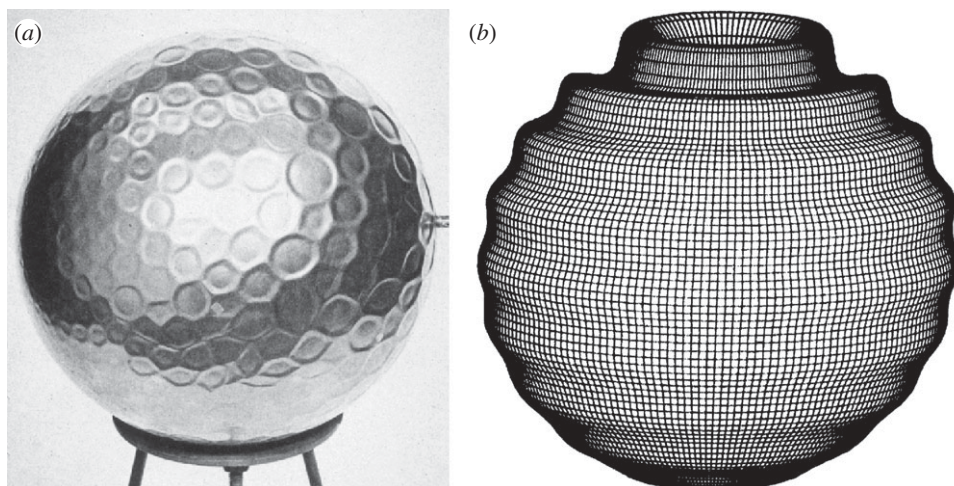
Figure 3. (*a*) An electroplated spherical shell made at Stanford by Nicolas Hoff and his team. It has been buckled into many dimples by evacuating the interior. Note that these dimples have been progressively produced and stabilized by hitting an internal mandrel. So they give no clue to the initial buckling pattern, but do show the high quality of the manufactured shell. (*b*) A theoretical post-buckling shape, created with the help of my son Richard. It was drawn on an old-fashioned (x, y) plotter by a felt-tipped pen traversing a moving sheet of paper. Hidden lines were conveniently 'removed' by pressing the 'lift pen' button when needed! I used it as the logo for the IUTAM Collapse symposium.

'Mr A. H. Chilver', as it said on his door, because in those far-off days the University of Cambridge did not 'recognize' doctoral degrees awarded by other universities. (Cambridge still does not recognize bank holidays!) The PhD of Henry Chilver was awarded by the University of Bristol. This did not stop him becoming first Sir Henry Chilver, and finally Lord Chilver. He has remained a good and close friend ever since I worked for 3 years under his supervision on the buckling of spherical shells (figure 3). He wrote a very kind biographical memoir about me [8] on the occasion of a workshop in my honour held at UCL shortly after my retirement in 2002. His sad death occurred during the writing of this article on 8 July 2012. He is greatly missed by all who knew him as a colleague and friend.

A second route by which money passes from government, via a university, to support research staff (though not now doctoral students) is through a research grant from a funding council such as the Engineering and Physical Sciences Research Council (EPSRC). Academic staff are increasingly pressured by their universities to get these grants which typically provide money for research equipment and one or more assistants. These assistants are now post-doctoral students (post-docs) who already have a PhD. This is one opportunity available later in your career. A member of staff will have worked hard to get one of these competitive grants by making a specific research proposal (on, say, the buckling of pipelines). If he or she were to employ you on the grant it would not be possible to allow you much freedom on the definition of your topic. This applies, even more strictly, to the third route, in which an academic has obtained a grant from industry to perform a fairly well-defined piece of practically relevant research work: it is unlikely to be about the number of regular n-sided polygons in m-dimensional space!

The situation in 2012 about UK funding from EPSRC is that grant applicants can no longer ask for the support of a PhD student. This leaves two EPSRC sources of PhD funding available to universities. (i) Akin to the standard research studentships of old (but less in total number) there are *doctoral training grants* made to a university based on the totality of its EPSRC funding. (ii) Much funding is now concentrated into *doctoral training centres* awarded competitively in priority areas of science (such as nano-materials, photonics, etc.), to what are perceived as deserving university research groups. Each such centre might be offered funding for, say, 10 doctoral students a year for a cohort of students to do effectively a 1-year master's degree followed by a regular PhD.

My colleague Lawrence Virgin (2012, personal communication) writes about the comparable funding circumstances in the USA:

> PhD students in the USA are supported in a number of ways. Government scholarships are available from the NSF (National Science Foundation) and NDSEG (National Defence Science and Engineering Graduate Fellowship). They are prestigious and highly competitive. Some overseas students benefit from scholarships from their own countries. In the basic sciences there are large training grants—these support a group of (say 10 or 12) PhD students for a couple of years and enables them to rotate through a number of labs before focusing on a faculty member's research. Many universities (like Duke) provide a fellowship to all first-year PhD students. After that students are expected to be picked up on faculty grant support. But in general (and certainly towards the end of the PhD) most students will be funded on a federal grant associated with a faculty member. I suppose there are some PhD students who are self-funded, but these are quite rare, I think.

(b) Your supervisor and thesis

So by one of these routes, you will find yourself working with a supervisor, who might be a lecturer, a reader or a professor of the university. Throughout the 3 years, you will probably work very closely with your supervisor who will guide you in your research (to a greater or lesser degree), and then help you in the writing process. So the supervisor is a key person in your life. You should take care to choose a supervisor (if you have the choice) with whom you really click. As his or her 'research student' you will be working closely and intimately together, and a good relationship is undoubtedly needed. When I was a research student at Cambridge, the only two talented people (that I knew) who failed their PhD examination had both had a big row with their supervisor. Your supervisor will not be one of your examiners, but might play a part in choosing them. In any case, upsetting your supervisor is not a good idea. One feature that is becoming common practice at universities is for a research student to have a second supervisor who keeps a general eye on progress. This could sometimes be useful, but smacks a bit of 'research by committee' in a sort of over-the-top 'health and safety' manner.

Under the guidance of your (main) supervisor, the idea is that you will do research for 3 years, including the last few months when you yourself will be required to write a report, technically called a thesis or dissertation. This must describe what you have achieved in the way of new and original discoveries, and what conclusions you have drawn. It may be up to 250 pages in length (practically a small book), which will remind you that scientists cannot neglect the quality of their English, including its grammar. The thesis will be examined by two experts in the field of study (an internal examiner from your university, and an external examiner from another university or research centre) who will read the thesis and then interview you about it in the 'oral examination' (sometimes called a *viva*).

(c) Equipment and environment

Unlike when joining a company, or large institution, where there is already a high degree of organization, you will find that on starting a PhD you may be on your own as far as planning, executing and saving your work is concerned. This is, of course, the joy of research; you can work where you want, when you want and how you want. John Baker (later Lord Baker) used to say to his academic staff in the Cambridge Engineering Department 'I don't care where or when you choose to work, at home or at your college, so long as you do your job and give your scheduled lectures'.

So it is useful to give consideration straightaway as to how you tackle these issues. The need for good equipment at a university is obvious. But most researchers, certainly the dedicated ones aiming for the top, do a lot of work at home. Here, they should make sure that they have a good PC (maybe a laptop as well), an efficient printer, a fast and reliable Web link and some form of

electronic back-up. A quiet room and a desk will also help. Anyone who imagines working from 09.00 to 17.00 at the university, and doing nothing at home, is probably not cut out for research at all!

Now there are some, with a hair-shirt mentality, who take pride in announcing that they have a really old and slow PC, and an ancient shaky printer, if they have one at all; and their Web link is fairly dicey as well. I am afraid these folk are beyond my help, and they should skip this section!

The ones that I will try to influence are those who are watching their money carefully. But assuming that they are not completely strapped for cash, I would argue that buying good equipment is an excellent long-term investment. It will help to get an earlier promotion and rise in salary, which will soon outweigh the money spent. A professor earns quite a bit more, year on year, than a senior lecturer.

Another thing: for goodness sake learn to touch-type now while you are young and your brain is receptive. I never did this, and I have wasted a lot of time writing books and papers as I type, ploddingly, with two fingers. Nowadays, with a research student (or indeed with my son-in-law, Allan McRobie) sitting at a computer perhaps hundreds of miles away talking to me on the telephone, I might say, 'I guess we ought to find Avril's views about this from her latest paper'. As I am speaking, I hear fingers flying over a keyboard, and by the time I have finished my sentence the young whizz-kid says, 'Yes I've got it in front of me right now'. What a fast world we do live in! As a matter of fact, to help with the writing of this article, largely text, I have just bought a DRAGON software package so that I can dictate it into a microphone. For this paragraph, the software, which has been learning the remnants of my Yorkshire accent for a week or so, made only a single mistake.

Most serious researchers have devised their own special way of finding a time or place where they can study undisturbed, as I remember Tom Kane remarking after a keen game of tennis at Stanford. I forget his personal solution, but it was clearly effective; during his long career, Tom devised a new formulation of the Newtonian equations of motion that led to some of the world's best dynamics software programs. He received the D'Alembert Award of the ASME for his contributions to mechanics in 2005. Years later, when I asked Stephen Wiggins, now at the University of Bristol, how he found time to write so many books on nonlinear dynamics, he said he simply got up 3 hours before everyone else (which would certainly *not* suit me), and wrote a chapter before breakfast!

(d) Making bricks

When I was in my first year as an undergraduate at Clare College, Cambridge, a friend of mine from the Hull Grammar School, one Leslie Boxell (sadly now deceased), wrote a letter to me. This was the age at which finding a partner was very much on every student's mind, and he said that when I found one I would be 'dependent on the love of a goddess, and not on a mind shearing through the bonds of ignorance. I would see the whole of human knowledge in one flash of intuition, but I would have lost the ability to make bricks'. Luckily, I never did lose the ability to make bricks, and am still making them.

The bricks under discussion are those modules of secure knowledge and technique that a conscientious student constructs during his or her undergraduate studies, and even more so during a research career. These modules do of course have a varied and non-trivial internal structure, unlike ordinary clay bricks; but having emphasized this, I will continue to call them 'bricks' which does invoke the concept of 'building knowledge'. A researcher can of course adopt a size of brick that is convenient and manageable, within his or her style of working.

I have always regarded everything that I have learned in research as being on a much firmer basis than anything else in life that I 'know'. This was revealed to me when I was in mid-career at UCL during a research discussion, when somebody asked me a question about the concept of virtual work in mechanics. I said, with what I considered to be complete honesty, 'I don't know anything about virtual work'. Later, I realized that I was actually giving a course on it to the undergraduates, obviously drawn from my 'lower-order' understanding. Thinking back to

some of my acquaintances over the years, knowing what you *do not know* is perhaps even more important than knowing what you do know. As Confucius say, 'To know what you know, and to know what you do not know; that is knowledge'.

In research, these varied 'bricks' take many forms, starting perhaps with a carefully drawn diagram, and then with modules of carefully checked material. Do not just skate along, thinking you will check everything when you begin to write your thesis! A major 'brick' for a researcher of any age is the writing of a short paper (see §6*c*,*d*) and many students write at least one during their three doctoral years: this is a very good thing, and you should try hard to do it.

(e) Do not forget that tricky bit

Careful planning is particularly important when, as often happens, you have to leave a piece of your work to go over to something else for a while. It is vital to leave your current work (including lists of references, etc.) in good order. As the months and maybe years pass, it is very surprising just how much is forgotten.

I have always been rather conscientious about leaving instructions to myself in the form of what I think of as 'flags'. But I do come unstuck sometimes. Once, I went back to a pile of work that I had done a few years earlier, to find a flag effectively saying 'This has all been carefully checked except for the tricky bit, so be sure to look at that again before publishing'. Unfortunately, although my memory for things that I have myself done is usually rather good, I could not make any guess as to what the tricky bit was. So I was obliged to do a much more thorough check than I would have wished. I believe that the famous French mathematician Laplace (1749–1827) was caught out in a similar fashion. It seems that Michael McIntyre's lucidity principle (below) about writing for others should be applied with equal care when writing for oneself (my italics):

> The problem is to remember that your reader's or listener's mind isn't full of what your own mind is full of . . . A good rule of thumb, for most of us, is to *be about twice as explicit as seems necessary*.

Of course finding a flag that says 'all ready for publication' only occurs in happy dreams.

5. Snippets of advice

(a) Get your first equation right

This sub-heading may seem an obvious thing to say, but it is remarkable how many people seem to come unstuck. So I emphasize:

> Research is not like an undergraduate examination question where you might get 8/10 for a good try, despite that little slip at the beginning! You have to get 10/10 every time.

A theoretician is often going to spend several months, or even years, studying an equation, so it seems obvious that he or she will make sure that it is correct. Let me report a recent experience of mine.

A school student living in Cambridge, who was going to a top university in the autumn to study mathematics, asked me if I could arrange some vacation work for him. At the time, I was working on the forced nonlinear vibrations of a simple pendulum, exploring regions of chaos and their fractal basin boundaries. I had retired from UCL, so I arranged for him to go to a university where I had some new connections. The equation was very simple, being just that of a pendulum excited by harmonic forcing, and I naturally gave it to him. I even said to him before he left, make sure you get the starting analysis correct, otherwise you could waste a lot of time. He started work under the general supervision of a research student and was in constant e-mail communication with me telling me his results. But as weeks progressed, it became clear that his results were not agreeing with mine at all. After about two months, I said 'Look, I believe you have got something

seriously wrong, please go back to the beginning and check all your working'. Well, as I imagine the reader has already deduced, he replied apologetically, so sorry, he had made a mistake on the first line (strictly, I suppose, the second line). He had differentiated cos x and got sin x. Some people just cannot be told.

Now while the 'first line mistake' is particularly stark, the moral of this story applies to all subsequent analysis. Like a surgeon, you have to strive to be right all of the time. On the positive side, you do have (unlike in an examination) plenty of time to make appropriate checks.

(b) Read: but not too much

Reading the literature is certainly important, but in science it can be overdone, so consult your supervisor. Disadvantages can be: it becomes a substitute for thinking things out for yourself; you get mesmerized by the accepted view; you can feel overwhelmed by the work of 'giants' and feel inadequate or just give up altogether. In his aforementioned book, Hudson [2, p. 31] talks about his early post-doctoral research on devising aptitude tests for the arts and sciences:

> Millions of research hours had been devoted to this problem of 'differential aptitude' before I learnt of its existence. Happily, though, my ignorance of the literature was complete: if I had had even a smattering, I should certainly have tackled something else.

In my own early days, there was a similar situation with regard to the monumental thesis of Warner Tjardus Koiter at Delft [9], which appeared, written in Dutch, immediately after the war in 1945. It was not until 1967 that it was translated into English by NASA: and very recently a set of Koiter's lecture notes were published [10]. As I wrote in 1973 in the preface of my book with Giles Hunt on *A General Theory of Elastic Stability* [11]:

> For several years the first author was blissfully unaware of the classic dissertation of Prof W. T. Koiter which had surprisingly lain largely unknown since 1945 and has in fact only recently been translated into English by the *National Aeronautics and Space Administration* of America. This was indeed most fortunate since the weight of Prof Koiter's contribution could well have discouraged him from proceeding with his own development of the subject. As it transpired, the full significance of Prof Koiter's work has filtered slowly into our consciousness in a gentle stream, moderated by the Dutch language and by our temperaments which have invariably preferred to explore the field for ourselves. This having been said, we must nevertheless hasten to admit our deep indebtedness to Prof Koiter's work, which we hope is adequately acknowledged in the text.

Koiter later referred to these remarks [12], writing 'at University College London . . . a similar approach for discrete elastic systems was developed more or less independently, as described so eloquently in the preface by Thompson and Hunt in their monograph'.

It was, in fact, when I submitted my paper on the basic principles of elastic stability [13] in 1963 to Rodney Hill at Nottingham that he drew my attention, for the first time, to the work of Koiter, as can be seen in the reproduced first page of his reply in figure 4.

In §5*c*, we look at another significant aspect of the scientific literature . . . it inevitably contains errors!

(c) Read: but do not always believe

Perhaps the most important thing that I should say about the literature is summarized in the motto of the Royal Society as *nullius in verba*, which roughly translates into *take nobody's word for it*. There is a fair amount of bad, erroneous and downright mischievous material published in journals and books, so you must be on your guard and develop your own critical faculties.

Under the adjective 'bad' will be low-quality theoretical work using over-simplified models, experiments with inadequate checks and controls, use of computer codes for stress analysis or fluid flow for problems lying outside their range of applicability (see §5*f*). Under 'erroneous'
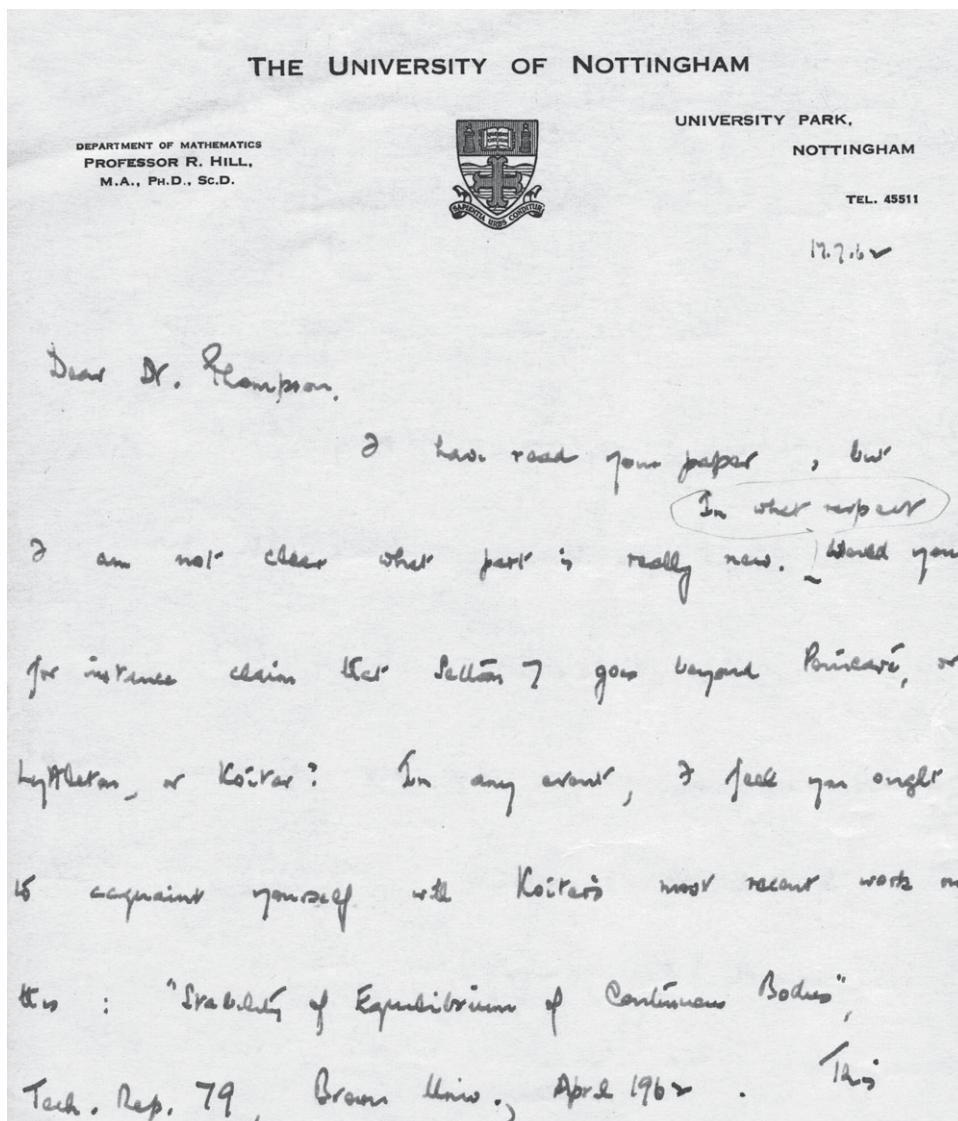
Figure 4. A letter from Rodney Hill, FRS, editor of the *Journal of the Mechanics and Physics of Solids*, in which he drew my attention to the work of Koiter. One of the joys of *JMPS* in those days was getting a hand-written reply the next day.

will be simple numerical errors in a theoretical analysis (which even a conscientious referee could never hope to spot), or not realizing that the 'pure' laboratory ether has been routinely contaminated by the night cleaner's duster (as happened at UCL). These things can happen to anyone, and it is worth remembering the two massive errors made by NASA (the epitome of rocket science!) over the years.

The Hubble Space Telescope (figure 5) was launched into orbit by a space shuttle in 1990. Unfortunately, there had been errors in the grinding of its primary mirror, including (among other things) a simple human goof of the 'upside-down insertion of a precision measuring tool into an optical system that guided mirror grinding'. This was a costly mistake of immense proportions in terms of time and money.

Eight years later, in 1998, the Mars Climate Orbiter, a robotic space probe costing 125 million dollars, was launched by NASA to study the Martian surface and atmosphere. Disaster struck in September 1999, when ground-based computer software erroneously produced output in
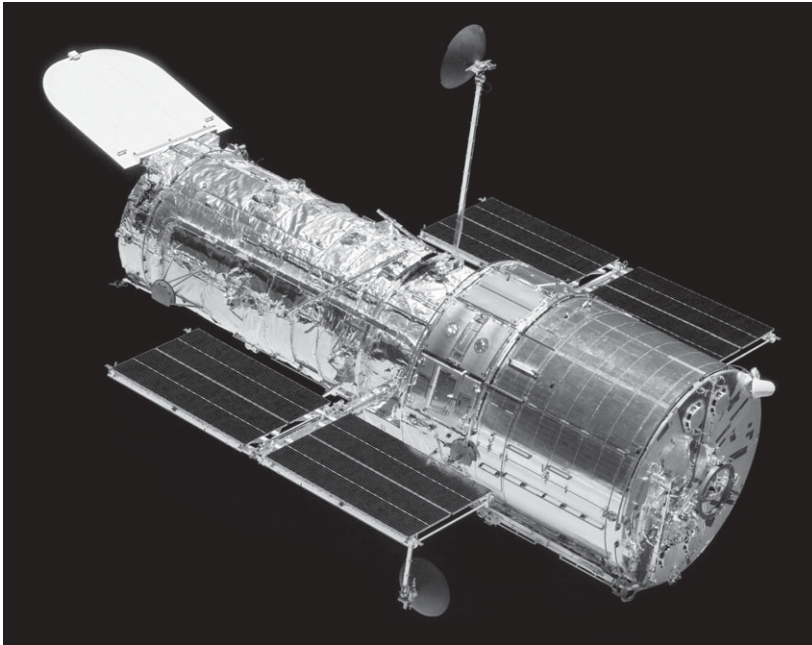
Figure 5. The Hubble telescope which needed expensive in-flight repairs due to errors made in the grinding of its massive primary mirror. Photograph reproduced courtesy of NASA.

the English unit of pound-force instead of the required metric newton. As a consequence the spacecraft approached Mars on an incorrect trajectory, entered the upper atmosphere and disintegrated.

So when you have a sickening feeling in the pit of your stomach as you realize that all the data you acquired yesterday were flawed by fitting the wrong calibrator, take comfort that it is only your supervisor that you will be confessing to in 5 minutes' time, not a Presidential Inquiry!

As you have seen, errors are made by the best of us! Rest assured, though, that the bulk of the scientific literature is reliable, especially if you choose the best authors writing in high-quality journals. These authors invariably find interesting ways to check their work, for instance, by noting that a problem can be viewed from more than one angle, or by using an additional independently written computer code; and they tell their readers what checks were done. Your supervisor will also be a useful guide.

(d) Synergy not secrecy

It is vitally important to talk about your research to, one might say, anyone who is prepared to listen. A casual, top-of-the-head reply from a lay person who is barely listening can often trigger a sudden new understanding by the alert researcher. As also can a basically stupid comment from a student who has been standing too long at the bar! You could also talk to your partner, if it is welcomed.

Synergy, where joint effort is greater than the sum of the several contributions is undoubtedly of tremendous value: and collaboration can be a great joy. Looking back at my own career, I started off as quite a loner and was the sole author of my first 13 papers. After that, there was an explosion of collaboration, which brought with it a lot of mutual excitement and just plain fun!

As with me, a major change to your own research patterns will develop when you have research students of your own. A lot of time will be spent talking with them, and often writing papers with them as well. This is a natural and welcome extension, which will allow you to explore simultaneously many new avenues of study.
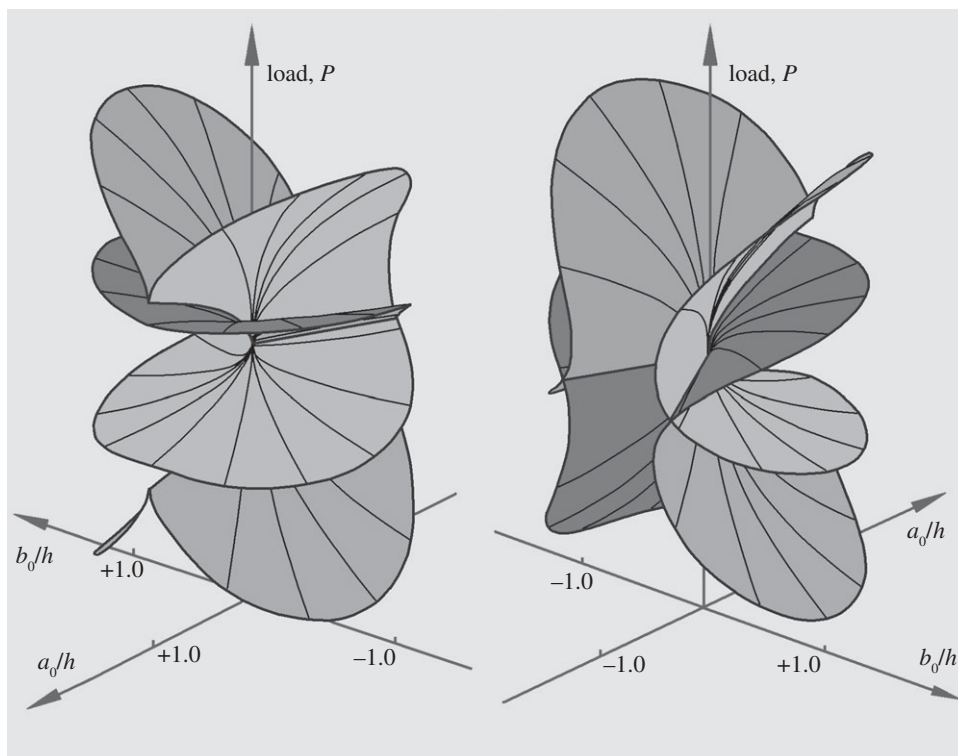
Figure 6. Imperfection-sensitivity surfaces for the interactive buckling of a stiffened plate calculated and drawn by Giles Hunt. We later learned that this was the first practical example of the hyperbolic umbilic catastrophe. Luckily Giles had, in curiosity-driven mode, drawn the top halves of the pictures which have no physical relevance for the stiffened plate. Here P is the load, a_0 is the overall imperfection, b_0 is the local imperfection and h is the plate thickness.

My extended partnership with Giles Hunt, a bearded hippie when he first joined me as a research student at UCL in the 1960s, was particularly enjoyable and fruitful; we eventually wrote two very successful books together. Two of his celebrated pictures (used on the cover of our second joint book [14]) are shown in figure 6. Subsequently, Giles had a distinguished career, with a spell at Imperial College London before ending up as a professor (now retired) at the University of Bath. In Bath, Giles and Chris Budd established a Centre for Nonlinear Mechanics, noted for its close links between engineering and mathematics.

Secretiveness in a scientist is regrettable and is always self-damaging. I remember encountering some such secrecy while I was a visiting Fulbright researcher in the Department of Aeronautics and Astronautics at Stanford University (1962–1963). Some of Nicolas Hoff's junior researchers were quite hush-hush about an explanation they had for the premature buckling of axially compressed cylindrical shells. I learnt, later, that by giving the cylinder a free (but loaded) end they had found a lower critical buckling load. Meanwhile, I was delighted to find that researchers at Stanford were using the method that I had pioneered in Cambridge as a doctoral student for manufacturing precision spherical shells by electro-deposition [15–17]. A photograph of one of their buckled shells is shown in figure 3a.

A particularly tragic (though often comical) trait of many young researchers is their illusion that everyone else is eager to steal their ideas and hurry off to do their research before they can. In reality, local colleagues are usually completely engrossed in their own research and would not dream of jumping in; though presenting a new unpublished idea at a big international conference (perhaps with no published proceedings) might be a bit risky.

Scientists who are too cagey or suspicious to tell their colleagues anything, will soon find that they learn nothing in return. I have always told others everything that I was doing and planning

to do, and sometimes even suggested things that they might like to try. But nobody has ever 'stolen' any of my ideas. They invariably had their own agendas.

While on the topic of discussions with others, you should nevertheless keep well away from anything approaching 'research by committee', which is a recipe for disaster. The classic anecdote about 'committee' versus 'free exploration' concerns Michael Faraday (1791–1867), who discovered the principles of electro-magnetism that led to the widespread electrical technology that we use today. He was eventually appointed the Fullerian Professor of Chemistry at the Royal Institution of Great Britain. Early in his career, he was obliged to work with a joint committee of the Royal Society and the Board of Longitude on improving the quality of optical glass to promote the accuracy of navigation at sea by providing better telescopes and sextants. It was not until Faraday was able to break away from this work on lenses that he was able to work in a free open-ended manner (*curiosity led* as we would now say); and very soon he had established the principles of electro-magnetic induction leading to motors and dynamos.

(e) Discrepancies: learn to love them

To a mature, well-educated scientist a discrepancy means an opportunity. It shows there is something new to be discovered. Indeed, if we read books on the philosophy and methodology of science, we are told that having developed a theory the researcher will devise crafty hard-hitting experiments specifically designed to test the theory to its limits.

This could not be further from the mind of the typical, young, anxious research student just about to write a PhD thesis. To this student (as I well remember), the appearance of a discrepancy will send a shudder down the spine, and possibly induce quite a panic. Between these two extremes, we must try to find a balanced view of discrepancies. However, we should acknowledge that *most* discrepancies will indeed point to an error somewhere and have nothing at all to offer in the way of a new phenomenon!

I recollect two occasions in my own career when a 'discrepancy' was ignored, thereby delaying the discovery of something exciting and new. The first arose when I was studying unexpected sub-harmonic resonances (figure 7) exhibited, in wave-tank tests, by articulating towers which are used by the oil industry for the offshore mooring of tankers.

We modelled the system as a mass restrained by an elastic spring, which had a discontinuity in its stiffness: this discontinuity corresponded to the mooring line between a tanker and its tower becoming slack during excitation by ocean waves. As an extreme case, we sometimes represented the sudden tightening of the line as an impact. Simulating the system on a digital computer, we were intent on plotting the amplitude of vibration of the tower versus the frequency of the ocean waves. My research student was plotting these curves and getting nice smooth sub-harmonic resonances, essentially what we were looking for. But between the resonances, where response amplitudes were relatively low, the graphs went all fuzzy. My student tried repeatedly to overcome this by carefully checking all his programs, but without success.

So, for the time being we passed over this 'little glitch', just leaving gaps in the curve where our computer simulations were seemingly giving unreliable results. It was later, when I turned back to this issue, that I spoke to mathematicians in Christopher Zeeman's group at the University of Warwick. David Rand was particularly helpful, and we realized that what we were seeing were chaotic motions of an impact oscillator [19,20]. In those days, mathematicians were excitedly exploring and delineating *chaos* (as it was called), while most engineers were totally unaware of the existence of these unpredictable motions. Indeed, it was my subsequent book on *Nonlinear Dynamics and Chaos* in 1986 that introduced the new ideas to engineers and scientists around the world [18]. The book was translated into Japanese and Italian and had worldwide sales of 14 000 copies.

Meanwhile, the reaction of some senior engineers, when I spoke to them about chaos theory, was to give a big snort and say 'load of nonsense'! A rather similar reaction had, indeed, greeted my earlier work on catastrophe theory [21,22]. So be warned, you must stick to your guns if you discover, or even use, something new. Remember that journal referees may be much older, fixed
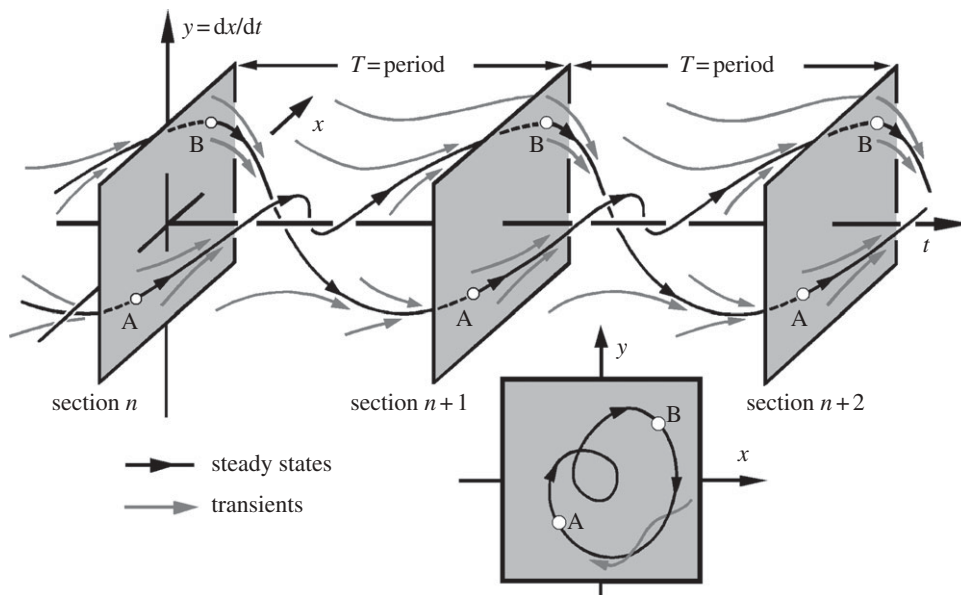
Figure 7. An illustration from Thompson & Stewart's book [18] showing stroboscopic Poincaré sections of a periodically driven nonlinear oscillator, illustrating a steady-state sub-harmonic motion of order $n = 2$. I spent quite some time drawing this figure, and it is rewarding that it has been reproduced (with permission) by quite a few researchers.

in their views, and often rather conservative by nature: if one of your papers is rejected by one journal, be sure to send it off immediately to another journal, thereby getting (hopefully) the views of different referees.

A second example of an apparent discrepancy came some years later. We were looking at the jump to resonance of a softening elastic structure under harmonic excitation, as a model for the capsizing of a ship. Here, as we hold the magnitude of the wave-excitation constant while slowly varying its frequency, there is a jump to resonance at what we would call a 'cyclic saddle-node fold'. The state to which the system jumps could in principle have a finite amplitude of vibration as a harmonic or as a sub-harmonic of order 3, or a very large (theoretically infinite) amplitude. At the time, I was under the firm conviction that a given system, with given parameters, would jump to one or other of these states, whether in a computer simulation or an experiment. But my research student found that in his computer simulations the jump went sometimes to one solution, sometimes to another. This issue was not central to our study (the ship would have capsized anyway!), so assuming that there was a glitch in the computer program, we looked no further.

Several years later, having learned about fractal basin boundaries from my collaborations with Bruce Stewart (Brookhaven Laboratory, USA) and Yoshi Ueda (Kyoto University, Japan), I revisited the problem with a new research student, Mohamed Soliman. We realized that the jump is indeed unpredictable, depending with infinite sensitivity on the precise manner in which the jump is initiated. This is possible because the critical fold sits (quite typically and generically) on a fractal basin boundary, as illustrated in figure 8. This new finding was quickly published in the *Proceedings of the Royal Society* as 'Indeterminate jumps to resonance from a tangled saddle-node bifurcation' [23].

The most remarkable example of a Nobel Prize-winning discovery arising from an apparent 'discrepancy' is undoubtedly that of Arno Penzias and Robert Wilson [24] in 1965. These two research engineers at the Bell Laboratories near Princeton were trying to clean up the reception of one of their big radio receivers, but had hit a problem. They had cleaned everything, and from inside the horn of their giant receiver they had even removed nesting birds and scraped off pigeon droppings. But despite all their efforts, there remained a persistence level of noise-like
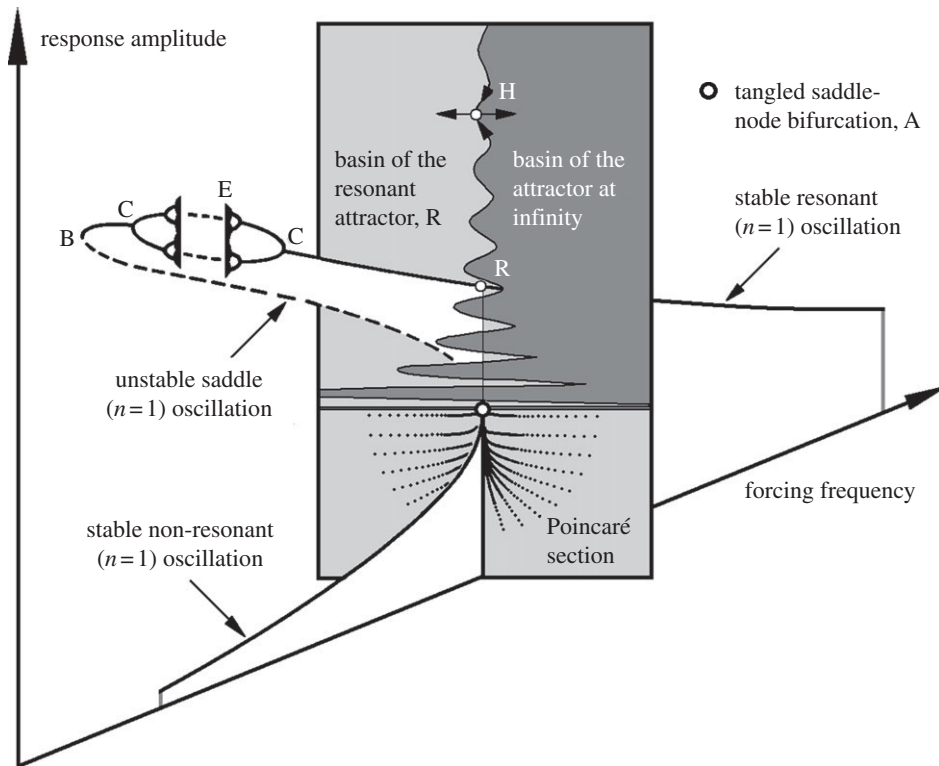
Figure 8. A schematic three-dimensional sketch illustrating the structure of the tangled saddle-node bifurcation. The basin boundary is the tangled inset of the hill-top saddle, H. This accumulates on the saddle-node at A. The indeterminate jump from A may: (i) settle on the stable, period-one, resonant attractor, R; or (ii) settle on a fugitive, small-basin, period-three attractor (not shown in the sketch); or (iii) escape from the potential well to infinity.

'interference', seemingly uninfluenced by where the horn was pointing; and they meticulously noted down its characteristics. Meanwhile, quite close by, the academic team of Robert Dicke were actively searching for a cosmic microwave background radiation which, it was thought, would be the afterglow of an ancient event, the Big Bang. On telephoning Dicke to seek his advice on cleaning up their unwanted 'noise', Arno and Wilson were stunned to discover, after feverish discussions, that it was they who had discovered the 'echo' of the Big Bang, key evidence for an expanding universe! They had stumbled across what the dedicated team next door was actually looking for, and duly collected the Nobel Prize in Physics in 1978.

The lesson of this section is do not ignore or hide discrepancies. You must learn to love and use them!

(f) Dangers of computer packages

Thinking about young readers in a *university* environment, I feel obliged to say a few cautionary words about the computer packages widely used for the stress analysis of solids and structures (including the buckling of shells) and the analysis of fluid flow, using for example finite-element techniques. These words apply equally to some of the nonlinear dynamics packages using finite-step time integrations. I accept that such general-purpose programs (usually commercial, sometimes provided freely by academics) are needed, but great care must be exercised when using them. Undergraduates in engineering now usually learn the underlying mathematics of finite-element modelling and may be introduced to solving problems using a commercial package such as ABAQUS; but the emphasis is mostly on understanding the basic principles.

The packages have usually been assembled over many years by (teams of) top researchers but inevitably contain, deeply and at every level, myriads of inbuilt assumptions and approximations. In the best instances, these 'hidden limitations' may be summarized in a necessarily massive handbook, but reading (and understanding) this may be quite impossible for a relatively inexperienced and unskilled first-time user.

Certainly, the first thing that you, the user, should do is to check the package out against a known bench-mark solution of a problem which has features closely similar to the one to be studied. Then you should try to find a novel way to check your work, by (say) viewing the problem from a different angle. If at all possible, you should repeat your calculation using a totally different package.

To give different points of view and balance to my 'academic' judgements, I give the two views from engineering consultants. The first is from Eilif Svensson (ES-Consult, Denmark) who spent some time with us in the UCL stability group in 1971, and writes in a personal communication (2012):

> Advanced programmes contain hidden assumptions (and semi-hidden in mediocre manuals). Apart from this the user himself has to decide on important assumptions such as boundary conditions—a fact that even the best programme package cannot compensate for through complexity (a great number of degrees of freedom) offering a perception of correctness. In that case the users own simpler, carefully drafted, model may yield better results. Another issue which annoys me from time to time is the unreflecting acceptance of codified provisions. Euro-codes offer an example of this. The physical backgrounds of many rules are obscure or absent and users apply the rules as blind recipes without questioning the context and hidden assumptions *'because then nobody can blame me'*. Rules are for the obedience of fools and guidance of wise men.

Secondly, Prof. Rod Rainey, head of technology (floating structures) at W. S. Atkins plc, writes in a personal communication (2012):

> The computer packages provided freely by academics are now about 1% of the market, and the commercial packages 99%. ABAQUS, for example, is the most widely-used nonlinear finite element package in the world—I don't know how big the support team at the software house is, but I would guess at least 1000. The number of users worldwide will be at least 100 000. It is the standard package used by both Boeing and Airbus for their crashworthiness work, for example, in which you seek to ensure that in a crash landing, the undercarriage pushes up through the wings to absorb energy, and the engines come off—all without rupturing the fuel tanks in the wings. This is all mega-nonlinear of course—lots of plastic buckling etc. And it all works amazingly well.
>
> In structural engineering design, which is a very different thing from research, of course, my own view is that poor designers waste a lot of time with trial-and-error design methods on big computer models, producing very complicated designs. And good designers don't— they use simple and elegant computer models to design simple and elegant structures.
>
> There are loads of empirical parameters in computer packages, to be sure, but the skill is to know what they are and what they are doing. That is what a lot of young engineers [in industry] spend their time learning. After 10 years of doing nothing but running ABAQUS, they become very competent indeed (assuming they are very smart and well-educated to begin with—that is important), know where all the 'rocks in the harbour' are, and steer a safe course though them. A beginner, of course, can still produce ridiculous answers!

(g) Motivation: just do it

Years ago, when I had a big personal decision to make, I was aware of cars driving around town with a sign in the window saying 'just do it'. I now realize that this is the motto of the sportswear company Nike, and I find that I have adopted this slogan, as a way of jolting myself into action.

Figure 9. Enthusiastic A-level students launching rockets that they have designed and built during a one-week residential course at the Villiers Park centre in Foxton. Small groups of talented students are given lively lectures and projects in their chosen subject (maths, electronics, drama, space, etc.), often by research students drawn from UK universities.

I particularly remember talking to Jim Croll in the early days of our stability group at UCL, standing in the balcony where the old photo-elasticity bench had stood. We were discussing optimal design, and its link to imperfection sensitivity in shell buckling. The idea was that an optimal, minimum-weight design would always be associated with compound failure. I remember thinking, at the end of this hand-waving conversation, 'Oh for goodness sake why don't I just quantify it? The result was a paper [25] which attracted quite a lot of interest.

More recently, Ian Gaseltine at my badminton club posed a little puzzle as follows. Hammer one nail a little way vertically downwards into a block of wood. Can you now balance 12 nails on this fixed nail? You are allowed nothing in the way of string, tape, magnets, etc. The 12 nails are circular in cross section, and must not touch the wood. The solution works well with two-inch nails. He assured me that this is not a catch or trick question!

After just a little thought I decided that it seemed impossible, and each week at badminton I said 'Come on Ian, tell us how it is done', to which the reply was always the same: 'No you've got to solve it yourself'. So finally I said to myself, 'Oh come on Mike, you're supposed to be an engineer! Just solve it'. So I got nails and a block of wood and spent an evening playing about with them, but no joy. But then in the morning, I suddenly solved it . . . very rewarding! Once made, the structure is remarkably stable, and the block can be carried around the room without mishap. I have given the answer in figure 1b, applying my own principle (see §6d) that most readers only look at the figures anyway! In a recent e-mail, Marian Wiercigroch (see §6e) tells me that his son, Michal, has risen to my challenge by balancing 30 nails in this manner!

So if you are temporarily stuck in your research, try giving yourself a mental jolt, which can work wonders. Figure 9 shows a little jolt being given to bright A-level students at the Villiers Park Educational Trust in Foxton, where I now live (near Cambridge). This Trust works with

high-ability students from all backgrounds and has had considerable success in facilitating fair access to leading universities. I took this photograph in my capacity as a voluntary worker at the centre during one of their 5-day residential courses, at which groups of students have stimulating lectures and workshops often given by research students from UK universities.

6. Presenting your work

(a) Draw good figures

I have always enjoyed drawing good and clear figures that display ideas clearly and precisely, as I hope do some of my figures reproduced in this article. I found this extremely useful, as a way of building well-defined 'bricks' of knowledge, particularly important to me because I tend to think in a very visual, and graphic way. So I formalized the whole system and give my figures reference numbers. These figures are then always available for lectures, papers and eventually books. In the early days of 35 mm slides, I accumulated box upon box of these slides. I still have them, and cannot quite bear to throw them out! Then at one point, I shifted to overheads, and later to PowerPoint presentations. I remember distinctly when I decided to change from slides to overheads.

Giving a plenary lecture in a German university, which was very proud of its high technology, I was introduced, before my lecture, to their magnificent new computer-controlled slide projector. Would I like, I was asked, to have either (i) a slide just vanishing and the next one appearing, or (ii) a slide gliding off the screen, in a direction of my choice, followed by the next one gliding in, or (iii) slides just gradually fading in and out, etc. I said all I want is just one slide after the next (but perhaps they took this too literally, as we shall see).

At one point in the lecture, I wanted to return to the previous slide and said, as one does, can I go back to the previous slide please. There was a long pause. The screen went blank, and remained blank for a long, long, time. Professors were rushing about, heads one imagined were being slapped, until finally they succeeded in getting back to the previous slide. I must have been very relaxed that day, because I remember being quite amused by the whole thing. Then, later in the lecture, I again wanted to step back to a previous slide. I decided to try again. Imagine my increased amusement when the whole pandemonium was exactly repeated. This was a time when academics were slowly changing from slides to overheads, and I thought perhaps I should join them.

The essence of drawing a good figure is to get as much information as possible onto the screen. This may mean sacrificing artistic elegance by packing things together fairly tightly, and without a 'decorative' border around the image. If you are displaying lines of text or equations, do not imagine that you should leave big spaces between the lines; this will just waste space. Finally, please do not arrange for the audience to see only one line at a time. It is much more helpful if viewers can occasionally run their eyes forward to see what is coming: as we do naturally when reading a book.

(b) Seminars and conferences

A group of any size in a university will invariably run a series of seminars (sometimes called colloquia) in which researchers speak about their latest findings. Some speakers will be outside specialists, invited from other universities, institutes or industry, while some will be internal academic staff, including research students. These offer a wonderful opportunity to hear what other people are doing, and soon you will have the opportunity to give one yourself. Planning for this will be a tremendous spur to organizing your material and is a good precursor to writing a paper on the same topic. Feedback from the audience can be of great benefit. The seminars are very informal, followed by coffee, etc., and are wonderful occasions to meet colleagues old and new. As you become known, you will certainly be invited to give talks at other universities as well.

The next step will be to attend, and make a presentation at, one of the many conferences (sometimes called symposia or workshops) that are organized all over the world. Usually, you can get at least some of your travel and subsistence expenses paid by your university, or a research grant, etc. This is where you will get to know everybody who works in your area and join an informal 'international college' of researchers. There is a continuous exchange of e-mails and papers within such a community making the scanning of current journals almost unnecessary.

Remember that when speaking at meetings the aim is to inform your audience by presenting your work in a clear and simple way. Simplicity will impress, unnecessary complexity most certainly will not. You should aim to illuminate, rather than to dazzle. Finally remember the simple lecturer's rule: say what you are going to do, do it and then say what you have done!

(c) Writing a paper: why

For researchers of all ages, the preparation of a paper for publication in a learned journal has tremendous benefit to the writer, quite apart from informing others and assisting in the building of a good curriculum vitae. Seeing your own work in print is a very rewarding experience, and your head of department will be delighted to have an extra paper for the next government research assessment (Research Excellence Framework and beyond). Meanwhile, writing the paper will involve carefully checking the material, writing it up in a precise and readable way, and generally becoming very familiar with it. This familiarity is a superb foundation for proceeding to the next stage of your research. Even the often tedious business of dealing with referees' comments (in what is called the peer-review process) and correcting proofs allows the details to sink more deeply into the brain. Another good reinforcement comes from giving seminars as discussed in §6*b*.

You should get into the habit of writing papers as soon as you have accumulated enough new material; and many have observed that it is easier to publish a short, concise paper than it is to publish a long and grand *magnum opus*.

Without this frequent writing of papers, a researcher may well be left after some years with piles of unchecked, unorganized material, which is in many senses lost both to the individual and to others. Indeed, the writing of papers can be viewed as a professional duty, since the pay of academic staff is geared to the fact that at major universities they are expected not only to do research, but also to publish it.

(d) Writing a paper: how

It is not my intention to deal in any depth with the wide subject of scientific and technical writing, about which many books have been written. Two recommended works are by Zanders & Macleod [26], which is short and jokey, and Doumont [27] which is a heavier read. Another excellent source of advice, specifically for the writing of papers in mechanics, is the paper by Villaggio [28]. Here, I just give, in the manner of the present article, a few tips drawn from my own experiences. Always bear in mind, though, that a key concept of science is that a publication should contain enough detail to allow a reader to repeat (and hopefully verify) the results independently; assuming that the reader has access to the necessary equipment, which usually will not include the CERN accelerator!

The first point that I would like to make is that hardly anyone, possibly no one at all, is going to read your paper systematically from beginning to end. In saying this, I am reminded of the following extract from James Boswell's *Life of Samuel Johnson* [29] first published in 1791:

> Mr. Elphinston talked of a new book that was much admired, and asked Dr. Johnson if he had read it. Johnson: 'I have looked into it.' 'What,' said Elphinston, 'have you not read it through?' Johnson, offended at being thus pressed, and so obliged to own his cursory mode of reading, answered tartly, 'No, Sir, do *you* read books *through*?'

A normal busy scientist will look at the abstract, possibly the introduction, and then the conclusions; and significantly he or she is likely to look through the figures. This must inevitably influence the way you write and organize your material. It is no good thinking that, having defined a mathematical symbol on page nine, the definition need not be mentioned again. Quite a bit of repetition will help the reader a lot. In particular, it is a good idea to have a comprehensive caption for each figure in which all the symbols are given their full names and the meanings of the graphs and diagrams are fully explained, without the reader having to wade laboriously through the text.

Do not just say, 'we have plotted α against β with $\gamma = 3$'. Rather say, 'we have plotted the load parameter $\alpha = PL^2/\pi^2 EI$ against the non-dimensional deflection $\beta = d/L$ while holding constant the aspect ratio at $\gamma = r/R = 3$ to show how the load increases gradually with the deflection after buckling'. Looking through an issue of a high-quality scientific journal, I found the average number of words per caption (covering seven different authors) to be 80, which is about right.

Next, you must learn to call a spade a spade. As a Yorkshire man, 'born and bred' as they say, I find no difficulty in doing this. Though I did just hesitate when, as a doctoral student, I was writing my second paper [15] entitled 'Making of thin metal shells for model stress analysis'. This described how I was making wafer-thin complete spherical shells, without seams, by depositing copper electrolytically onto a rotating wax sphere, and then melting the wax out through a minute hole. Would a scientist just say 'The liquid wax was finally driven out by means of boiling water, which was forced into the sphere down a hypodermic tube'?, I asked myself. I decided that they would, wrote those precise words, and never worried about that sort of thing again!

The point I want to make here is that if you were writing a book about gardening, it would be perfectly natural to keep using the word *spade*; you would not want to say fork, incorrectly, just to stir things up. Now in literature, and in the minds of some scientific copy-editors, there is a general feeling that you should not repeatedly use the same word. Having referred to the 'gravity of the sun' controlling the planets, you should perhaps, following the heroine of *Cold Comfort Farm*, next refer to the 'gravity of the golden orb' [30].

Well this sort of variation is usually bad in science, as emphasized by a number of distinguished writers. Repetition can make for clarity, as Michael McIntyre, Fellow of the Royal Society (FRS), illustrated in his article on 'Lucidity and science' [31, p. 200] with the following (my italics show the valuable repetition):

> **Example 1.** Whereas the spectral method engenders Gibbs fringes, no discretization oscillations are manifested by the TVD algorithm.

The writer meant:

> **Example 2.** Whereas the spectral method *produces Gibbs fringes*, the TVD method *produces no Gibbs fringes*.

We can imagine how a beginner to the field would be totally confused by example 1, where the simple meaning is totally camouflaged. Unfortunately, one sometimes suspects that some devious writers actually *want* to make the story seem more complex than it really is.

This brings us to the vital need to keep things as simple as possible, to help both yourself and your readers. You should, indeed, give yourself every possible help. If this makes your current hard problem seem easy, it might correspondingly make the next very hard problem manageable. Keeping things simple applies, in the first instance, to choosing a good notation, where I will again quote from McIntyre [31, p. 200]:

> ...bad mathematical notation where four things of the same kind are written as a, M_3'', ε_2, $\Pi_{1,2}''$ instead of $a, b, c, d, \ldots$

I was, in fact, pulled up on something a little like this early in my career on the second page of Rodney Hill's aforementioned letter (figure 4), where he commented that my compact notation 'will give the printers (also) a headache'.

Finally, I must mention a style of showing off and sheer obfuscation that was prevalent when engineers started to learn about chaos theory, which involved reading some advanced mathematical books where definitions were quite important. A research student, imitating such books, would say (in obscure mathematical notation) that the time, t, is an element of the real numbers lying between minus infinity and plus infinity. Gosh! All this, and just while studying the oscillations of a driven pendulum. Please keep your level of mathematical precision appropriate to your problem.

(e) Building a research group

Research groups are on the whole rather mysterious things that seem to pop up, and then sometimes fade away, in times and places of their own choosing. Almost any university will have one or two sparkling groups, and they are certainly not restricted to the top universities. This is very clearly recognized by the Royal Society, which is why it always resists the current fashion for the concentration of research funding into just a few universities.

The deliberate start-up of a research group will inevitably require a core of talented researchers and a good supply of funds to attract more staff and students. I think the best I can do here is to say a few words about the three groups that I have been involved with during my career.

During my six post-graduate years at Cambridge, three as a research student at Clare College and three as a research fellow at Peterhouse, my supervisor Henry Chilver was appointed head of civil engineering at UCL (in 1961), and he attracted me to join him as a lecturer. Henry was a very talented and energetic organizer (a *remarkable man*, as John Baker once said to me at Cambridge) and quickly built up a superb group working on the stability of engineering structures. This Stability Research Group attracted, for example, Jim Croll (a New Zealander, who later became head of the department), Alastair Walker (who eventually took a chair at Surrey) and John Roorda (later a professor at Waterloo in Canada). These were heady days for us young researchers, and we probably overlooked the hard work that Henry had put in to establishing the group. Henry left UCL in 1970 to become vice chancellor of the Cranfield Institute of Technology, and I effectively inherited his group.

A high point of our activity was attracting to UCL, with the encouragement and support of James Lighthill, then Provost of UCL, one of the prestigious symposia sponsored by the International Union of Theoretical and Applied Mechanics (IUTAM) which brought together all the top researchers from around the world. The logo that I used is shown in figure 3*b*. The meeting, in 1982, was devoted to *Collapse: The Buckling of Structures in Theory and Practice*, and Giles Hunt and I edited the proceedings as a book with Cambridge University Press (CUP). It contains a significant early paper [32] by Isaac Elishakoff (seen in figure 10) which pointed the way towards a probabilistic theory of imperfection sensitivity. The appearance of a subsequent book on this was most welcome [33].

In 1985, I was elected an FRS, and at about the same time I was asked by the then head of department at UCL, Ken Kemp, to develop an undergraduate course in dynamics. Giving this course, my research interests drifted towards nonlinear dynamics, and in 1988, I was awarded a 5-year Senior Fellowship by the Science and Engineering Research Council (SERC). This gave me the time and impetus to build up a new group at UCL as the Centre for Nonlinear Dynamics and its Applications. This was strongly supported by the Marine Technology Directorate, and a grant from the Wolfson Foundation brought total earnings to £1 million before the formal creation of the Centre in 1991. Awards since then brought the running total to £2 million.

A particular success of the Centre was the winning of three illustrious University Research Fellowships from the Royal Society. The first was awarded to Allan McRobie to work on topological methods for the dynamics of structures, and the second to Michael Davies to study time-series analysis using phase-space reconstruction. The third went to Gert van der Heijden to pursue his studies on the spatially chaotic twisting of elastic rods that we had discovered with Alan Champneys [34,35]. This work on spatial chaos, using the static-dynamic analogy [36], created an interesting link between the two groups and a seminal paper was by Hunt *et al.* [37].

Figure 10. Detail from a photograph taken at the IUTAM Collapse symposium held in London in 1982, showing three participants at the reception held under the UCL dome. On the left is James Lighthill, Provost of UCL; with (from left to right) Isaac Elishakoff and Joseph Singer, both from Haifa, Israel.

The pattern of bifurcations for a beam on a nonlinear elastic foundation is shown in figure 11. Meanwhile Steve Bishop won an Advanced SERC Fellowship. The Centre attracted an IUTAM symposium on Nonlinearity and Chaos in Engineering Dynamics to UCL in 1993 which uniquely brought together engineers, scientists and mathematicians. Jaroslav Stark, promoted to a chair at UCL in 1999, was founder and director of our MSc course. He moved to Imperial College, but sadly died at an early age in 2010.

Finally, I am today witnessing, as a part-time Sixth Century Professor, the building of a new dynamics group at Aberdeen under the energetic leadership of Marian Wiercigroch, the Centre for Applied Dynamics Research (CADR). This, too, is attracting big grants, including those for the development of resonance-enhanced drilling for the oil industry [39]. It hosted an IUTAM symposium on Nonlinear Dynamics for Advanced Technologies and Engineering Design, in 2010.

(f) The grant report: depth to simplicity

One thing that my mentor, Henry Chilver, always emphasized to me, relevant to engineers in particular, was that one should look at problems in great scientific depth and generality. But then it is important to come out again, and try hard to conjure up some simple ideas for the people in industry. The emphasis was always on the word *simple*. I found this advice particularly useful and relevant when writing final reports on engineering grants, which activity always focuses the mind amazingly, and with great benefit. I remember, in particular, struggling really hard when writing a final report to the Navy on a long-running grant about the capsizing of frigates in beam seas. My over-enthusiastic, and rather naive, research assistant said at the time 'won't the Navy be delighted and impressed by our discovery of the homoclinic tangling of the invariant manifolds of the escape equation'. I pointed out, as delicately as I could, that the man at the Navy would have no idea what we were talking about. We would be lucky if he knew anything about linear resonance, never mind the advanced ideas that we were exploring in nonlinear dynamics and chaos.
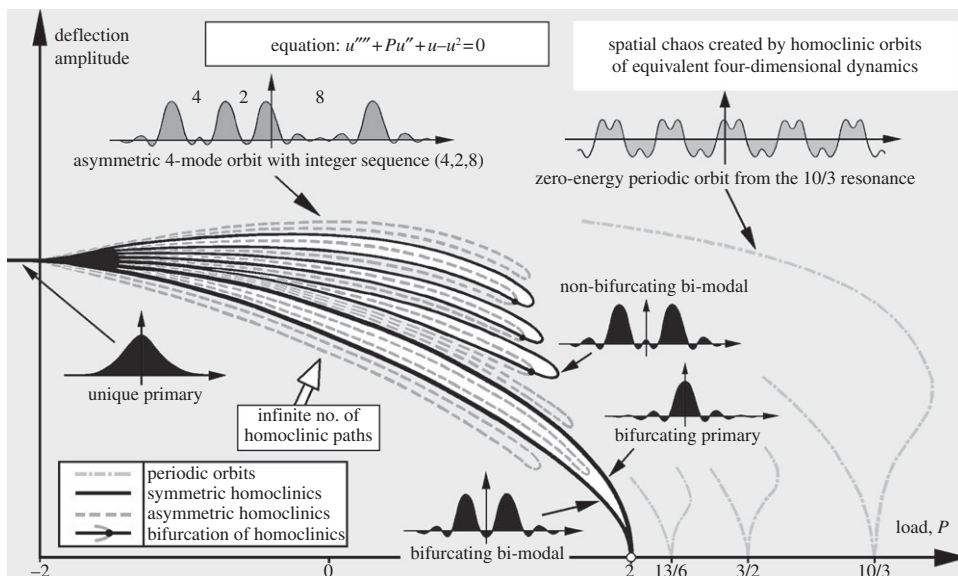
Figure 11. The complex spatially chaotic load–deflection diagram for an infinitely long strut on a nonlinear elastic foundation. Four complex eigenvalues give a saddle-focus at the origin for $-2 < P < +2$. The critical buckling load is at $P^C = 2$. An infinite number of homoclinic paths approach arbitrarily close to P^C. This is an archetypal example of the static-dynamic analogy. The underlying mathematical results are due to Buffoni *et al.* [38].
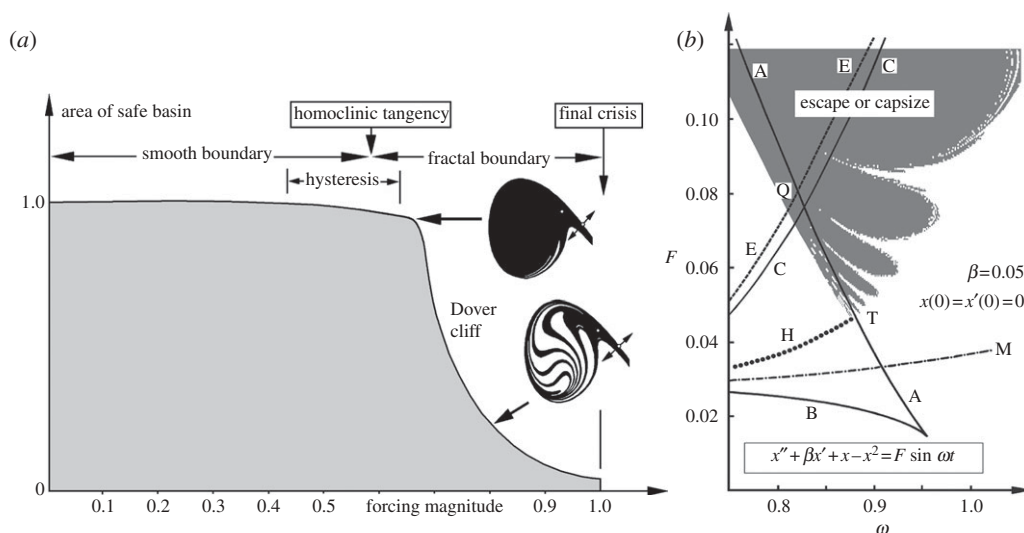

Figure 12. Two advanced concepts of nonlinear dynamics whose discoveries allowed the formulation of the simple practical idea of transient capsize testing. (*a*) The Dover cliff phenomenon in which there is a sudden fractal erosion of the safe basin of attraction; and (*b*) the associated fractal structure that appears in the control space of forcing magnitude versus forcing frequency.

In the event, under the pressure of writing the final report, and with Henry Chilver's guidance in mind, I did indeed come up with some good and reasonably simple ideas of *transient capsize testing* (figure 12) based on what I called the Dover cliff phenomenon of basin erosion [40]. I should add that during my research on articulated mooring towers and ship dynamics, I was greatly aided by having a first class running mate in industry, namely Rod Rainey (quoted in §5*f*), whose razor-sharp mind contributed greatly to the successful outcomes.

The concept of looking at problems not only in depth but also in *generality* deserves some elaboration. The capsizing of a ship is theoretically equivalent to the escape of a particle from a potential well, which has wide applications in physics, chemistry and engineering. So, with no loss to the Navy, I was able to cast my work in this wider framework. In fact my most cited paper (see §7 about citations) is on chaotic phenomena triggering the escape from a potential well [41].

(g) Importance of writing books

A book can be thought of as a solid structure built of many of our 'brick' modules, and indeed a series of papers can often evolve into a book. Like the bricks themselves, this structure helps writers to organize their material, and put it into good, preserved, order. Many researchers have said to me that they needed to write a book, even if just to keep their own files in order. This was definitely the feeling that I had when I wrote each of my four books.

One thing that I should mention, in a wider context, is the importance of (someone) writing a book when there has been a great explosion or breakthrough of research in a field. This is needed to clarify, codify and record the achievement, and it is important that one of the key workers should take it upon themself to summarize the new developments, preferably as a book or monograph. Luckily, in my case, I rather like writing books, so there was no problem there. However, when I look back at the advances in shell buckling in the early 1960s, I cannot help wishing that there had been an extensive and clear write-up of the deep theoretical progress that was made in imperfection-sensitivity studies. Unfortunately, key workers, such as Koiter at Delft and Budiansky and Hutchinson at Harvard, did not seem to be the book-writing types (I know that Koiter particularly regretted this). At the time, I would have known just who to consult about the particular shell formulation (von Karman, Donnell, Flügge, Sanders, etc.) needed to deal effectively with a given shell geometry and loading. But now I feel I would not know where to look, and more and more of the experts are, sadly, no longer with us. There are, of course, a lot of books on shell buckling, as can be seen (for example) on the comprehensive website created by Bushnell [42]. These include the insightful treatise by Chris Calladine [43], but none goes into quite the depth that might be required. Particularly worrying to me is the current reliance in shell buckling on commercial general-purpose finite-element programs, as I discussed in §5*f*.

When there is a big new discovery, similar to chaos theory, there is for a time a complete cacophony of noise and confusion from which a beautiful tuneful symphonic melody finally emerges. This symphony needs to be written in book form by one of the key workers. This has, if anything, been overdone in the case of chaos theory where there is a plethora of such books! In this context, I recall with pleasure encouraging Michael Païdoussis of McGill University to write a book on fluid–structure interactions, using the above arguments. He did [44], including a section on my 'magic box' [45], and later kindly expressed his gratitude to me for giving him the impetus. He obviously enjoyed the experience, because he has just written his third book [46].

Some of my research students ended up as prolific book writers. Koncay Huseyin, distinguished professor emeritus, was head of Systems Design Engineering at the University of Waterloo and wrote about his extensive studies of multi-parameter systems in three excellent books [47–49]. Koncay also started a new international journal in 1986, which still appears (with a change of name) as *Dynamical Systems*, published by Taylor & Francis; my colleague at UCL, Jaroslav Stark, edited this journal for some years. John Roorda wrote a valuable monograph describing the ground-breaking experiments that he performed with Henry Chilver at UCL [50]. Lawrence Virgin, professor (and recently head of department) in the engineering faculty of Duke University, USA, published two stimulating books with CUP [51,52], the first describing his unique and outstanding experimental investigations in nonlinear dynamics and chaos; his Nonlinear Dynamics Research Group at Duke has had a major impact on engineering dynamics. Giles Hunt wrote his second book with me in 1984, which included his exceptional and innovative work on interactive buckling and his elegant pictures (figure 6) of the hyperbolic umbilic catastrophe [14]. Steve Bishop collaborated with Tomasz Kapitaniak on a *Dictionary of Nonlinear Dynamics* [53]. Listing all these names reminds me of many (seemingly sunny) Sundays when I

played tennis in Regent's Park with Steve, Lawrence and Giles and his family; folklore has it that booking the court under the names of Bishop and Virgin was always a bit of a giggle.

7. How well am I doing?

(a) Citations and impact factors

Throughout your career, it is a good idea to consider how you are progressing, especially in relation to your colleagues. Of course, optimists will usually imagine they are doing better than they really are, while pessimists may take the opposite view. It can be very embarrassing, and lead to all sorts of difficulties, if your self-image deviates too far in either of these directions. This often comes into sharp focus when optimists apply (or imagine applying) for a job that is far, far beyond their abilities; or when pessimists dare not apply for an ideal opportunity because they fear, incorrectly, that they are not good enough. So try to maintain an objective view of your standing.

Luckily, with the Web, it is now very easy to observe *one measure* of your progress and impact by looking at the Web of Science (WOS), or an alternative such as Scopus or Google Scholar. You can access this freely through your university's subscription link. On this site, you can type in your own name (and those of your rivals or supervisor!) and see all papers published and the number of citations that each has attracted in the research literature. You can automatically sort the list by various criteria, such as 'by publication date' or 'by number of times cited'. Of course, WOS scans only those journals that it regards as internationally significant.

This raises one important point. It is a good idea to use all your initials, or at least a consistent version of your name, on all your papers. In my case, I always use my three initials J. M. T. before my surname (or an unambiguous variant such as J. Michael T.). However, in everyday life, I have always been called Michael; so on one occasion I did write a paper in an informal journal under 'Michael Thompson'. Of course, WOS now believes there to be two distinct people, 'J. M. T. Thompson' and 'Michael Thompson', and citations for these two people are provided in separate lists. This did not matter to me in this instance, but if you want to follow your citations (and allow prospective employers to see them) it is not a good idea to be giving alternative names. Women scientists need to give this careful thought if they get married during their career: some may prefer to keep their unmarried surname at least for professional purposes. Of course, if your full name is John Smith, everyone is going to have serious problems finding your data! One way around this is to register with WOS to obtain a ResearcherID number, which helps to identify you uniquely.

Citations of a given paper build up over time, but even if 2 or 3 years have passed since publication it could well be the case that one of your papers has attracted no citations. Do not despair! Even the best of us have one or two papers that, in a lifetime, have never been cited. Indeed the average number of citations per paper is actually surprisingly low.

This low value is best understood by looking at the impact factor of journals, also on WOS. The impact factor of a journal is the average number of citations per paper, published in a 2 year period, that were made in the literature in the following year. More specifically, it is the number of times articles published in 2007 and 2008 (say) were cited during 2009, divided by the total number of articles published by the journal in the same period (2007–2008). The result is the journal's 'impact factor for 2009'. This impact factor appears in WOS in 2010, because it cannot be calculated until all of the 2009 publications have been scanned by the indexing agency.

Now a typical good-quality journal in applied mathematics or engineering will often have an impact factor of about 1.9. So an average paper in that journal will have received, say, just two citations in the relevant year. I should add that impact factors (and expected citations) vary quite considerably between disciplines: biology and chemistry typically have much higher figures, pure mathematics much lower. So do not be disappointed if your citations seem low.

From the WOS page displaying your (or anyone else's) list of publications, you can click on the 'Create citation report' to get a summary and overview of your career, including two
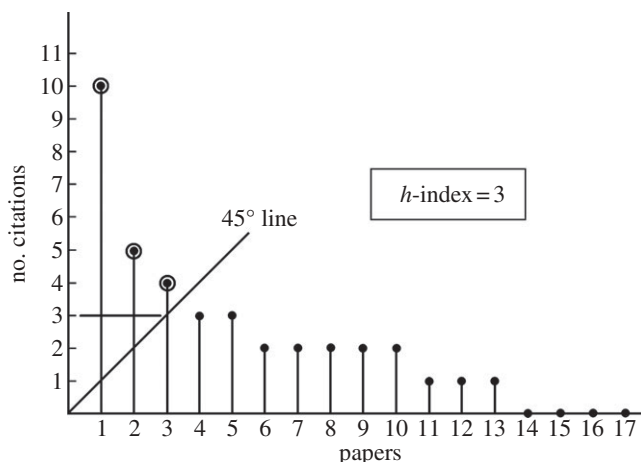
Figure 13. The notional histogram of a young researcher, Jane Smith, showing papers listed in order of decreasing number of citations (not chronologically). The drawn 45° line illustrates the meaning of the *h*-index, here equal to 3.

histograms of papers and their citations distributed chronologically over the preceding 20 years. Also displayed will be the following data, where the numbers included are entirely a figment of my imagination!

— *Results found:* 15 (the total number of papers that you have published)
— *Sum of the times cited:* 254 (the total number of citations to all your papers)
— *Sum of times cited without self-citations:* 244 (with 10 self-citations subtracted!)
— *Citing articles* 157 (those articles which have made the citations to your work)
— *Citing articles without self-citations:* 150 (with self-citing articles subtracted)
— *Average citations per item:* $254/15 = 16.9$
— *The* h-*index*: 3 (described in the following section)

Note that there is a facility for viewing the specific articles (by other authors) that have cited your work via your university's database links. Finally, there will be the list of all your papers with very comprehensive citation information about each paper. Sorting the papers by selecting 'Times cited—highest to lowest' you will find a horizontal orange line underneath one of them which corresponds to the *h*-index that I describe next.

(b) The *h*-index of solidity

Let us consider the fictional Jane Smith, a talented post-doc whose citation histogram is shown in figure 13.

This shows the citations of her individual papers, which are listed in decreasing numbers of citations. Jane may have more than 17 publications (30, say), but those beyond 17 have no citations. We want to assign a single number as a measure of the weight or solidity of her scientific contribution, which will give a valid comparison with that of her friend, Andrew. If we choose as our measure her total number of papers, 30, this would be unsatisfactory if Andrew had published fewer papers, but all were much more heavily cited. A better one would clearly be the total number of citations.

The *h*-index was devised by Hirsch [54] to give a good all-round measure of weight. In particular, he wanted to reduce the advantage of having just one very heavily cited paper, giving an enormous spike at the first paper of figure 13. This one paper might even have contained a fundamental error, which could have generated a host of critical citations! At the same time, he wanted to decrease the disadvantage of having a long tail of uncited papers at the right-handed end of the figure which would, for example, pull down the overall citations per paper ratio.

Table 1. A broad-brush correlation between citations and other achievements. Because the numbers vary dramatically between different fields of research, interested readers are encouraged to produce an equivalent table using WOS data for known individuals in their own fields.

	papers	cites	*h*-index
exceptional international researchers, leading cosmologists and biologists, etc.	213	27 579	76
top world figures (Nobel Prize, or President of the Royal Society, say)	172	19 112	51
distinguished professor at top university (FRS, or head of department, say)	163	4968	33
professor at a middle-ranking university (fellow of various learned societies, say)	101	1349	20
lecturer or senior lecturer, middle-ranking university	16	84	6

He chose the illustrated *h*-index, which is now quoted (among the other statistics) for all researchers listed in WOS. It estimates the 'distance' along the $45°$ line, by assigning a value, h, when h papers have a citation greater than h. This clearly fixes Jane at $h = 3$ (because three papers have more than three citations).

(c) Citation levels and academic achievement

It must be emphasized, straight away, that a scientist's citation profile is *one and only one*, rather focused, measure of his or her total contribution. Having been head of a big university department or a vice chancellor, having sat on national and international committees, having given years of advice to industry, none of this will count. Not even the writing of books (or papers at many conferences) makes any contribution to WOS listings.

Bearing these limitations constantly in mind, it is nevertheless useful to relate lifetime citation and *h*-index levels (of people at a late stage in their careers) to other measures of distinction as follows. Here, in table 1 each entry, listed in order of decreasing h, shows the average score of three people, over their full life in research, in the designated category.

This must be viewed as a very notional outline, with large variations to be expected for *different subjects* and probably for different countries as well. But I do nevertheless think that it gives quite a useful feel for the distribution of citations with academic achievement. Older researchers suffer a bit because WOS only started a systematic scan of journals in 1975, though a few papers before that date do appear in their lists (possibly because they are still being heavily cited after that time). This is offset by the fact that younger researchers are only half way through their writing days!

To overcome the strong variations between subjects that I have mentioned, the interested reader could easily *construct a version of my table* relating specifically to his or her own subject, using WOS data for known individuals.

(d) Some starting research profiles

Finally, it seems appropriate to have a look at sample profiles for papers and citations of some researchers covering the first 8 years following the award of their PhD degrees. Four such profiles are shown in figure 14. These are based on WOS data for real people, known to me, who will for obvious reasons not be named. They are now at various stages in their careers.

The first is a brilliant researcher who obtained a doctoral degree from a top university working with a first class supervisor. The early papers have all attracted very high levels of citation. Based on this early profile, the researcher could be expected to rise to great heights as a professor, head of department and international researcher; and be elected to fellowships of scientific institutions and national academies.

The second is a high-flying individual who was a research student in an established group at a top university. Like the first, this researcher can be predicted to reach positions of great distinction. The third is a talented scientist at a top university who obtained a first class honours
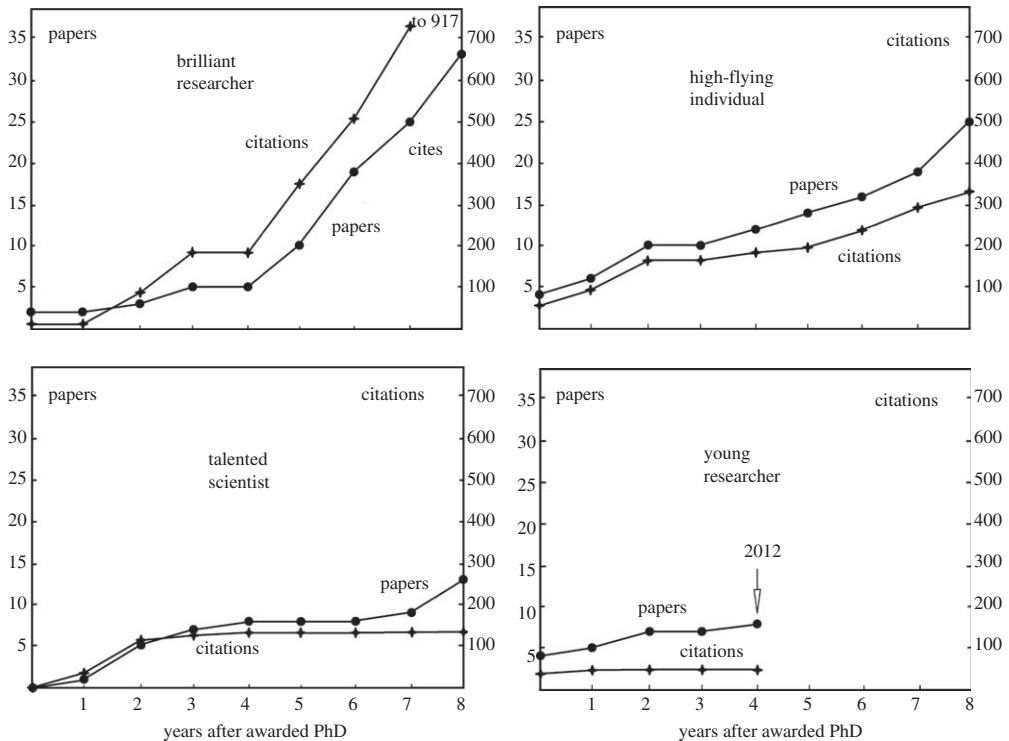
Figure 14. Four plots of papers published and citations gained (both are cumulative) for the early careers of four scientists. Eight years are covered by the horizontal time axis, starting from the award of the PhD.

degree, and then a PhD under an excellent supervisor. The build-up of papers and citations is here slower. Many of the papers were presented at conferences, and these (even when listed) attract fewer citations than those published in peer-reviewed journals. The researcher's career is only just beginning, and the future is not entirely predictable; remember that as time passes the papers displayed will continue to be cited, raising the citation graph higher.

The last profile is for a young researcher at a provincial university who has so far had only 4 years as a post-doctoral student. Four papers were published before the PhD award, and the total is now eight. Again, we remember that the papers have not been collecting citations for very long; indeed, the paper published in 2012 could not possibly have been cited yet. I wish the researcher well.

It may seem, to some, that I have put too much emphasis on citation metrics, which have many limitations as I have discussed earlier. A serious deficiency is that they do not include books, and they vary dramatically between disciplines. It is clear that they will tend to be high in fields which include many researchers. This could be the case in fields that address an important societal problem, such as managing climate change; or in fields, perhaps supported by a lot of money, such as denying climate change. However, the metrics do now play (for better, or more likely for worse) a key role in the government funding of universities both in the UK and abroad. I will end with a quotation, a succinct version of *Goodhart's law* in economics:

When a measure becomes a target, it ceases to be a good measure.

This quotation relates, of course, to the 'playing of games' the simplest of which is for an author to engage in massive self-citation. One step further is for a ring of unethical scientists to agree to give each other excessive numbers of citations. We must all urge funding agencies to move away

from counting citations, and give much more emphasis to actually reading research proposals to judge their quality.

One very good reason for publishing as much as possible early in your career is to get an early lectureship, and hence (hopefully) research students of your own. This will greatly expand your activities and horizons, allowing you to get several lines of research up and running at the same time and gain even more exposure for your ideas.

8. Concluding remarks

I have tried to write an article that is both informative and reasonably entertaining, comprising many good memories from my own lifelong research activities. At the same time, I have tried to offer useful snippets of advice to young researchers who are just starting their careers. Research can, and should, be both exciting and fun as you follow your instincts for increased understanding of fascinating phenomena. I have enjoyed my own career immensely, and found it extremely rewarding and satisfying, and I trust that some who read this article (or at least look at the figures!) may be tempted to follow. The research lifestyle at a university offers a lot of personal freedom, and international conferences provide wonderful opportunities for travelling and meeting like-minded, enthusiastic people from many lands.

More details of the research activities at UCL can be found in this Theme Issue in the personal memories of Hunt & Virgin [55], and the opening article by Isaac Elishakoff summarizes the theme and gives some account of my own research career [56].

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