Submission 28 June 2012

Advice to a Young Researcher: with reminiscences of a life in science

J. Michael T. Thompson

Honorary Fellow, Department of Applied Mathematics and Theoretical Physics, University of Cambridge, Cambridge, CB3 0WA, UK and *School of Engineering, University of Aberdeen, Aberdeen, AB24 3FX, UK*

1. Introduction

This Festschrift for my 75th birthday, is kindly being organised as a theme issue of *Phil. Trans. R. Soc.* (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 realised, would be to assemble some frank and informal advice for young university scientists during their early careers. I have adopted this approach, follow chronologically the progress of a researcher for 10 years from when he or she starts thinking about doing a Ph.D. 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 life-long 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 life style 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 (DAMTP) in Cambridge, who enthused in his very readable article (Batchelor 1997):

> 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?

3.1. 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’s 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 (1966) 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:

> 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 Liam Hudson, 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!'

3.2 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 explain John's achievement, at the time of this essay, of having 27,214 citations (see §7) to his researches 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).

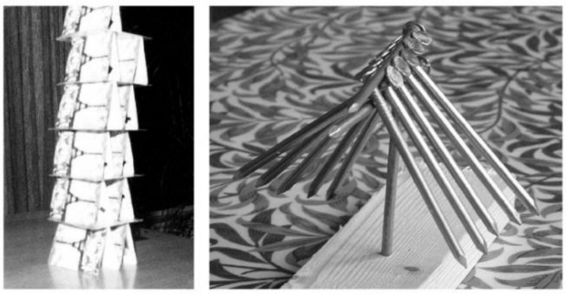


Fig 1. All a matter of balance. (a) A house of cards, built by my daughter Helen in 1980 during a visit to John Hutchinson of Harvard. (b) Answer to a nail-balancing puzzle, described in §5.6, posed to me at my badminton club.

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 Cambridge University, from which I graduated in 1958, and has worked on the crowd-induced vibrations of the wobbly Millennium bridge in London (Strogatz *et al* 2005). Having developed a late passion for science, Helen is now a lecturer in ‘Biomedical Science and Molecular Biology’ at Anglia Ruskin University (in their Cambridge campus) where she is studying the DNA profiles of black squirrels (McRobie *et al* 2009, McRobie 2012). Richard, meanwhile, is a Director (IT) at the head office of a leading financial institution in London’s Canary Wharf. There, 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 3b).

3.3 Medawar’s intelligence test

The distinguished biologist Sir Peter Medawar (1915–1987) was an Oxford graduate who spent eleven years at UCL as the Jodrell Professor of Zoology. His brilliant research on graft rejection, vitally important for organ transplants, was recognised by the award of the Nobel Prize in 1960. In his informative and instructive book (Medawar 1979), entitled *Advice for 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 instantly that this explanation is nonsense is undoubtedly bright. Conversely, anyone who still can't see it as nonsense even when it is explained (as below), must be rather dull.

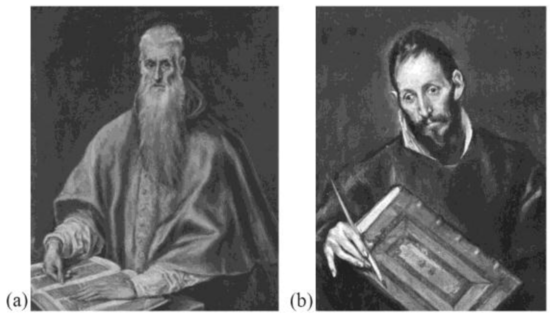


Fig 2. (a) Painting of Saint Jerome by Domenikos Theotokopoulos (1541 – 1614). Known as El Greco (The Greek), the painter was born in Crete and settled in Toledo. He was a significant painter of the Spanish Renaissance. (b) Painting, thought to be a self-portrait of the artist.

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

3.4 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 (Batchelor 1997). Here, I will just quote from (Medawar, 1979). 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 (1972).

3.5 Cultivating good ideas

I am indebted for this section on creative problem-solving to a private communication 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.

4. Getting started on your Ph.D.

4.1 Finding a place with funding

The usual route into research is to stay at university after your first degree and work for a doctor's degree, which usually takes three 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 Ph.D., though at Oxford as D.Phil.). 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 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 ‘Mr A. H. Chilver’, as it said on his door, because in those far-off days Cambridge University did not ‘recognise’ doctor’s degrees awarded by other universities. (Cambridge still doesn’t recognise bank holidays!) The Ph.D. of Henry Chilver was awarded by Bristol University. This didn’t stop him becoming first Sir Henry Chilver, and finally Lord Chilver. He has remained a good and close friend ever since I worked for three years under his supervision on the buckling of spherical shells (figure 3). He wrote a very kind biographical memoir about me on the occasion of a workshop in my honour held at UCL shortly after my retirement in 2002 (Chilver 2006).

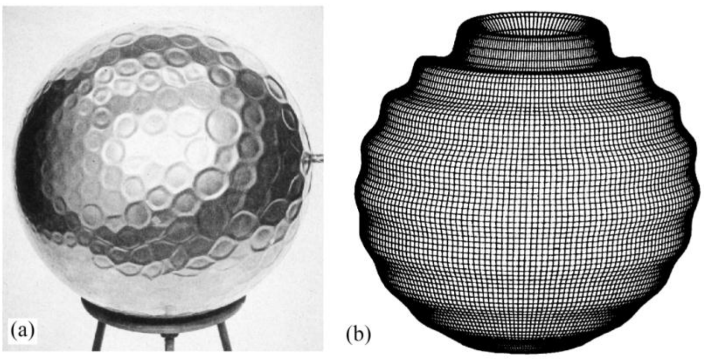


Fig. 3 (a) An electroplated spherical shell made at Stanford by Nicolas Hoff and his team. It has been buckled into many dimples by evacuating the interior. Note that these dimples have been progressively produced and stabilized by hitting an internal mandrel. So they give no clue to the initial buckling 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.

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 Ph.D. 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 funding from EPSRC is that grant applicants can no longer ask for Ph.D. support. This leaves two EPSRC sources of Ph.D. funding available to universities. (1) Akin to the standard research studentships of old (but less in total number) there are *doctoral training grants* made to a university based on their other EPSRC funding. (2) Much funding is now concentrated into *doctoral training centres* (DTCs) 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 one-year Master's degree followed by a regular Ph.D.

4.2 Your supervisor and thesis

So by one of these routes, you will find yourself working with a supervisor, who might be a lecturer, reader, or professor of the university. Throughout the three 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 Ph.D. 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 three 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*).

4.3 Equipment and environment

Unlike when joining a company, or large institution, where there is already a high degree of organisation, you will find that on starting a Ph.D. 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. Sir 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 9am to 5pm 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 out-weigh the money spent on equipment. 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 I finish 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 programmes. 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 Bristol University, how he found time to write so many dynamics books he said he simply got up three hours before everyone else (which would certainly not suit me), and wrote a chapter before breakfast!

4.4 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 deceased this year) 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 nontrivial 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 realised 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 don't 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. Don’t 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 §6.3 *et al*) 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.

4.5 Don't 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 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 one’s self (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

5.1 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 emphasise:

> 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 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 can't 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 the time.

5.2 Read: but not too much

Reading the literature is certainly important, but can be overdone, so consult your supervisor. Disadvantages can be: it becomes a substitute for thinking things out for yourself; you get mesmerised by the accepted view; you can feel overwhelmed by the work of ‘giants’ and feel inadequate or just give up altogether. In his afore-mentioned book, Liam Hudson (1966) talks about his early post-doctoral research on devising aptitude tests for the arts and sciences:

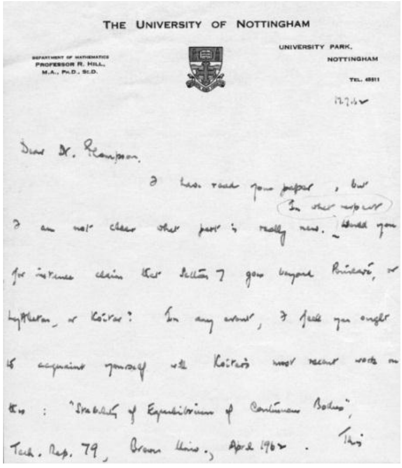
> Millions of research hours had been devoted to this problem of ‘differential aptitude’ before I learnt of its existence. Happily, though, my ignorance of the literature was complete: if I had had even a smattering, I should certainly have tackled something else.

In my own early days there was a similar situation with regard to the monumental thesis of Warner Tjardus Koiter at Delft (Koiter 1945), which appeared, written in Dutch, immediately after the war. 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 (van der Heijden 2009). As I wrote in the preface of my book with Giles Hunt on *A General Theory of Elastic Stability* (Thompson & Hunt 1973):

> 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 (1979) later referred to these remarks, 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 (Thompson 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.



THE UNIVERSITY OF NOTTINGHAM

DEPARTMENT OF MATHEMATICS
PROFESSOR R. HILL,
M.A., PH.D., SC.D.

UNIVERSITY PARK,
NOTTINGHAM

TEL. 48811

12.7.62

Dear Dr. Thompson,

I have read your paper, but
in other respects
I am not clear what part is really new. Would you for instance claim that section 7 goes beyond Poincaré, or Lyttleton, or Koiter? In any event, I feel you ought to acquaint yourself with Koiter's most recent work on this: "Stability of Equilibrium of Continuous Bodies", Tech. Rep. 79, Brown Univ., April 1962. This

Fig 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.

In the next section we look at another significant aspect of the scientific literature … it inevitably contains errors!

5.3 Read: but don't always believe

Perhaps the most important thing that I should say about the literature is summarised 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 §5.6). Under 'erroneous' will be simple numerical errors in a theoretical analysis (which even a conscientious referee could never hope to spot), or not realising 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.

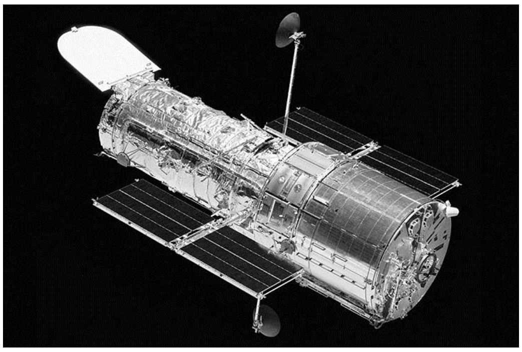


Fig 5. The Hubble telescope which needed expensive in-flight repairs due to errors made in the grinding of its massive primary mirror.

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 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 realise that all the data you acquired yesterday was flawed by fitting the wrong calibrator, take comfort that it is only your supervisor that you will be confessing to in five 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 noticing 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.

5.4 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 layman 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 many new avenues of study.

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

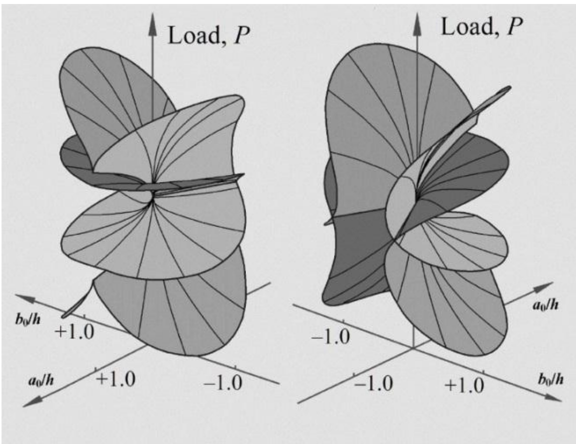


Fig. 6. Imperfection-sensitivity surfaces for the interactive buckling of a stiffened plate calculated and drawn by Giles Hunt. We later learned that this was the first practical example of the hyperbolic umbilic catastrophe. Luckily Giles had, in curiosity-driven mode, draw the top halves of the pictures 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.

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 (1962-3). 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 (Thompson 1960, Carlson *et al* 1967, Bushnell 1981). 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.

5.5 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 Ph.D. 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 were used by the oil industry for the off-shore mooring of tankers.

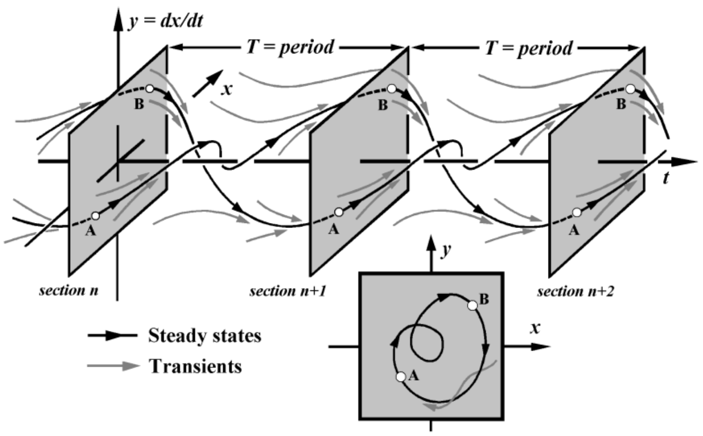


Fig 7. An illustration from Thompson & Stewart (1986) showing stroboscopic Poincaré sections of a periodically-driven nonlinear oscillator, illustrating a steady-state sub-harmonic motion of order $n = 2$. I spent quite some time drawing this figure, and it is rewarding that it has been reproduced (with permission) by quite a few researchers.

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 programmes, 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 Warwick University. David Rand was particularly helpful, and we realised that what we were seeing were chaotic motions of an impact oscillator (Thompson & Ghaffari 1982, Thompson 1983). In those days mathematicians were excited 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* that introduced the new ideas to engineers and scientists around the world (Thompson & Stewart 1986). The book was translated into Japanese and Italian, and had world-wide 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 (Thompson 1975, 1982). So be warned, you must stick to your guns if you discover, or even use, something new.

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 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 programme, we looked no further.

Several years later, having learned about fractal basin boundaries from my collaborations with Bruce Stewart (Brookhaven Lab, USA) and Yoshi Ueda (Kyoto University, Japan) I re-visited the problem with a new research student, Mohamed Soliman. We realised 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* (Thompson & Soliman 1991).

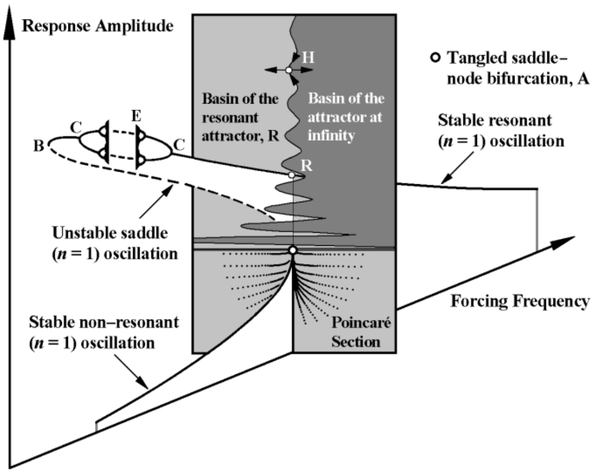


Fig 8. A schematic 3D sketch illustrating the structure of the tangled saddle-node bifurcation. The basin boundary is the tangled inset of the hill-top saddle, H. This accumulates on the saddle-node at A. The indeterminate jump from A may: (1) settle on the stable, period-one, resonant attractor, R; or, (2), settle on a fugitive, small-basin, period-three attractor (not shown in the sketch); or, (3), escape from the potential well to infinity.

The most remarkable example of a Nobel-Prize-winning discovery arising from a 'discrepancy' is undoubtedly that of Arno Penzias and Robert Wilson (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 cleaning 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 '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: don't ignore or hide discrepancies. You must learn to love and use them!

5.6 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 programmes (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 like 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 summarised 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) viewed the problem from a different angle. If at all possible, you should repeat your calculation using a totally different package.

To give a different angle and balance to my 'academic' judgements, I am giving next 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 private communication:

> 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, Professor Rod Rainey, head of technology (floating structures) at W. S. Atkins plc, writes in a private communication:

> 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 1,000. 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!

5.7 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 realise 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.

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 the paper (Thompson & Lewis 1972), 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 1(b), applying my own principle (see §6.4) that most readers only look at the figures anyway! In a recent e-mail, Marian Wiercigroch (see §6.5) tells me 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 five-day residential courses, at which groups of students have stimulating lectures and workshops often given by research students from UK universities.

Fig 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.

6. Presenting your work

6.1 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 formalised 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 35mm slides, I accumulated box upon box of these slides. I still have them, and can't 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 an 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 (1) a slide just vanishing and the next one appearing or (2) a slide gliding off the screen, in a direction of my choice, followed by the next one gliding in, or (3) 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, don't imagine that you should leave big spaces between the lines; this will just waste space. Finally, please don't 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.

6.2 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 or institutes, 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 organising 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 organised 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!

6.3 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 himself, quite apart from informing others and assisting in the building of a good CV. 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 (REF 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 §6.2.

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, unorganised 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.

6.4 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 (2010) which is short and jokey, and Doumont (2009) which is a heavier read. Another excellent source of advice, specifically for the writing of papers in mechanics, is the paper by Villaggio (1993). 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.

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*' (Boswell 1986) 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 is likely to look through the figures. This must inevitably influence the way you write and organise 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.

Don't 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 quality scientific journal, I found the average number of words per caption (covering 7 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 (Thompson 1960) 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 he 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 wouldn't want to say fork, incorrectly, just to stir things up. Now in literature, and in the minds of some 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' (Gibbons 1932).

Well this sort of variation is usually bad in science, as emphasised by a number of distinguished writers. Repetition can make for clarity, as Michael McIntyre, FRS, illustrated in his article on *Lucidity and Science* (McIntyre 1997) with the following (my italics show the valuable repetition):

> **Example1.** 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 (1997):

> … 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, …

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.

6.5 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 recognised by the Royal Society, which is why it always resists 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 organiser, 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 Sir 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 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 by Isaac Elishakoff (seen in figure 10) which pointed the way towards a probabilistic theory of imperfection sensitivity (Elishakoff 1983). The appearance of a subsequent book on this was most welcome (Elishakoff *et al* 2001).

Fig 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 Sir James Lighthill, Provost of UCL; with (from left to right) Isaac Elishakoff and Joseph Singer, both from Haifa, Israel.

Asked by the then head of department, Ken Kemp, to develop an undergraduate course in dynamics, my research interests drifted in the same direction, and I was awarded a five-year Senior Fellowship (1988-93) 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 was won by Gert van der Heijden to pursue his studies on the spatially-chaotic twisting of elastic rods that we had discovered with Alan Champneys (Champneys *et al* 1997, van der Heijden *et al* 2002). This work on spatial chaos created an interesting link between the two groups, and a seminal paper was by Hunt *et al* (1989). 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 2012.

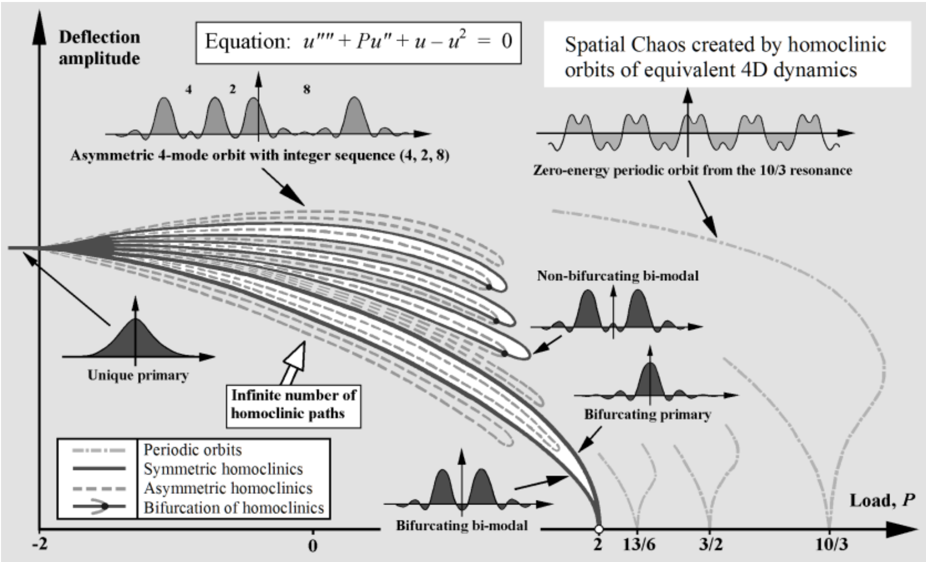


Fig 11. The complex spatially-chaotic load-deflection diagram for an infinitely long strut on a nonlinear elastic foundation. Four complex eigenvalues give a saddle-focus at the origin for -2 < P < +2. The critical buckling load is at P^C = 2. An infinite number of homoclinic paths approach arbitrarily close to P^C. This is an archetypal example of the static-dynamic analogy. The underlying mathematical results are due to Buffoni *et al* (1996).

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 (Wiercigroch *et al* 2005). It hosted an IUTAM symposium on *Nonlinear Dynamics for Advanced Technologies and Engineering Design*, in 2010.

6.6 The grant report, depth to simplicity

One thing that my mentor, Henry Chilver, always emphasised 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 naïve, 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.

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 (MacMaster & Thompson 1994). 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 §5.6), whose razor-sharp mind contributed greatly to the successful outcomes.

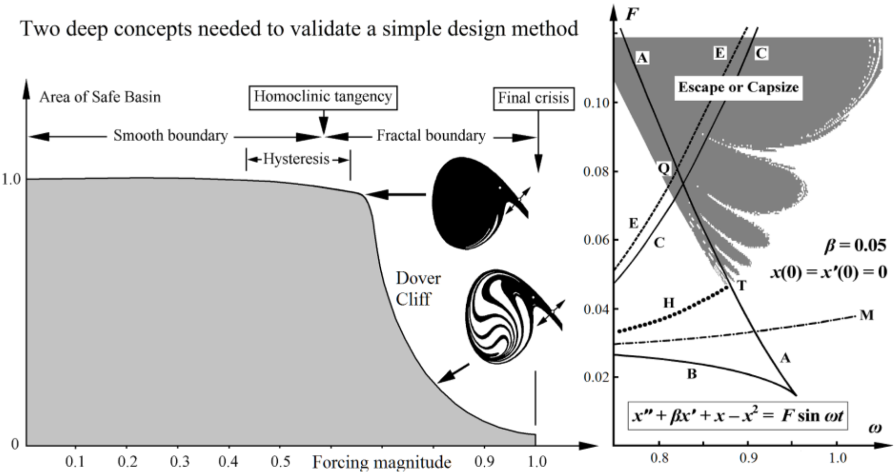


Fig 12. Two advanced concepts of nonlinear dynamics whose discoveries allowed the formulation of the simple practical idea of transient capsize testing. (a) The Dover cliff phenomenon in which there is a sudden fractal erosion of the safe basin of attraction; and (b) the associated fractal structure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

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 (Thompson 1989).

6.7 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 organise 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 summarise 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 can't 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 didn't 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 wouldn't 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 (2012). These include the insightful treatise by Chris Calladine (Calladine 1983), 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 programmes, as I discussed in §5.6.

When there is a big new discovery, like 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 (Païdoussis 1998), including a section on my 'magic box' (Thompson 1982), 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 (Païdoussis *et al* 2011).

Some of my research students ended up as prolific book writers. Koncay Huseyin, distinguished Professor Emeritus, was Head of Systems Design Engineering at Waterloo University and wrote about his extensive studies of multi-parameter systems in three excellent books (Huseyin 1975, 1978, 1986). 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 (Roorda 1980). Lawrence Virgin, professor (and recently head of department) in the engineering faculty of Duke University, USA, published two stimulating books with CUP (Virgin 2000, 2007), 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 (Thompson & Hunt 1984). Steve Bishop collaborated with Tomasz Kapitaniak on a *Dictionary of Nonlinear Dynamics* (Kapitaniak & Bishop 1999). 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?

7.1 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 world-wide-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 only scans 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 every-day 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 two or three years have passed since publication it could well be the case that one of your papers has attracted no citations. Don't 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 (2007-8). 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 don't 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 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 data-base 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.

7.2 The *h*-index of solidity

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

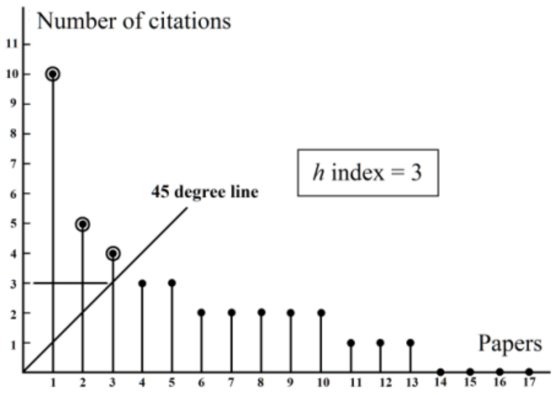


Fig 13. The notional histogram of a young researcher, Jane Smith, showing papers listed in order of decreasing number of citations (not chronologically). The drawn 45° line illustrates the meaning of the *h*-index, here equal to 3.

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 (2005) 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. At the same time, he wanted to decrease the disadvantage of having a long tail of un-cited papers at the right-handed end of the figure which would, for example, pull down the overall citations per paper ratio.

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 3 papers have more than 3 citations).

7.3 Citation levels and academic achievement

It must be emphasised, 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 life-time 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.

	Papers	**Cites**	***h*-index**
Exceptional international researchers, leading cosmologists and biologists, etc	213	27579	76
Top world figures (Nobel Prize, or President of the Royal Society, say)	172	19112	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

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.

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.

7.4 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 eight years following the award of their Ph.D. degrees. Four such profiles are shown in figure 14. These are based 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.

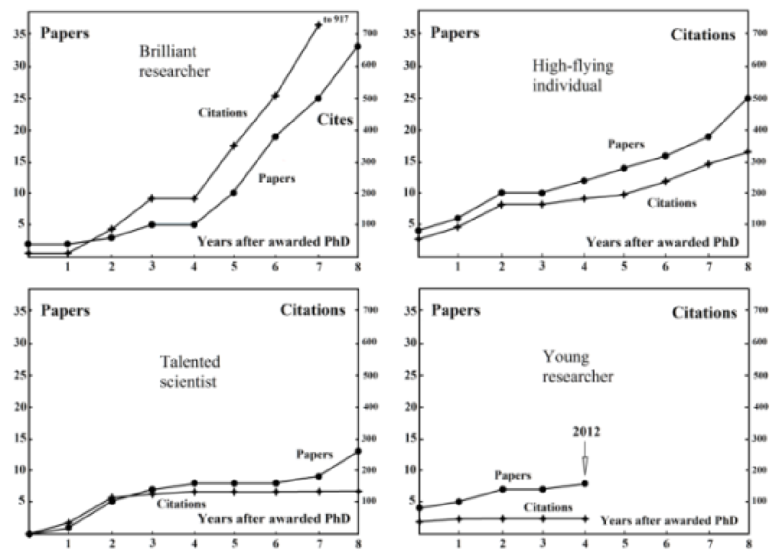


Fig. 14. Four plots of papers published and citations gained (both are cumulative) for the early careers of four scientists. Eight years are covered by the horizontal time axis, stating from the award of the Ph.D.

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 degree, and then a Ph.D. 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 four years as a post-doctoral student. Four papers were published before the Ph.D. 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 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.

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, 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 life-style 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.

Acknowledgements

Being unaccustomed to writing an article of this type, I have solicited comments from many friends and colleagues. I have particularly valued the inputs received from Michael Ashby, David Bushnell, Chris Calladine, Alan Champneys, Isaac Elishakoff, Giles Hunt, John Hutchinson, Michael McIntyre, Michael Païdoussis, Lawrence Virgin and Marian Wiercigroch. Finally special thanks go to Linda Smith and my wife Margaret for a vigorous three hour discussion in my village of Foxton.

References

Batchelor, G. K. 1997, Research as a lifestyle, *Applied Mechanics Reviews*, **50**, 11-20.

Boswell, J. 1986, *The life of Samuel Johnson*, (Hibbert, C., ed.) New York: Penguin Classics (Boswell's book was first published in the UK in 1791).

Buffoni, B., Champneys, A. R. & Toland, J. F. 1996. Bifurcation and coalescence of a plethora of homoclinic orbits for a hamiltonian system. *J. Dynamics Difl Equat.* 8, 221-281.

Bushnell, D. 1981. Buckling of shells – pitfall for designers, *AIAA Jnl*, **19**, 1183-1226. DOI: 10.2514/3.60058.

Bushnell, D. 2012 *Shell buckling*, website, http://shellbuckling.com/index.php

Calladine, C. R. 1983. *Theory of shell structures*, Cambridge University Press, Cambridge.

Carlson, R. L., Sendelbeck, R. L. & Hoff, N. J., 1967. Experimental studies of the buckling of complete spherical shells, *Experimental Mechanics,* **7**, 281-288.

Champneys, A. R., van der Heijden, G. H. M. & Thompson, J .M. T. 1997. Spatially complex localization after one-twist-per-wave equilibria in twisted circular rods with initial curvature, *Phil. Trans. R. Soc.* A **355**, 2151-2174.

Chilver, A. H. (Lord), 2006. Michael Thompson: his seminal contributions to nonlinear dynamics – and beyond, *Nonlinear Dynamics*, **43**, 3–16.

Doumont, J-L. 2009 *Trees, maps and theorems – effective communication for rational minds*, Principiae Press, Kraainem, Belgium.

Elishakoff, I. 1983. How to introduce the imperfection-sensitivity concept into design, in *Collapse: the buckling of structures in theory and practice* (ed. J. M. T. Thompson & G. W. Hunt), Cambridge University Press, Cambridge (pages 345-357).

Elishakoff, I., Li, Y. W. & Starnes, J. H. Jr, 2001. *Non-classical problems in the theory of elastic stability*, Cambridge University Press, Cambridge.

Gibbons, S. D., 1932. *Cold comfort farm*, London: Longmans.

Hirsch, J. 2005. An index to quantify an individual's scientific research output. *Proc. Nat. Acad. Sci*.**46,** 16569.

Hudson, L. 1966. *Contrary Imaginations: a psychological study of the English schoolboy*, Penguin Books, Harmondsworth, England.

Hunt, G. W., Bolt, H. M. & Thompson, J. M. T. 1989. Structural localization phenomena and the dynamical phase-space analogy, *Proc. R. Soc* A **425**, 245-267.

Huseyin, K. 1975. *Nonlinear theory of elastic stability*, Noordhoff International Publishing.

Huseyin, K. 1978. *Vibrations and stability of multiple-parameter systems*, Sijthoff & Noordhoff. (translated into Chinese).

Huseyin, K. 1986. *Multiple-parameter stability theory and its applications*, Oxford University Press, Oxford.

Kapitaniak, T. & Bishop, S. R. 1999. *Dictionary of nonlinear dynamics*, Wiley-Blackwell.

Koiter, W. T. *On the stability of elastic equilibrium*, 1945. Dissertation, Delft, Holland. (An English translation is now available as *NASA, Tech. Trans.*, **F 10**, 833, 1967.)

Koiter, W.T. 1979. Forty years of retrospect, the bitter and the sweet. In: Besseling, J. F. & van der Heijden, A. M. A., editors. *Trends in solid mechanics*. Delft: Delft University Press, Sijthoff & Noordhoff Int. publishers; pp 237-46.

MacMaster, A. G. & Thompson, J. M. T. 1994. Wave tank testing and the capsizability of hulls, *Proc. R. Soc.*A **446**, 217-232.

McIntyre, M. E. 1997 Lucidity and science, I: writing skills and the pattern perception hypothesis. *Interdisciplinary science reviews*, **22**, 199-216.

McRobie, H., 2012. Black squirrels: genetics and distribution. *Quarterly Journal of Forestry*, **106** (2), 137-141.

McRobie, H., Thomas, A., Kelly, J., 2009. The genetic basis of melanism in the grey squirrel (*Sciurus carolinensis*). *Journal of Heredity*,**100**, 709-714.

Medawar, P. B., 1979. *Advice to a young scientist*, Harper & Row, New York.

Païdoussis, M. P. 1998 *Fluid-structure interactions: slender structures and axial flow.* Vol 1, Academic Press, London. (Vol 2, Elsevier Academic Press, London, 2004).

Païdoussis, M. P., Price, S. J. & de Langre, E., 2011. *Fluid-structure interactions: cross-flow induced instabilities*, Cambridge University Press, Cambridge.

Penzias, A. A. & Wilson R. W. 1965. A measurement of excess antenna temperature at 4080 Mc/s, *Astrophysical Journal Letters*, **142**, 419–421.

Popper, K. R. 1972. *The logic of scientific discovery*, 3d edn, London: Hutchinson.

Roorda, J. 1980. *Buckling of elastic structures*, University of Waterloo Press, Waterloo.

Strogatz, S. H., Abrams, D. M., McRobie, A., Eckhardt, B. & Ott, E., 2005. Crowd synchrony on the millennium bridge. *Nature*, **438**, 43-44. DOI: 10.1038/43743a

Thompson, J. M. T. 1960. Making of thin metal shells for model stress analysis, *J. Mech. Engng Sci.*, **2**, 105-108.

Thompson, J.M.T. 1963. Basic principles in the general theory of elastic stability, *J. Mech. Phys. Solids*, **11**, 13-20.

Thompson, J. M. T. & Ghaffari, R. 1982. Chaos after period-doubling bifurcations in the resonance of an impact oscillator, *Physics Letters,* **91A,** 5-8.

Thompson, J. M. T. & Soliman, M.S. 1991. Indeterminate jumps to resonance from a tangled saddle-node bifurcation, *Proc. R. Soc.* A **432**, 101-111.

Thompson, J. M. T. & Hunt G. W. 1973. *A general theory of elastic stability*, Wiley, London.

Thompson, J. M. T. & Hunt, G. W. 1984. *Elastic instability phenomena*, Wiley, Chichester.

Thompson, J. M. T. & Lewis, G .M. 1972. On the optimum design of thin-walled compression members, *J. Mech. Phys. Solids*, **20**, 101-109.

Thompson, J. M. T. & Stewart, H. B. 1986. *Nonlinear dynamics and chaos: geometrical methods for engineers & scientists*, Wiley, Chichester, Second Edition, 2002. (Translated into Japanese and Italian)

Thompson, J. M. T. 1975 Experiments in catastrophe, *Nature*, **254**, 392-395.

Thompson, J. M. T. 1982 *Instabilities and catastrophes in science and engineering*, Wiley, Chichester. (Translated into Russian and Japanese)

Thompson, J. M. T. 1982. Paradoxical mechanics under fluid flow, *Nature*, **296**, 135-137.

Thompson, J. M. T. 1983. Complex dynamics of compliant off-shore structures, *Proc. R. Soc.* A **387**, 407-427.

Thompson, J. M. T. 1989. Chaotic phenomena triggering the escape from a potential well, *Proc. R. Soc.* A **421**, 195-225.

van der Heijden, A. M. A. (ed) 2009. *W. T. Koiter's elastic stability of solids and structures*, Cambridge University Press, Cambridge.

van der Heijden, G. H. M., Champneys, A. R. & Thompson, J. M. T. 2002. Spatially complex localisation in twisted elastic rods constrained to a cylinder, *Int. J. Solids & Structures*, **39**, 1863-1883.

Villaggio, P. 1993. How to write a paper on a subject in mechanics, *Meccanica*, **28**, 163-167.

Virgin, L. N. 2000. *Introduction to experimental nonlinear dynamics*: a *case study in mechanical vibration*, Cambridge University Press, Cambridge.

Virgin, L. N. 2007. *Vibration of axially-loaded structures*, Cambridge University Press, Cambridge.

Wiercigroch, M., Wojewoda, J. & Krivtsov, A. M. 2005. Dynamics of ultrasonic percussive drilling of hard rocks, *Journal of Sound & Vibration*, **280**, 739-757.

Zanders, E. & Macleod, L. 2010. *Presentation skills for scientists*, Cambridge University Press, Cambridge.