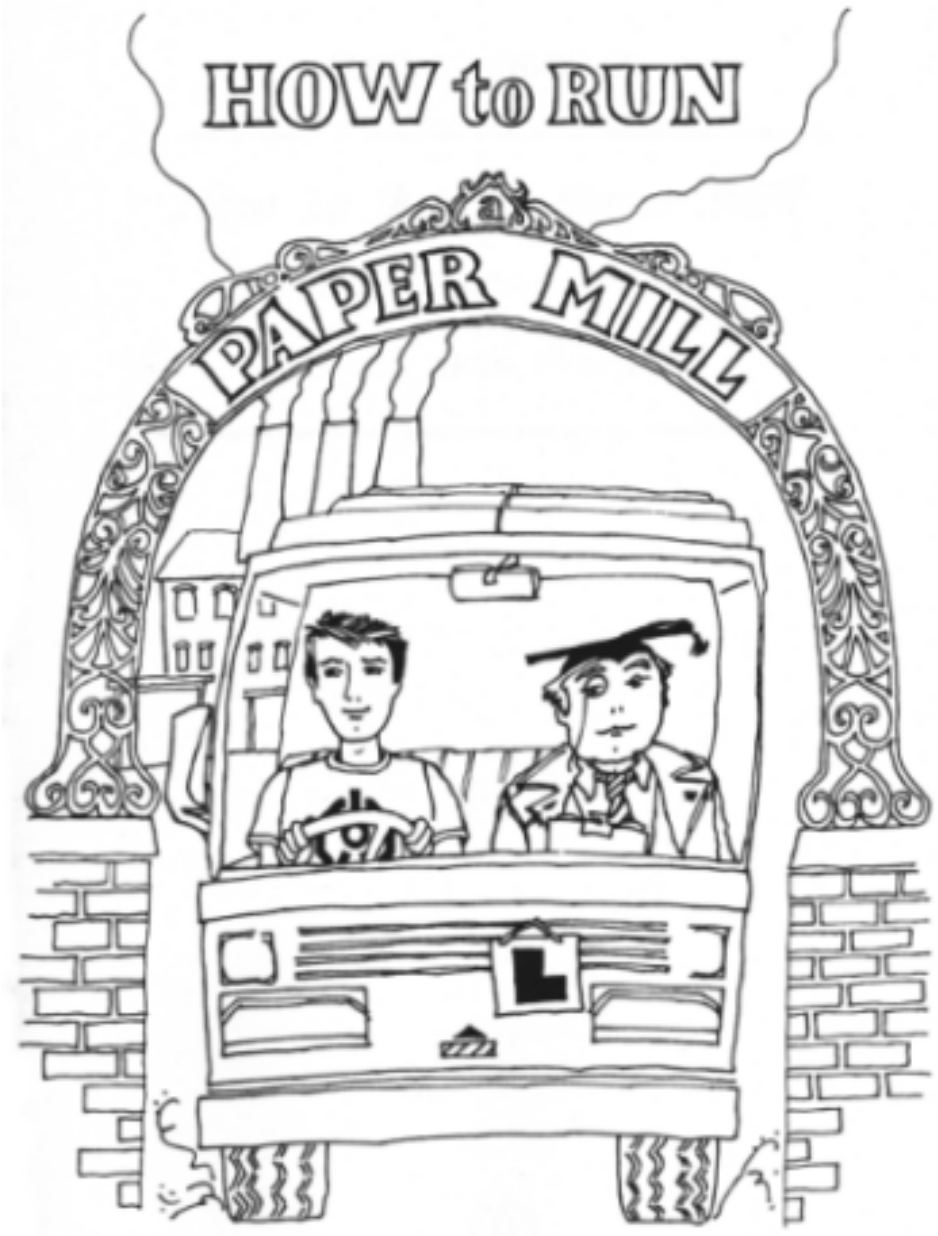

by

JOHN WOODWARK

Cartoons by Carol Wade

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Information Geometers Ltd
47 Stockers Avenue
Winchester
SO22 5LB
UK

books@inge.com

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This PDF version is basically the master at 110% enlargement from which the original edition was printed. Some necessary changes have been made to pagination, typefaces and figures, and (four) known typographical errors have been corrected.

Typeset and designed by the author.

What is the end of Fame? 'tis but to fill
A certain portion of uncertain paper.

Byron

Acknowledgements

I had much good fortune in having the draft of this book read through by people who actually suggested some good material to add, as well as (correctly) rubbishing what I had originally written. Craftily I offered them no money for their contributions, so the least I can do is to give them an advertisement instead. They are: Adrian Bowyer, Ralph Martin, Alan Middleditch, Tony Nutbourne, Malcolm Sabin, Phil Willis, and my wife Flavia.

I would also like to thank Carol Wade for her cartoons; without them, this book would be lemonade without bubbles.

Contents

Illustrations

Foreword

In this book, you will find nothing about mashing up trees and processing them into Fine White Wove. The illusory title is—in concept—borrowed from a favourite book of mine: *How to Run a Bassoon Factory*[1], which is not actually about making bassoons, but is a satirical commentary on the Business Methods of its time. *How to Run a Paper Mill* is also an illusory title; this book is really about Writing Technical Papers and Getting them Published. If you have ever written a technical paper, or you are thinking of doing so, or (most likely) somebody else is pressing you to do it, you will find that this book at least discusses some of those matters which other books (excellent in their way) that revolve around Writing Short Sentences don't: simple but important questions such as whether to do the work first and write up second, or vice versa.

How to Run a Paper Mill is a wildly expanded version of a lecture given at a Geometric Modelling Society course at Keble College, Oxford in January 1992. The enthusiastic reception given to that lecture provided the impetus to get writing.

No doubt *How to Run a Paper Mill* serves up as many faults as Wimbledon fortnight, but there are some Defects which are particularly glaring. We'll get them over here to stop you being surprised later[2].

The first Defect concerns not the book, but the credibility of its Author. When I was thinking about the material for this book, I spotted a couple of rather depressing newspaper articles which seemed to point an accusing finger at my suitability for the task. One was an obituary for a German theologian, (Professor) Oswald von Nell-Breuning (SJ), who had died aged 101. Despite being

[1] Written by Nigel Balchin (under the pseudonym of Mark Spade) and published by Hamish Hamilton c1933. You will be lucky to find a copy; mine is a reprint dating from the year that I was born.

[2] And also to spike the reviewers' larger guns.

stopped for a time from writing by Hitler (an enviable accolade), over his 101 years he had managed to put out "some 1800" books and articles. The second cutting concerned a survey of publication rates, and named the most prolific writer that they had found[3], who was producing a paper every 4.7 days.

I look at my own publication record[4] and see a dwarf. What's worse, I think that much of my own stuff was sent to the wrong place at the wrong time. So, be warned: this book is not *How to Do It*, by The Expert, but *How I Now Think You Do It*, by Someone Who Has Tried Many Unsuccessful Techniques Personally.

The second Defect relates to the coverage of this book. I do not know how you can have experience of the world of technical publication from the Author, Referee and Editor's viewpoints without being a specialist in something. Therefore, advice on this matter is always tainted with the flavour of some specialism: both the subject itself and the characters of the particular conferences and journals that serve it. My own is—broadly—mechanical and manufacturing engineering and computer science[5], and—more specifically—computer-aided design and applications of geometry in computing. So, if you're working in this area, you're laughing. If you work in another technological area, you will probably find this book strikes a lot of chords. But if you're working in 'pure' science—where the journals are all Royal Society-ish—or if you're involved with the Arts, then you'd better try to get your money back. You certainly won't find out from me how to get away with writing the 476th thesis on the works of Jane Austen.

The third Defect is simple, and should already be more than apparent. This book is not itself a carefully-researched piece of work, and was never meant to be. There has been no proper literature search (see Chapter 4, Method No. 4). If you wanted a comparative study—or if there is a similar and better-researched (and cheaper)

[3]Prof. Stephen Bloom, Royal Postgraduate Medical School, Hammersmith Hospital, London produced 773 papers between 1981 and 1990 (survey by Institute for Scientific Information, Philadelphia, 1991).

[4]Forty-ish papers, a couple of books written or edited.

[5]You can tell this from the way I use footnotes as surrogate subroutines. This book was typeset as it was written, and Knuth's TEX and Lamport's LATEX systems make footnotes far too easy (end of advertisement).

book—then find someone to swap.

The final Defect in this book to which I am prepared to admit is actually another defect of my own: Lack of Moral Fibre. There are two distinct viewpoints from which to survey technical publication. On one hand, there is the purpose of the whole exercise, which should be the Advance of Knowledge, Benefit to Mankind and so on. On the other hand, there is the small matter of individuals and their careers. There's no reason why these aspects shouldn't be in balance: just as capitalism—for instance—is intended to harness the cupidity of the individual to the benefit of society. But if (you're interested in that sort of stuff, and) you bought a book on—say—taxation, you might expect it to be something like *Fiscal Policy in the Keynesian Economy* or *How to Pay a Lot Less Tax*, and you wouldn't expect the author to mix up the viewpoints. But I'm afraid that's just what I have done. One viewpoint would have made the book unbearably pompous throughout, instead of in patches; the other logically led to endorsing various sharp practices of which I disapprove. So—I'm sorry—I've remained wobbling on the fence.

1

Introduction

When the Greeks, under Miltiades, beat the Persians in the Battle of Marathon in 490 BC, they dispatched a man[1] back to Athens to tell the citizens, who had a considerable stake in the matter. This chap ran as fast as he could, and appeared in the *agora* the next day fairly exhausted. He was just able to gasp out the information that the Greeks had won, and then promptly expired. Seen as a Communication System, from the relative comfort of our seats in the Information Age, this episode had its flaws: low *bandwidth* and probably an unacceptable *mean time between failures*. Taking a day to deliver one bit of information to the next town and then dropping dead will earn you no plaudits in the time of gigabyte-per-second fibre-optic cables and satellites, etc., etc.

However, while Miltiades' datalink to Athens may have been inefficient, it was the only one. Nor did it carry any advertising. If the *agora* had been full of other runners from all over Hellas reporting breathtaking developments in bronze armour, new Greek fire, or refinements to the Doric column giving 15% better fatigue life, then perhaps the messenger from Marathon would have been noticed only by the ambulance men.

In fact, lucky old Miltiades only had to worry about beating the Persian hordes. Keeping the Athenian media On His Side was clearly a secondary matter. Many commanders—up to Napoleon's time, perhaps—enjoyed the same advantage, but generals in more recent wars have had a rude awakening. Surrounded by a battery of reporters with microphones, camera-persons with satellite dishes, and their retinues, keeping the press On Your Side has become a prerequisite for a successful campaign. You no longer dispatch runners

[1] One Pheidippides, or perhaps Philippides.

THE DATALINK TO ATHENS HAS GONE DOWN

when you've cleaned up on the beach, but time the battle to catch the primetime newsflash between the soap advertisements; 'communicating' may take longer than the actual fighting, and is probably more important to your career.

In science and technology, the same change has taken place. The likes of Archimedes, Euclid and Hero had the field to themselves, or so it seems to us today. And in the Middle Ages, the alchemists actually tried to keep anything they discovered[3] to themselves. For two thousand years and more, we gather, a grateful world beat paths to the doors of the successful mousetrap-makers.

In the late twentieth century, there are suddenly thousands and thousands of would-be Archimedes (and would-be Heroes, too), and there are hundreds and hundreds of technical journals to tell everyone about their splendid work. There's far too much Information slopping about and a great deal of it is useless. Suddenly *doing* 'research' is the easy part, getting anyone to take any notice of it is difficult. Really good ideas can remain buried for years; moderate ideas (like yours and mine) never surface. But many scientists and

[2]Which is, of course, a rough translation of *χαίρετε νικῶμεν* (put that in your algebra system).

[3]Precious little.

technologists still believe that the value of their work is independent of whether anyone finds out about it or not. (In any practical discipline, that looks a shaky proposition, even in some mysterious 'moral' sense.) Some of us are still up in Euclidean space, and we need to be talked down.

Like the military, we must learn what soap-manufacturers' sales executives have always known, that the dullest product, produced in quantity, and sold with gusto, will make a much bigger name for its producer than the revolutionary formula which never sees the outside of a test-tube. Technical excellence has to be sold in the same way as soap-powder excellence—not in boxes, but in technical publications—and, yes, these have to be produced in quantity, and sold with gusto. The advertising budgets of the soap conglomerates are enormous, their marketing plans are more elaborate than anything von Schlieffen dreamt of. You must emulate them; allocate half your time to publishing papers, and then read on!

2

Who Wants Me to Write It?

The answer to the question "Why write a paper?" depends on whether you're taking a philosophical or pragmatic view. Since we agreed to sit on the fence, the skeletons in both cupboards can be dusted off and rattled around in an amusing way—almost as if they had something to say.

One of the interesting things about technical publication is the amounts of money involved and not involved. What I mean by this is that some of the participants in the publication process see the matter as not (directly) a financial one at all; others live on the proceeds. The 'economics' of a technical paper look something like this:

- *Cost of research to taxpayer:* this can of course vary wildly but an approximate figure might be £25,000[1].
- *Publisher's gross receipts:* also subject to much speculation: from less than nothing if it's in loss-making conference up to, say, £5000 in a successful journal.
- *Benefit to author:* difficult to tell, but two papers in a year will probably keep a UK academic in a £20,000-a-year job.
- *Benefit to referees:* £10 if they're lucky.

Authors

Everyone likes to see themselves in print (although presumably the Agatha Christies of this world get rather blasé about it). However, technical authors are mainly trying to publish because:

[1] c1990 and courtesy Malcolm Sabin, who used it in pursuing value for the UK taxpayer's money.

They want to build their own reputation and so advance their own career.

They have been told by their employer to publish and so advance his business. (I definitely count the batch manufacturing of graduates and postgraduates as a business.)

These have usually been quite gentle processes; the tide of publications laps higher and higher—over the dam—and, look, you're a Senior Research Thingummy, a Professor of Domeflipping, or whatever. However, banning the ivory trade seems to have had an unfortunate effect on towers made of that material, which are in bad repair everywhere. Their inhabitants are thus all of a sudden subject to all sorts of cold draughts, one of which is called Motivation by Measurement, and publications are a horribly measurable quantity.

US universities have been at this game for years, setting up hurdles that academic staff have to jump to get their position confirmed: in other words, tenure[2]. These hurdles are often rather specific: a particular number of papers in a particular length of time; and they can be further tightened up by the supply of a list of acceptable journals and (more rarely) conferences. Publication in fly-by-night rags set up by your uncle who's a printer won't do. Some institutions don't trust the 'real' journals and their referees, either. They want to see—not your papers themselves—but your papers *cited* by other people in *their* papers. This looks watertight, but of course there is scope for secret cartels to cross-reference each other's work: hence the conspiracy theory of technical publication.

Organizations

This idea of measuring publications exists in various forms in various countries and organizations. Recently in the UK, whole University Departments, rather than individuals, have been assessed on their research 'performance'. This is an obvious convenience for those with their hands on the financial tap, who can then turn departments' budgets on and off to suit. That is perhaps an interim phase; the obvious extension is to whole institutions. Great battles

[2]US academics in the early stages of this race often use the phrase *publish or perish*; maybe *Perishing Publications* would be a good alternative title for this book.

are promised with one university pitting a prestige Department of Fine Art against the five-star Department of Plant Physiology of another: like so many football teams flaunting their most expensive players.

The industrial sector has its own version of this game, called Research as Public Relations. It is—perhaps unfortunately—playable only by the largest corporations. The idea is to impress your customers by the strength of your scientific establishment. In other words, if they can't understand the product, and certainly doubt whether it's better than anyone else's, you make up their mind by waving Nobel Prizes under their noses. Often, the scientific pinnacle of a company's research effort is really not too close to its products: it's pure scientific derring-do. Nearer the production line, the rate of authorship may not be so prolific[3], but a number of these companies—nominally at least—encourage all 'technical staff' to publish; cash incentives are not unknown and, as this is the language of commerce, you can tell they are taking technical papers seriously.

Referees[4]

Over some ten years of desultory refereeing—perhaps forty or fifty papers—I have received[5]:

- A few cheques for £10.
- One (1) mug.
- One teeshirt—or maybe two; I forget, they didn't fit anyway.

Not a big haul, I would say. Refereeing is the great hangover from the days when Science was the preserve of leisured gentlemen who wrote Letters to each other, and when they got fed up with licking stamps they invented Meetings to present their work at and

[3]I have recently noticed some well-written papers emerge from what should be the most desolate metal-bashing departments of certain Pacific Basin manufacturing companies; the reason for this, and its portent, are obscure.

[4]Referees are sometimes called *reviewers*, and then their reports are called *reviews*. This is common terminology and at least sounds less like football; but it causes confusion with *review papers*, so I have not used it here.

[5]Not including sundry items available F.O.B. Anaheim (CA) etc., which I was unable to collect and the conference organizers wouldn't send.

Spot the Mug

Journals to put them in and—Damme, Sir!—if you needed *money* then you should be in Trade. Was it ever actually like this? My own knowledge of the History of Science begins and ends with a minor obsession with Newton, and reading some of the biographies of him, it would seem that it *wasn't* quite like that[6].

However, to return to the matter in hand, people do still keep on refereeing stuff. Because the process is secret (see Chapter 8) you don't even get any publicity for refereeing a paper. (Although some journals and conferences do publish a list of all the referees they consulted, without saying who did what: that's how to get mugs and teeshirts.) And it's bad form for journals to favour someone's paper merely because he or she has been a steady referee. So I think that people really must be doing it out of a sense of duty, to keep the whole vast juggernaut rolling. How long this can survive against the increasing desire of the paymasters to get something for every researcher-day expended is a question I like to ponder[7].

Editors and Conference Organizers

Editors and Conference Organizers at least get paid—sometimes. I've tried both jobs, and in either case the sheer quantity of paper you receive often makes these primarily bureaucratic problems, rather than exercises in fine judgement. Authors with neuroses to feed—or tenure to get—believe you've got it in for them when really you are expiring under a thick covering of paperwork.

The people with the ultimate financial responsibility for journals and conferences tend to take a profile that is almost as low as that adopted by referees. Thus Editors and Conference Organizers are left as prime targets—not to say sitting ducks—for the spleen vented by disgruntled authors; you have to accept that being spleened at is part of the job.

Publishers

[6]Newton and Leibnitz wrangling for years over the credit for the Calculus certainly eclipses minor modern disputes like the question of who discovered HIV.

[7]Those who say forever should learn to say it in Russian and see how it echoes around in Lenin's Mausoleum.

Amazingly, academic publishers are often profitable; it is related that the late Mr Maxwell started his career on an upward curve[8] by importing and translating Russian scientific documents. How this made him quite so rich is a mystery to me, although academic libraries certainly had more money then. While none of the players other than the publisher thinks of it primarily in this way, it follows that technical publication is actually a commercial activity.

Readers

Readers of technical papers are meant to get information from them; this should have two effects. The first is that other people working on similar problems can start where the authors of the paper left off, and thus avoid going up a blind alley. The second expected result from publication in scientific and, especially, technological fields, is that the published—'pre-competitive'—information is available to be made into commercially competitive products.

In practice, the very different reasons for writing and reading technical papers means that the readers (who are, after all, passive participants) don't get such a good deal as they should. The very volume of publication is probably the biggest problem; but authors can also code things so as to stake their claim with a minimal transfer of information to their competitors. If they leave other researchers unsure what they're up to, it will certainly be difficult to extract the basis of a commercial product from their papers. At the other extreme, elegant and crystal-clear papers may not be about anything actually *useful* at all. Academics are often able to go on clocking up brownie points exploring 'interesting' avenues long after it has become apparent that their likely practical value is negligible.

The Public

The taxpayer is the mainspring for most technical papers. He foots this bill in the twin expectation of keeping the authors sharp to teach his sons and daughters, and to keep progress on the march. Technical papers are the travelogue of this march of progress. It is by turns a great annoyance and a blessed relief to technical authors that the taxpayer is not very familiar with the language in which

[8]Later shown to be part of a parabola.

that travelogue is written.

3

What is a Paper Anyway?

Having pointed out that technical information is not in short supply, the easiest way to find a technical paper is to go into almost any library—and certainly any university or college library—and examine one, or rather, as many as you care to; there will be shelves of them. Generalizing from example is exactly how most people get hold of their idea of what a paper should be. After all, as children we found out about books by pulling a bookshelf down on our heads and examining the hard rectangular objects that fell on us.

Different Viewpoints

Generalizing from the particular is always a dangerous sport and it behoves us to try to be a bit more specific. Just as a car is simultaneously a way of getting from A to B, a blight on the environment, a public danger, and the thing that stops you getting your (environmentally friendly) bicycle out of the garage, we can look at a paper in various ways:

A Work of Literature

The idea of a paper as *literature* is rather hard to swallow. (But then look at what's on offer at an airport bookstall.) Nevertheless, in writing a paper it is unavoidably necessary to do battle with parts of speech and stuff and emerge with a *style*. Just as getting married necessarily involves acquiring a husband or wife, so in writing a paper a style cannot be avoided; only the quality of the acquisition can be varied. Having my own problems in this area (papers, not—of course—marriage), I try not to pontificate too freely in this book. However, there are some observations in Chapter 10.

It is, of course, right to point out that not all technical papers are in English[1]. There are many respectable journals in almost any language you can think of. In science, that is particularly true; but in the more recently mushrooming technologies the preponderance of the literature is in English. No doubt that is a result of the ascendency of the US in matters technological during the immediate post-war period, and the gradual growth of English as a technological *lingua franca*. This is a great gift to native English speakers, but one that we often throw away. If English speakers find it easy to be idiomatic, they also find it easy to be imprecise; and sub-editors can do a lot more to restore idiom than to restore meaning.

A Gobbet of Information

A paper usually has to fulfil certain requirements in terms of its length and the amount of information that it contains. In mathematics, a great discovery can be presented as a terse page or two of symbols—and then off for your Fields Medal. In technology, both the medium and the message usually have to fit certain limits; the problem is, that these limits are not prescribed. But we know that something shorter is called a Technical Note; something bigger is called a Monograph.

A Piece of Formula Writing

Novels with certain predictable plots, characters etc. are often referred to as *formula fiction*. Technical papers follow a formula much more closely than any novel—indeed than anything short of a tax form. The basic idea is a central core of explanation preceded by an Abstract and an Introduction, and followed by Conclusions and References. How the central matter is laid out, the different weights given to the various sections, and matters like Appendices, vary somewhat from subject to subject, and from journal to journal; individual authors may, of course, also depart a little from the norm without disaster.

It's not only the layout that is a formula. There is also the matter of how much detail and comment is presented. For instance, everyone will tell you that a report will have more facts and less inter-

[1] An alternative view that has been conveyed to me is that none of them are.

pretation than a technical paper, whereas an article in a technology trade rag will have fewer facts but more interpretation (shading into pure sales talk.) Unfortunately, no-one will spell out for you where these limits are, until you give them a sample of your work—*then* they'll tell you.

An Approved Communication

The existence of desktop publishing (and cheap offset litho printing) means that lots of things get typeset and printed that previously might have existed merely as copies from a spirit duplicator. Printing something conveys a legitimacy of a sort but, if you try it, you will soon find that the greater part of publishing is getting people to buy the stuff you've printed. One of the reasons for publishing in an established journal is that people might read it (if not that many). So one level of approval is that of the publisher, who has legal responsibilities, and should at least believe that your paper is free of libel, seditious material etc.

Publishers are always saying that publishing is risky, and they need convincing of the potential[2] of something before they publish it. In the case of fiction, the publishers are easily convinced if the author is already a best-seller. But in technical journals, past reputation theoretically counts for nothing. Instead, there is a special procedure in place by which your paper is vetted by the editor of the journal—usually a cynical person—who sends it out to a few cronies to demolish if they can. This is called refereeing, and the tougher it is (folklore has it) the better is the journal and the more—not fewer—submissions it receives.

A Contribution to the Field

The word *novel* is a curious one for works of fiction that are often reworkings of tired old themes[3]. In the same way, the claim that all technical papers contain some contribution to their field is generous in the extreme. It seems impossible to predict precisely what degree of originality is actually required, but novelty is one of the things that referees often say that a paper hasn't got: even if they don't

[2]That's actually the *commercial* potential, of course, however heavily veiled.

[3]Usually *Jane Eyre*.

A Contribution to the Field

really know themselves, and I'm afraid that sometimes they don't.

The very volume of technical publishing makes this requirement hard to meet when everyone's heart is in it: and their hearts—and other vital organs—often aren't[4].

A Context

Although omitting references is one way to reinforce an air of novelty, a technical paper is most unlikely to be published if it has no references at all: anyway, it doesn't look right—you'd probably get it back with a note saying that some pages were missing.

References are meant to be citations of other works which set your contribution in context: the foundation, kindly supplied by others, on which your edifice is constructed. Saying that is like saying that your head is there to stop your neck looking funny; like your head, the references have other functions and can be a Rosetta Stone of coded messages from the authors to the *cognoscenti*. (For details of some codes, see Chapter 9.)

Different Flavours

There is not just one flavour of paper. Papers can be 'spun off' from technical endeavour at many stages, from the time when you're thinking of something worth 'researching', until long after there's a something in every home, or much more likely, until it's finally obvious to everyone that you've been chasing a mirage. Publishing a paper or two at every stage of this rake's progress is the raw material out of which a whole row of publications (see Chapter 7) can be constructed. These have various recognized forms.

The Bibliography

A bibliography is just a list of papers in a particular area: typically a few hundred. It is naturally the first thing to be compiled by

[4]Almost all papers are written as though they do contain something novel, however. Using the language of discovery is a condition of entry, like having the necessary number of cornflake packet tops; if you don't, your slogan will never win a prize.

a well-organized person who is becoming interested in a new topic. However, journals recognize that their readers are so lazy—"short of time" is their phrase—that they (or at least their libraries) will pay money to see lists of references in a particular part of a specialism. In fact, the publication of such a paper often attracts more notice than (yet another) 'original' contribution. In an annotated bibliography, some comments—usually keywords—are hung off each reference. It is possible to spend time thinking about how best to select and classify papers, but not all bibliographies show evidence that such thought has taken place. However, a good bibliography deserves attention; it is a useful handle to a subject area. For instance, new students can be given a recent bibliography as their Baedeker for a certain acreage of literature, thus allowing their supervisors to conserve valuable mental energy.

The Review

This covers a smaller area than a bibliography, but in more depth; it is naturally the next step of a bibliography writer who is now focusing his attention more narrowly. A poor review paper contains a paragraph about each of a few dozen papers; and each paragraph is little more than the Abstract from the paper. A good review paper makes a proper *comparison* between a number of papers, by cutting through the extravagances of the original authors and applying common terminology and notation. The advantages and disadvantages of the various approaches can then be discussed 'on a level playing-field'; and finally, the most useful product of such an exercise can be one or two *tabulations* of the features of the work being reviewed. Writing a good review paper can establish you as an authority without the need for you to think up any revolutionary new ideas. If your strength is clarity and persistence rather than manic flair, then this is a good form of publication to specialize in. But it is not quick.

The Speculative Paper

Having reviewed an area of a subject, a natural next step is to speculate about what further developments might be appropriate, and how they might be achieved. To this extent, the speculative paper follows the review paper, but in another way it is the extreme

Writing the Speculative Paper

opposite, containing only new ideas instead of only old results.

The speculative paper can be used in a rather bogus way as part of a series of papers to present rhetorical speculations which will subsequently be brilliantly fulfilled by the Authors (see Chapter 7). Alternatively, if you are the sort of person who has more ideas than you have resources to exploit them, you can try to capitalize on your mental fertility by putting out speculative papers[5]. That man da Vinci still gets a lot of credit for 'inventing' the helicopter, although his design wasn't even a good umbrella; and the Chinese had got there already[6]. I've seen plenty of people credited with 'firsts' in computing on the same basis. Having speculative papers refereed is a chancy business, though, as you rely on a certain empathy with the Referees. For every Referee who thinks you have amazing vision another will recommend publication in *Amazing Tales*.

The New Theory or Algorithm

Now we come to what is perhaps the classic academic paper. This is a speculative paper plus just enough algebra, or enough results, to show (you hope) that you aren't talking through your hat. It's also the classic form in which to publish if you wish to keep something dark for a time while you work on it a bit more. To do this you ensure that the theory or results are just detailed enough to back up the speculation, and not detailed enough to allow anyone to reproduce the work: at least, not at all easily. This should be totally inadmissable in a *scientific* paper, as the methodology of science requires results to be reproduced, but it seems possible to get away with it in the jungle of many technological subjects, where 'systems' are built one upon another, and *full* details—if available—are too bulky for publication anywhere.

Results

Experimental results are of course a fundamental ingredient of real science. Even in computing, some examples of how a new algorithm or approach works are often highly publishable, even if the original

[5]If you've got *a lot* of far-out ideas, apply for a column in the back of *New Scientist* or *Scientific American* instead; they pay more.

[6]They made helicopters as toys: arguably a good example of oriental wisdom.

development was not yours: much better if it was, though.

Also, *really* good review papers will present figures for processing the same data in a number of the different ways that have been put forward in other people's 'original' papers. This sort of thing requires a horrifying amount of work and is as rare as a shower of frogs. That's probably just as well, as it allows everyone to continue unhindered in painting their own techniques in the most favourable colours.

A Practical Implementation

This sort of paper describes an idea actually built into a usable system. In some disciplines we have just crossed the divide between science and engineering (e.g. from the study of the physics of liquid crystals to the mass-production of LCDs for pocket calculators). In other areas—software again—this divide is not so obvious; there may never be a frozen product, as we do our research at the customer's expense, sold to him as upgrades.

Not very original algorithms and results papers are quite often found masquerading as practical implementations. In this way it is hoped to avoid scrutiny and also to be seen to be doing 'commercially relevant' work. The tell-tale element of these papers is that the system is always being used "in-house" (horrid phrase!), and soon to be "made available" to a hugely impressive clientèle of aerospace companies etc. Whether it's available or not, they rarely do seem to get around to using it.

The 'Experiences With' Paper

Finally we get down to papers that deal with topics like "Using spreadsheets to track spacecraft orbits". The Authors invented neither spreadsheets nor spacecraft; the originality in the paper lies in the unlikely combination. Because they contravene the golden rule of presenting everything as novelty, not many of these appear. It's a pity, because genuine 'experiences' might put lead in the boots of some of the authors of the more theoretical sort of paper, and stop them drifting towards the stratosphere and quite out of sight.

4

The Right Time to Write Up

As well as being about the tactics of putting together a paper, this book is also meant to cover the strategy of publication. In doing this, it is necessary to tread a fine line. It would be folly—and also rather insulting—to try to tell any reader how to conduct their own research, or whatever other activity it is that—hopefully—leads to publishable material. But it is possible to exhibit a few general ways in which it *might* be organized, relative to the publication process. That is what this Chapter is about.

At this stage, we will assume that we are concerned with that—actually rather rare—event, when you have one good idea that looks as though, correctly developed and written up, it will make a single publication. This is muzzle-loading technology, but it makes it possible to develop these observations in two stages; we will come on to the Kalashnikov technique later.

Three things have to be done to write a paper: generating ideas or results, writing up, and finding out who got there first. Let's look briefly at each in turn.

> *Generating ideas or results:* in other words, doing the work. This is the area I said I shouldn't say much about. But it is worth recalling from the previous Chapter that there are many stages of a given piece of work at which you may have something publishable (see also Chapter 7). For the moment we will assume the standard art-form: a bit of newish theory and some results obtained from trying it out.
>
> *Writing up:* this is the actual writing bit. Let's call it *planning and writing a draft*, because, as you will see shortly, this document may have holes in to receive pegs which are manufactured subsequently. But we will assume that it is the bulk

The Literature Search

of the job of putting together a document—and usually also the hard part, even if it is the interesting part too.

Finding out who got there first: you would probably call this the literature search: finding out what anyone (including yourself) has done that might be relevant to your current *opus*, and then taking that as your starting point. It is the bit of the publication iceberg that you can't so easily see, but which allows the visible bit to float unsinkably above the water. But, in the manner of the hidden bits of icebergs, it can cause trouble[1].

The question that I want to address next is *in which order should these activities be undertaken?* Maybe you think that's obvious? Postgraduate students often do; or at least they accept the default

[1]That's a poor metaphor; the hidden bits of icebergs don't cause any trouble if you avoid them; literature searches can cause trouble either way.

suggestion put forward by their supervisors. Anyway, since we have only tabulated three activities, we can easily examine all $3! = 6$ possibilities. They can conveniently be grouped into three categories, arranged by what is done *last*. So here goes.

The Classics

The first 'classic' is perhaps the obvious approach:

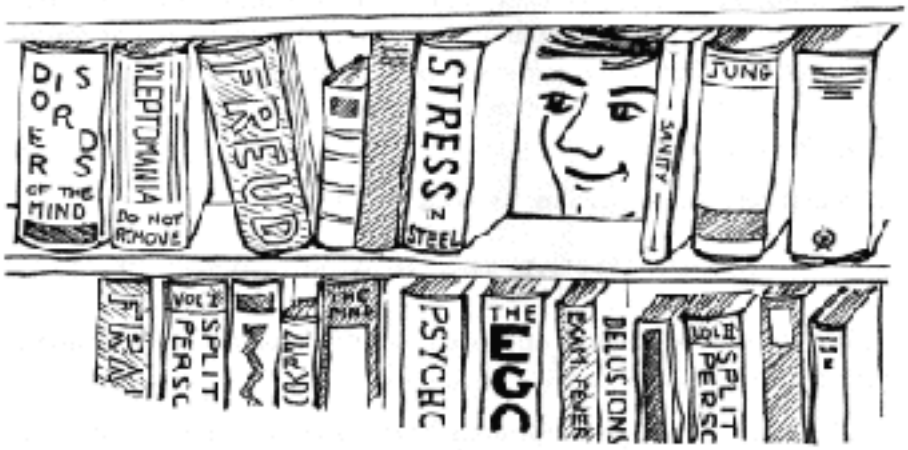
1. Search literature Do work Write up

This may be the obvious approach, but it actually has a couple of major disadvantages:

> Doing the literature search before anything else means that you don't really know how much Literature you need, and there's an awful lot out there. It's easy to be lured into thinking that you will eventually be exploring more avenues than is even vaguely feasible. You can always forget about your original idea, and write a review paper; but that contingency plan can hardly be counted as a benefit of the approach.
>
> Looking too hard at the literature inhibits innovation. The martinet school of PhD supervisor would disagree with me, but I think that this second problem is more serious. If you sit looking at ten or a hundred ways in which a problem has been tackled, it is difficult to avoid pursuing one of them.

In summary, this approach is only recommended for what I might call 'development' work, where a team of people are collaborating on a programme of work that has been more or less fixed by someone; the day-to-day 'research' is of the join-the-dots variety. (That's why this 'classic' is favoured by factory-farm PhD supervisors.) If the direction is so clearly in view, than a wide look at the literature is simply a check that things haven't been done already; and if they have, you try a slightly different approach. It's like devising workarounds to sidestep someone's patent: a demeaning but increasingly common way of passing the time.

The second 'classic' is also a well-tried PhD schedule:



Mis-shelved for the Day of the Oral

2. Do work Search literature Write up

This is favoured by more liberal—or lazier—PhD supervisors, and their students too. The idea is that you muck about for a couple of years, and then do a serious literature search to stock up a nice bibliography for your thesis. The problem is that it can produce one or more *big shocks*. In particular, it is possible to discover that *everything* you have done is neatly encapsulated in someone else's effort.

If this happens with a thesis, the honourable thing to do is to join the Foreign Legion; if it's just a paper, try again. A dodgier approach is to forget you saw the offending publication. If you think there might be a row on oral day, make sure it's out of the library and can't be consulted; say you lost it and pay the fine; it could be well worth it. At least be sure it's mis-shelved for the day of the oral examination.

If the previous paper was written by Cayley in 1876, you may feel hopeless, but antiquity is good news, not bad news as you might think. The older the work, the less likely anyone else is to have read it. Have you actually *read* Euclid's *Elements*? For the same reason, you can probably breathe again if the offending article is in Russian or Japanese. In any case, a cover-up frequently works

either with external examiners or with referees. I cannot approve, but I sympathize, because there are lots of other people whose stuff was just as unoriginal, but whose literature search was faulty, or was simply omitted. See No. 3 below.

If you are a compulsive experimenter, or—commonly enough—a compulsive writer of computer code, then you don't need anyone to make you take the No. 2 approach; rather, you must try to break the habit.

The Quick and Dirty

Now we come to two approaches which both involve searching the literature last, if at all.

3. Do work Write up Search literature

No. 3 is very common, and looking at the literature can be omitted completely. Done deliberately, this is either a gamble that the Referees know less than you do, or a cold calculation that—even if 50% of your papers are rejected—it's a more effective use of your time to go on churning them out than finding out things you didn't want to know at the Library. Providing you have at least some stock references to trot out, you may make a better score than 50%.

Speaking game-theoretically (which I often do) this is unfortunately a winning strategy. But, speaking severely, it corrodes the idea that a technical literature is a record of advances, and that research work should be funded by taxpayers who also have schools and hospitals to pay for.

4. Write up Do work Search literature

This sounds a little mad, but actually has two applications. One is legitimate, the other is not.

> It suits a speculative paper. Write up the paper cold, so that you are as little influenced as possible either by what you can achieve in practice, or by what's been done already. Then—if necessary—do a little work to explore your speculations, and finally see whether any or all of what you produced is old hat. This may seem like No. 3, but it's not *if* you know before you

start that you are working at the edge of the field. Writing the paper makes you put your thoughts in order and will hopefully lead to some 'advance'; which you can then try out. If you're not fairly sure that you are speculating in a new direction, then of course the method is unsound.

Also unsound, but not uncommon, is the use of this order of events to drag in journal or conference referees as your collaborators. Write a speculative paper but do nothing else. Add some space-filling references, and send it off to a conference or journal (the journal is better, because this approach needs a certain amount of time, and the conference deadline is therefore a problem). If it gets accepted, then that's a welcome fluke. Otherwise, you can expect two or three sets of Referees' Comments. If the Referees know what they're talking about and are conscientious, they will point out the flaws in your ideas: why they won't work, and who had them first. You then have a programme of work, which you follow. Send the results back to the journal, and you can't fail; you appear to have done an inordinate amount to accommodate the Referees' Comments. Naïvely, they are flattered, and accept the paper. So they should; it's good; they wrote it.

Some referees spot this and complain: so do some editors; and no-one likes it very much. Don't expect to be popular everywhere if you persist with this technique.

The Paperchasers

I call these last two methods paperchasers because they are the ones to adopt if your eye is more firmly fixed on the publication than on the work you are going to report in it. They sound like cynical procedures, and of course to an extent they are. However, even if you are wedded to a millenial plan of work *guaranteed* to bring about the robot research assistant by the year 2010, you may find these approaches useful for ideas you have which are oblique to your main project, but which you feel deserve publication. Indeed, the Paperchasers are definitely the best way to get a nagging "idea for a paper" into print and off your mind as quickly as possible. We start with the more focused of the pair; and very sharply focused it is, too.

5. Write up Search literature Do work

The idea is simple. Start by writing the paper that you would like to see coming out of a little piece of work. Dealing with 'Related Work' in the Introduction, and 'Results' in the Conclusion looks tricky, but actually it is a surprisingly easy process once you've thrown caution to the wind. What you are really writing down is what you think the state of the art is, and what you hope your idea will achieve to advance it. In fact, it's in writing just this sort of fiction that the benefit lies.

The same broad brush can advantageously be applied to the References. How many background references do you need? If you are applying a concept from one field in another (often a good Paperchaser formula) you'll need some references from each area. What are the aspects of your idea which may have been worked on already? Put down a couple of placeholders for each. How about your own work? You can easily find your References section is half-full in no time, *and* you have a plan of action to take to the Library.

Writing such an imaginative draft won't take too long, and obviously it shouldn't be polished at the start. The cynic will point out that I am merely describing a Plan of Work, or some such, which shouldn't be confused with a paper. Look it like that if you must, but by writing it out *as a paper* you get a much better idea of how far you need to develop the idea, how many examples will look appropriate etc. A free-form 'plan' is perhaps easier to prepare, but could be anything from a shopping-list to a proposal for a moonshot; you are not forcing yourself to decide what is adequate or necessary for a publication.

The second Paperchaser is like unto the first, namely:

6. Search literature Write up Do work

This is obviously the better plan to pursue if you have a brain-wave that's right out of your usual line of activity, and you've got no idea at all whether it's a Brilliant Innovation, Standard Practice, or (most likely) Fatally Flawed. It is better than the previous method if a quick literature search will decide between these verdicts on your idea. But don't forget that the disadvantage of Method No. 1

is back again, and this approach is not such a good idea if the thing you want to pursue is at all vague; in that case, go back to Method No. 5.

Thus far, Methods Nos. 5 and 6 have been presented rather fraudulently: for effect, I admit it. Of course it's actually necessary to revise the paper extensively at the end, slotting in references, results and conclusions as appropriate. Maybe you'll end up rewriting completely; but there's an enormous advantage in having that initial draft. It forces you to reconcile whatever the work and the literature search turned up with your original objectives. If—lucky you—there is now too much good material for a single publication, it should be apparent. And at the other end of the scale, if what you've got has been whittled right down by what you found other people have done, or the results of experiments didn't come up to scratch, you can abort the process early. Okay, the draft is wasted. But *without it* you could have spent much longer starting to write a paper that was certain to end up too thin.

5

Where Shall I Send It?

No-one's got anywhere near actually writing the paper yet, and quite right too; the next thing to think about is where to send it.

This is the moment to point out a Common Pitfall, into which I've fallen—or been pushed—into more than once. Publication *media* (to use a tired but useful word) can be sorted into two rather significant categories. Those that do, and those that don't, impose a deadline.

Conferences, Special Issues of Journals, and Yearbooks provide a deadline; if your copy isn't ready by a certain date (give or take extensions obtained by whingeing down the phone) then they won't publish it. In fact, many conferences provide much more than a single deadline. Often, when you receive a 'Call for Papers', there are three or four dates: typically, an Abstract By date, a Full Paper By date, and a Camera-Ready Copy By date: and of course the date of the Conference itself. It is extraordinarily easy to climb on to this conveyor. If your professional life is already full to overflowing (and it's very bad form to admit that it isn't), climbing on is one way of getting prodded to write up. Many technical authors seem to have a masochistic desire to be told what to do, and the bossier the conference flyer is, the better they like it.

That's fine as far as it goes, but these *flyers* are often like *fly-papers*: waving about in the breeze to catch the unwary. If at all possible, you really *should* try to make your own decision on where to try to publish, and set your own timetable.

So, here are some remarks on each of these 'media', starting with the sharks.

Conferences

Conference fly-paper is really double strength. Not only are the

deadlines fatally attractive, but you may actually fancy going to the conference itself. And in many institutions, the only way of doing that is to be Giving A Paper. There are ways around that. For instance you could offer a tutorial[1], or even offer to help on the registration desk! Perhaps the best thing is to be asked to give an Invited Talk, which can be much chattier than a submitted paper, and a written version is not usually required. But getting Invited can of course be difficult to arrange.

Another worrying aspect is the quality of the Conference Proceedings. Sometimes these don't exist: participants are just handed paper copies at the door. Sometimes they're a roughly bound volume of copies, each paper set out as the authors fancied. Sometimes there is a proper edited book. Sometimes the Proceedings are published as a special issue of a journal[2]. *Sometimes* the Organizers tell you that the Proceedings are going to appear in one form and they appear in another—or they don't appear at all. And sometimes only a *selection* of the papers are chosen to be reprinted in a special issue of a journal, leaving the rest in limbo.

So, Conference Proceedings can vary between something nobody reads and is not available afterwards[3] to an edited book[4] *and* a throw of the dice on getting into a Special Issue of a journal. All Conferences are *not* created equal.

And finally, how about your talk itself? One of the reasons given for going to a conference is to have an 'opportunity to present your work'. But, how many people will actually be there[5]? How many

[1] Condense some lecture notes, if you've got any suitable ones. But be careful; some conference organizers have designs on the copyright of (your) tutorial notes. Unlike papers these are re-usable and should not be given away lightly; watch what you're signing when you sign-up to give a tutorial.

[2] If you are 'into' computer graphics, you will be familiar with the ACM Siggraph Proceedings which appear as a special issue of the ACM journal, *Computer Graphics*. This a rather curious publication; for the rest of the year it's rather low-key and carries mainly internal Siggraph stuff, and then this one issue is the summit of most graphics people's aspirations.

[3] So in effect it can't be cited—why did you bother?

[4] The really nice edited books contain transcripts of the discussion, but they are not very common nowadays. I did one once; it took *ages*.

[5] On the other hand how many people read journals? See the next Chapter.

of them are likely to be interested in your field (important if it's a large 'composite' conference)? Will there be parallel sessions? And will yours be on at the same time as Sir Isaac is speaking in the other room? Further, suppose your presentation is the last one. The scheduling is normally outside your control, and giving a final presentation can be like watching the Farewell Symphony[6]. *And*, because your talk was the last one, everyone goes home immediately afterwards and so you get no opportunity to capitalize on it in little sales talks in the Bar.

Yearbooks

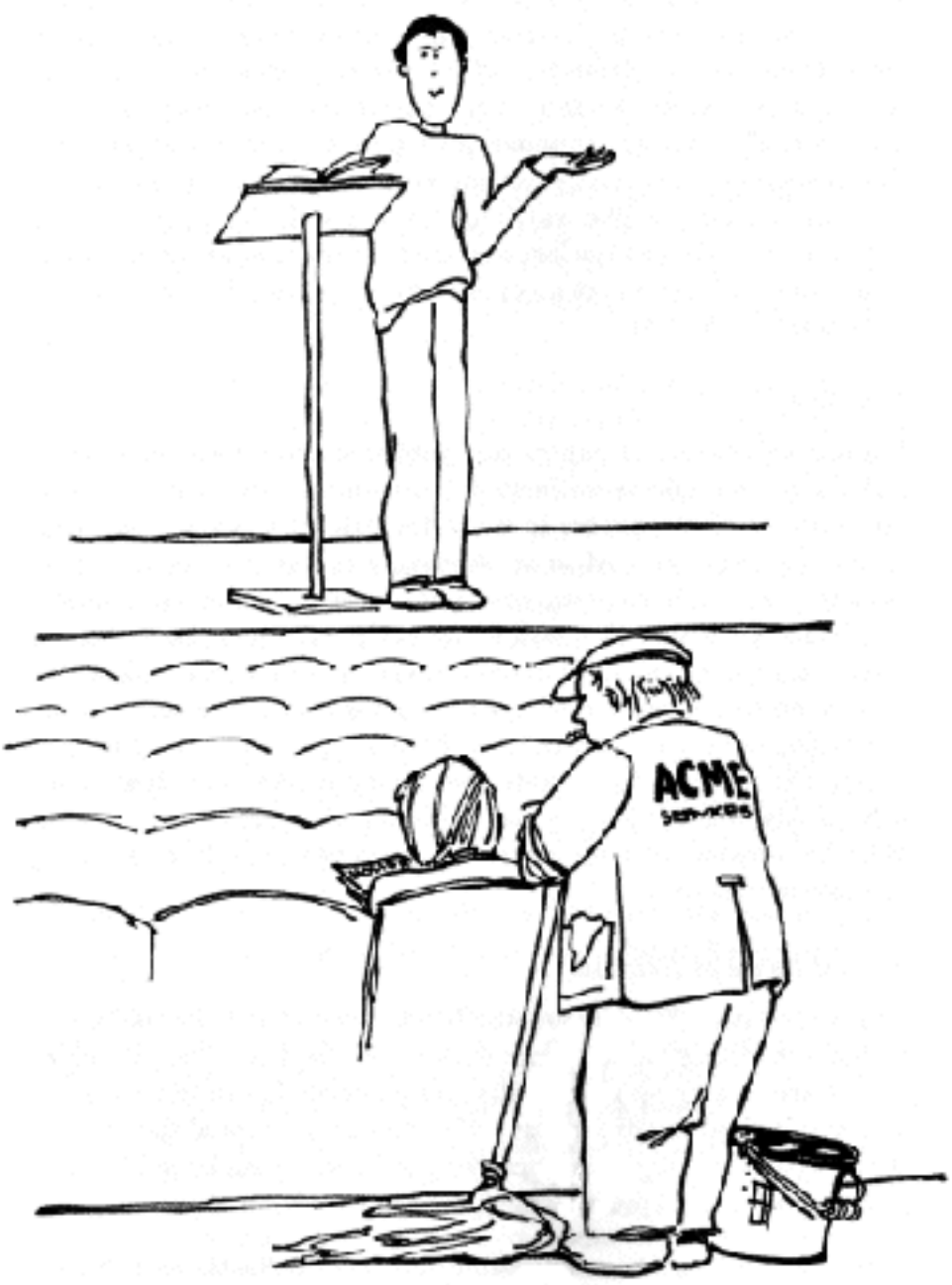
Various collections of papers are published from time to time. I call them Proceedings without a Conference. The worst of them are re-issues of old papers in bulk; the best of them are new contributions commissioned so as to make a coherent volume. Unlike conferences, and like books, you might receive a small fee. And if a yearbook appears at all it will be properly published; the publisher presumably wants to make money and therefore a yearbook has to be a commercial venture, not just an unavoidable expense incurred in running a profitable conference. Even so, the edition may be very short, if it is aimed to saturate the library market and then to sell out quickly without leaving the publisher with stock on his hands. Whether anyone actually *borrows* a given yearbook from a library is another question.

Special Issues of Journals

These are rather like Yearbooks. It's decided that a Journal ought to 'do something' about some part of the field it covers in which people are kicking up a brouhaha. So someone known to be a prime kicker is invited to edit an issue of the Journal devoted to this fuss. A call for papers, similar to a conference flyer, is then issued (usually as an advert in the journal itself, for economy).

This is quite a good compromise. Hopefully, the journal's refer-

[6]You *may* be put last to encourage people to stay by your hortatory brilliance, *or* because the organizers know that they'll never stop everyone leaving 'before the rush hour' anyway, and you're the human sacrifice. All men are liars, and you may never be sure which was the real reason.



Suppose Your Presentation is the Last One....

eeing procedures will be cranked up for the occasion, so you should be on a conference-type schedule. But you get publication in a journal—maybe even a good one. And there is one extra advantage. Unlike most conferences, the number of papers accepted is not necessarily fixed because, if there are some left over, another issue, or part of one, can be turned over to them.

That's enough about media with a deadline. Now we'll look at books, ordinary issues of journals, and reports. Although they require more 'push', if you have a Publication Strategy (see Chapter 7) then the flexibility in timing becomes a positive asset.

Note in passing that books, journals and reports also all constitute, or provide, a source of promotional handouts. Conferences usually don't generate anything convenient, unless you care to hand out copies of the whole Proceedings, which is not cheap.

Books

Everyone knows that books take a long time to write[7]; at least that's what everyone thinks. But a short book or monograph can be shorter than a very long paper: certainly shorter than a blockbuster series of papers. In theory, and at the start, books are very much under your control as regards timing and format, but you find that you quickly cede control when you get into bed with a publisher.

You usually get paid for books, and publishers have their own sort of fly-paper, called the advance. This sounds good. They pay you some money before the book is published, maybe before you've even started it. The problem is that they make you sign a long and involved contract[8]. (You also have to fill in a Proposal Form and a Promotional Form, *inter alia*: in fact quite a lot of stuff.)

[7]The late Isaac Asimov comes to mind as one of a band of amazing book-a-month authors who were obviously never told this.

[8]Which you should read very carefully, and argue about if necessary—publishers expect this, but are of course relieved if you don't. I should watch particularly what is happening to foreign sales. You may find that—when your book is re-published by a foreign publishing house—you are splitting the royalties with your own publisher. This is annoying, but fun for publishers, who *acquire* books on these terms as well as selling them. It is best to select a publisher with a significant operation in the part of the world where you think the

After all that you are tied to letting your particular publishers have the book. (If you write fiction, they also try to sign you up for the next book, but I've not yet heard of such a thing in technical publishing.) Academic and technical books from reputable publishers are often refereed, like papers, and this can be a painful process if the referees cut up rough and suggest changes that you don't want to make. You realize then that you can't withdraw the book from the publishers as easily as a paper from a journal.

My advice (obtained with some pain) is, if at all possible, to avoid the flypaper, write up the book as you want it, and then hawk it around as a completed manuscript. But in any case, with all this going on, you will realize that getting a book published can be slower than writing it.

Journals

Some journals are very prestigious places to have your work published; they also have long waiting lists[9]. Other newer ones may or may not be a good place to be seen, but are sometimes at least a bit faster. There are also some 'letters' journals which specialize in publishing 'early results' quickly. Many journals reject a large proportion of submissions (accepting only one third is quite ordinary), so it can be something of a lottery.

One problem with journals is that, because there is no deadline, arguments with referees can be extremely protracted. They can in fact go on until the Editor calls "Time" and throws you out as No Longer of Current Interest. Since there are so many other papers to choose from, there's nothing much stopping this happening. At least

main market for your book will be.

[9]There is also the *possibility* of having to pay 'page charges' for publication. I have no direct experience of this, but understand that some journals literally charge per page. This has the smell of vanity publishing, and you also have to find the money somewhere. I guess it should be avoided unless it is the usual thing in your field.

I have come across other, and more subtle, ways of charging. A fee for 'colour plates' used to be common, but improved printing technology has made that much rarer. You may still be charged for large changes to proofs (see Chapter 8) and I have seen attempts to cadge 'sponsorship' from (industrial) participants, but that's usually for small conferences, rather than journals.

with conferences there's a timetable, and with books, the publisher has an interest in getting his advance back.

When you look at submitting to a journal, you should try to find out about two possible sources of delay. One is in refereeing. This is erratic, so even if the average time is short you may be very unlucky indeed. The other delay should be more predictable: the waiting list of accepted papers[10]. This is perhaps a less obvious problem, as a paper "Accepted for Publication" is, for many purposes, nearly as valuable as one that's actually appeared. But, if you have a series of papers to publish, an early paper stuck in a journal's queue for printing can be a significant embarrassment. Some journals occasionally give summary statistics of times to publication (usually if they're doing quite well). You can also find out about these things by looking at 'Received' and 'Accepted' dates which are at the head of the papers in many journals. Failing either of those sources, you could ask the Editor (who will probably give an evasive answer) or some colleagues who have been published in that journal recently. But (just like the warnings on advertisements for financial services which say that "the value of investments can go down as well as up") journals' waiting lists can change: caused perhaps by a change in editor, in editorial policy (deliberately rejecting more papers early on), the launch of a competitive journal or conference, or simply a fluctuation in interest in that field.

Reports

Technical Reports are essential to a publication strategy. In the UK in particular, very few people understand this. The reason that reports are essential is that they are the only way of getting a toehold in the literature *exactly when you want*. Providing that you have a Technical Report Series that is (informally) recognized, then other people *will* cite your papers in report form: so you can get your work out with a nice early date[11]. But the payoff comes when you

[10]It's not *that* predictable; if your paper happens to have the right number of pages to fill an issue, you can jump the queue quite nicely or—conversely—if it never seems to fit you can be kept hanging around: that's the reality of issues having to be multiples of eight pages, or whatever corresponds to the size of sheets that the printer's presses take.

[11]Dark work at the crossroads in respect of Technical Report dates is not

want to submit your paper to a journal because, and unlike any other sort of publication, it is traditional not to regard the production of a report—however widely advertised—as a bar to further publication. (The rationale for this is a mystery. Like the Tooth Fairy paying for teeth, or Father Christmas bringing presents, it is probably wise not to probe too deeply, or it will stop happening.)

So you really get it both ways. In fact, some people simply send their report off to a journal in its original cover, and most journals *will* accept this (although I think it is slightly rude to make no attempt at re-packaging[12]). In fact, in acknowledgement of this process, the odd university department actually calls its reports "Preprints": a splendidly pushy title. You can also—especially if you work for a firm rather than an academic organization—claim that having published your paper as a report prevents you from assigning copyright to the journal. You foist a "right to publish" letter on them instead, and thus keep your options open. Yes, it seems most journals will wear this one too.

The only problem with this Nirvana is that someone has to invest a little time to create a credible report series. You need:

> *A house style*—pretty covers and so on—but there's no need for any proper bookmaking; photocopying and stapling or spiral binding is standard.
>
> *A catalogue* of reports which comes out at some regular interval, and is mailed together with a little postcard on which people can request copies. You may want to charge money for your reports: to cover expenses, so that people value the

unknown. In particular, you may find a report of interest on a list that you have been sent with a publication date of some months past; but when you ask for it it is not available. Later it *is* available—but the date has not changed. This illustrates what a powerful weapon a report series is and—like all powerful weapons—it can be Used Wrongly.

[12]Leaving the name of the last journal it was sent to is another poor sales tool, although amusing for the Editor.

There is also the more general question of dating manuscripts. Some people make the date of submission, or some—nominal—earlier date of completion, prominent on the manuscript. They hope that this will act as a reproach to Editor and referees, and hurry the process up. But if there are problems in refereeing, that backfires; the paper starts to look like 'old work' and may not be taken seriously.

reports, or just to look good in one of today's market-driven Universities. In that case, you may as well have a form instead of the card, because the requester will need to use an envelope anyway. Expect fewer replies in this case: not because of the money, but because of the hassle.

A mailing list: this can be started by pooling likely names with your colleagues, but it should subsequently be self-propelled, as people ask to be added or removed[13].

Someone who does the mailing, deals with the postcards, makes sure the stock of each reports is kept up (each report in its own clearly labelled boxfile, so *you* can get at them too) and handles the money if you decide to charge any[14].

Normally, plans falter on the last of these items. But it's a false economy in the budget of any department that's in the business of publishing papers—like buying an expensive hi-fi and connecting it up to a speaker taken out of an old transistor radio[15].

[13]Advertising the reports may seem unnecessary in achieving the rather abstract goal of "getting a toehold in the literature". But if you keep the existence of the series more or less a secret then, even though the reports really *are* produced and nominally available, your elaborate strategy may simply be ignored.

[14]There is also the question of what to do when demand seems to have died out, or all the copies have been sent out, or both. You can have a rubber stamp saying "Regret Out Of Print", or some such insult, and stamp people's letters and return them. If the report has subsequently been published 'properly', then that's the correct move. Otherwise, the problem is that the person who does the mailing may misjudge the correct time to start using it, and your report will get a reputation as Impossible To Obtain and thus not worth citing. A rubber stamp that implies an eventual reprinting, but is unspecific about a date, is safer for you, if frustrating for the recipient. Something like "To Be Reprinted—Please Re-Order" is particularly annoying, and does not oblige you to contact the enquirer again if you don't want to.

[15]The Empire Should Probably Strike Back Or At Least Make An Effort: this is a special appeal to my UK colleagues. Do pull your socks up, chaps.

6

Reprise:
Dr Excellent Publishes a Paper

The contents of this book so far may be criticized as descriptive, not to say vague. In this *Reprise* we look at what someone might actually do if they were determined to extract the maximum benefit from publication of their work. Dr Excellent[1] is such a person. She does not have many brilliant ideas—in fact none at all—but she is determined that almost everyone will hear about the rather ordinary ones that she does have.

Let us suppose that she has had one of these moderate ideas, and it's a gobbet the right size to make into a single paper. Its first appearance in print was actually some time ago, just as soon as she had thought of the title, when she added an entry to her CV, endorsed "In Preparation". However, today she has finished writing it up. What does she do next? Here is a possible train of events.

Just as soon as her laser-printer output has cooled down, Dr Excellent gets her paper issued as a Technical Report. How? That depends on where she's working.

> Probably, she's got the sense to be working somewhere where they already have a strong Technical Report Series, and someone in her Department is busy accepting five-dollar bills and

[1]Dr Excellent crept into one of the early drafts of this book, and my wife liked the persona and insisted that I keep it. Dr Excellent is not, of course, a representation of any real person, alive or dead, including the Author (who was more or less in the former category at the time of writing). In fact, Dr Excellent is so methodical and unlike me, that I made her a 'she' to emphasize the contrast. That also counterbalances the occasional unfashionable use of the pronoun 'he' elsewhere.

Dr Excellent and Friends

sending out reports, and also mails catalogues from time to time.

If not, she set a Report Series up herself, to the admiration and amazement of her sluggish colleagues. A little while after she'd done this (and since her publication rate was so good) she got someone else to run it: as an administrative job to make up their workload.

If her Manager or her Head of Department is dead set against 'wasting money' in this way, then she gently sponges into someone else's series. Maybe that of another department (this could be what is necessary to change her boss's mind), or a department in another university at which she just did a sabbatical; or maybe she sets up an embryonic Learned Society with some colleagues and they have a report series of their own.

Anyway, off the report goes to the printers. Usually, the copies can go straight out on to the streets as soon as they get back, but sometimes Dr Excellent believes the idea encapsulated in her paper

is so good that her institution will want to patent it[2]. Or perhaps she works for a firm which insists on all publications being vetted. Or possibly the project was collaborative and the collaborators have insisted on a say in publication.

Now is the time to cudgel these bureaucrats into action. If the reports come back from printing and *still* nothing's happened, she can circulate a little note saying "YOU are now the only person holding back publication" etc. etc. This is a bit of fun[3].

Now the Report is out, Dr Excellent starts thinking about a 'proper' publication. She may have a journal or (less likely) a conference in mind, but she knows that, once committed, the paper is to an extent on autopilot. When she started in this job, she was given a list of places in which to publish, which made the choice easy. She started with the journal which, she judged, had the most favourable combination of being likely to accept the paper, and speed of publication. She sent the first one there, and sent subsequent ones to the others using the same formula.

Now, all that nonsense is behind her. She is after *fame*. So, she does a little analysis. In her desk drawer she keeps a list of possible homes for papers—journals and decent conferences. For each of these, she has made an estimate of two numbers. Both are impossible to know, but not too difficult to guess:

> The first is the number of relevant people that the journal or conference will reach. *Relevant* means relevant to Dr Excellent's field. Conference proceedings may well be read by very few people, but they'll probably all be people whom Dr Ex-

[2]It never is; but it's impossible to patent an idea that's already been published, so you have to wait for that avenue to be explored first. In Dr Excellent's case, I'm afraid it usually ends in the discovery of what patent people call 'prior art': which is not in fact a picture from a monastery, but someone else's idea that's so close as to disqualify hers from being patented.

[3]The bureaucracy-sympathizers among you will probably point out that the whole point of looking at Excellent's paper is so that anything sensitive can be changed *before* printing. Well, that is sometimes necessary but, ninety-five times out of a hundred, the bureaucrats just want to have the thing come across their desk, so that they can tell the super-bureaucrats "Yes, we approve all publications". If the worst comes to the worst, and something has to be changed, repairs may be possible. What's the price of a few hundred reports compared with the true cost of the work in the first place?

cellent would like to reach; very specific journals are similar, but might have a few more readers. General or Learned Society journals (e.g. general computing journals such as *The Computer Journal* or *ACM Computer*) will have many many more readers: but fewer relevant ones. Everyone can make their own estimate. Circulation figures are a help. These are usually obtainable from publishers (if necessary by a bogus enquiry about advertising), but I'm afraid to say that most people never bother.

The second estimated number is the kudos that attaches to publication in that medium. What units do you use? Dr Excellent selected a very prestigious journal in her field (a prestige general journal like *Nature* fulfils this role over a wide range of subjects), and then awards marks to all other forms of publication as a fraction of that.

One amusing way to guess these numbers is to get a few colleagues together for half an hour and get them all to make their own lists. The pooled and averaged information is probably rather valuable (see the Conclusions).

Dr Excellent then works out a third factor; for each outlet, she guesses the likely time to publication, and subtracts it from an approximation to the shelf life of her manuscript. She will have an idea of the reputation of various journals for celerity, and of course she will know conference dates.

Finally, she makes an estimate of the chance of being accepted. Different journals 'like' different sorts of paper and the result should be weighted by the subjective value of this match. Some of the journals on her list are on the fringe of Dr Excellent's subject area, but there may sometimes be a good chance of them accepting the paper nevertheless. For instance, Dr Excellent may be well aware that the subject of her paper is slightly old hat in her own field, but just starting to penetrate a related field. A lot of this 'publication by relocation' goes on[4].

So the whole sum is:

[4]In my own area, the no man's land on the borders between Computer Graphics and Computer Vision is a wonderfully fruitful place for publication by relocation. The same geometric concept, in particular, grows up as a sturdy plant in each, but a different terminology is used. Of course, everyone is attached

$$\begin{aligned}&\text{Number of relevant readers}\\ &\times \text{ Kudos}\\ &\times (\text{Shelf life} - \text{time to publication})\\ &\times \text{ Chance of acceptance.}\end{aligned}$$

Dr Excellent does this multiplication for each journal or conference. A little thought about the result, then off the paper goes.

> At this stage, it may be worth considering strategies in case the paper is (rapidly) rejected; for instance, if the journal Dr Excellent first tried is being slow, she might have a note in her diary to withdraw it just in time for the submission date for a good conference.
>
> When the paper is accepted, Dr Excellent changes that entry in her CV from "In Preparation" to "To Appear". She is extremely efficient in returning proofs. When asked for a potted biography, she is careful to look at the ones adorning other papers in the journal, and adopts the same style. Some of these biogs seem to emphasize career to date, some emphasize previous publications, others emphasize the societies of which the Author is a member. Whichever sort is required, Dr Excellent's is just a *little longer* than the average, and certainly more impressive. She is also asked for a photograph. She sends one in which she looks devastatingly intelligent, but also not unattractive; it *was* taken six years ago, but of course she has not changed at all[5]. In sending all this stuff in, she finds it necessary to speak on the telephone to the journal's Production Controller, or someone similar. Subsequently, she can use this contact to make polite enquiries about progress, if publication seems to be taking a long time.

---

to their own terminology, so the successful exporter/importer must be a bit of a technical linguist.

[5]The question of exactly how old a photograph to send is complicated and beyond the present scope. However there is definitely a limit. Extreme cases, in which a photo of callow youth adorns biographical notes which show the Author to be a Senior Tutor, Dean of Faculty, Head of Laboratory, Director of Divison, or indeed all of these and more, are certainly misjudgements.

> After the paper has been published, Dr Excellent deletes the phrase "To Appear" from the entry on her CV; but she doesn't forget the paper, particularly if it's any good. She will try to get good stuff into a Yearbook. On lucky days, she can even manage to get a conference paper accepted for re-publication in a journal; her line of patter is that it "went down very well at the conference, but because (...fabricate reason here...) not many of your readers are likely to have been at that event".

That's about it, until you come to bind it in with all the others to submit for your DSc. Good-bye Dr—oh, sorry—*Professor* Excellent.

7

Ambition: Writing a Series of Papers

Life is not conveniently cut up into single-paper slices. Often you will have a couple of related ideas, or a piece of work opens up a number of different directions. Or perhaps you are steadily 'progressing' a topic from concept to implementation. Maybe you are rashly moving with it: from a university to a commercial research laboratory and even out to make and sell it.

If you feel your work is worth a number of papers, of various different types as outlined in Chapter 3, how do you do it? Well, the main danger is simply waiting too long. There are so many wonderful avenues to explore, that by the time you've been down them all and written your *magnum opus* it has the unmistakably antique look of a brass and ebonite projecting galvanometer[1]. The other extreme is just as common; you publish the central idea in a distinctly low-brow conference and all your subsequent papers are hampered by having this embarrassing reference in their guts.

However, there are some people who rarely write a single paper; they fire them off in salvoes, or patterns, like depth-charges. The impact of individual papers may be variable, but the overall effect is one of remorseless probing and advance: a good example of the whole being greater than the sum of its parts. Commander Excellent is naturally a follower of this naval metaphor, and her tactics are:

- Get each aspect out as a separate report as soon as possible.
- Perhaps publish a speculative paper or two (never the 'main' paper) at lively conferences; make sure the proceedings will

[1] Although worth a lot less.

be genuinely available; keep the titles and contents of these papers distinct from the 'results' papers which come next.

- Submit one or two major 'results' papers to journals or *prestige* conferences.
- Methodically generate 'aspects of' papers—e.g. practical applications, variant algorithms etc.—and use these as pegs on which to hang the whole edifice at average conferences. Thus hammer home your achievements to the audience while the published papers look—well, at least adequately—different.
- Finally, you may well find yourself Excellently placed to write a book on the subject.

That is fairly straightforward. Because you are launching so many of these missiles at once, you must try to stop them hitting one another, by keeping titles, destinations and (where possible) contents as diverse as possible. But, at the same time, do not be tempted to economize on paperwork, and snappy titles, by writing 'two-in-one' papers. You may think that you have too many ideas to write them all up separately. But (I *know*) the two-in-one paper can be disastrous.

Suppose you have a moderately good idea, and you want to try it out by applying it to some problem. You choose an interesting but not mainstream problem, you apply your technique successfully, and publish just *one* paper: which reports both the idea and its application[2]. Then the original *idea* turns out to be one that you want to use again—and claim the credit for. But you find difficulties in staking your claim by referring pointedly to that original paper. It is obviously 'an application', and the assumption everyone will make is that it's an application of a previously developed technique. That impression will be reinforced if the first paper appeared in a part of the literature with which the application is more closely associated than the method. So, anyone who published just vaguely related work around the same time—but in the correct place—can easily take all the credit for this wonderful mainstream technique

[2]Yet more disastrous pairings exist, such as the hybrid technical paper and financial report. These birds are seldom seen in flight; that doesn't prove that they never leave the hangar, just that they rarely get off the ground before reaching the end of the runway.

that everyone's now using. Your paper is a footnote to history in the form of an 'early application' of *their* technique[3].

One possible solution to the two-in-one paper problem is to split the paper into two—or more—halves, identified as Part 1, Part 2 etc. But this constitutes two missiles on the same trajectory, and they will often blow each other up.

For instance, you might see two papers with titles like:

"The PANCAKE flatness tester—Part 1: The PANCAKE methodology"

and

"The PANCAKE flatness tester—Part 2: Issues of implementation".

Sometimes, such papers are in the same issue of the same journal, sometimes they are in one issue following another, sometimes they are in quite separate issues or even different journals. The originators of the PANCAKE system obviously found themselves with so much material that—they thought—they would get two publications out of it. Also, they wanted to separate idea and implementation (sound idea), and they thought that people would really come to believe in PANCAKE if they saw "PANCAKE" twice (or three times, by means of the ingenious 'Part 1' title above) on the Contents page of their favourite journal.

Well, if you saw these papers in print, they obviously got through, and there *is* a certain impressive insistence about this sort of arrangement; but I don't advise it, for three reasons.

> If you send them to the same journal, you lost the opportunity to differentiate idea and application as clearly as possible.
>
> It can make refereeing rather hazardous. If they go to the same journal, the Editor might send it out to just one set of referees. Then, usually, at least one of them will suggest that the papers are rolled back into one. If the Editor accepts that (which he's

[3]Your paper was published first? Don't expect anyone to notice a little thing like that, especially if there were several in the same year. And here's a subtle point; if your rivals published in a 'better' journal, people may mentally backdate their work somewhat in compensation for the time required for more 'rigorous' refereeing.

inclined to do, to save space), then bang goes your double; and a lot of time has been wasted, because the compressed paper is sure to need refereeing again. Or supposing (nightmare) that one referee says that they should be published together, and another says *one of them* should be rejected. Then you're stuck.

Conversely, if the papers are sent out to two sets of referees, there's twice the chance of the usual delays. And, again, if you (or the Editor) has set your heart on them both appearing in the same issue, you've got to wait for the paper that takes longest.

Alternatively, you might send them to separate journals. But, you will presumably reference each paper from the other. Thus, because of the form of the titles, you can hardly fail to focus the Editor's and Referees' attention on the other paper. So, they think "Are these papers a disguised attempt at double publication?", or just "Why didn't the author send them both to my journal?". Both journals will probably ask to see the paper that's going to the other; result, more delay and confusion; and if Part 1 is eventually rejected and Part 2 is accepted, it looks rather silly. The references from the part that *was* published to the other part will actually point at nothing (except, accusingly, at you the Author).

Even if you're lucky and both papers are published together or in consecutive issues of the same journal, you may still lose out when (and if) they are cited, because you're quite likely to see:

"The PANCAKE System" (Part 1: Methodology, Part 2: Implementation), *Journal of Inferior Studies* **32**,1 (14-23) and **32**,2 (37-48) 1992.

Serves you right! The answer is simply to keep the papers quite separate, in this case probably by dropping PANCAKE out of one title altogether, e.g.

"A double-gradient method for planarity measurement", *Incomprehensible Theory* **XIII**,47 (1002-1015), 1992.

and

"The PANCAKE system for flatness testing", *Oily and Greasy Engineering* (17-28), July 1992.

So here's the message again; to keep a series going, make the titles, destinations and contents of each paper as different as possible.

8

Getting Accepted

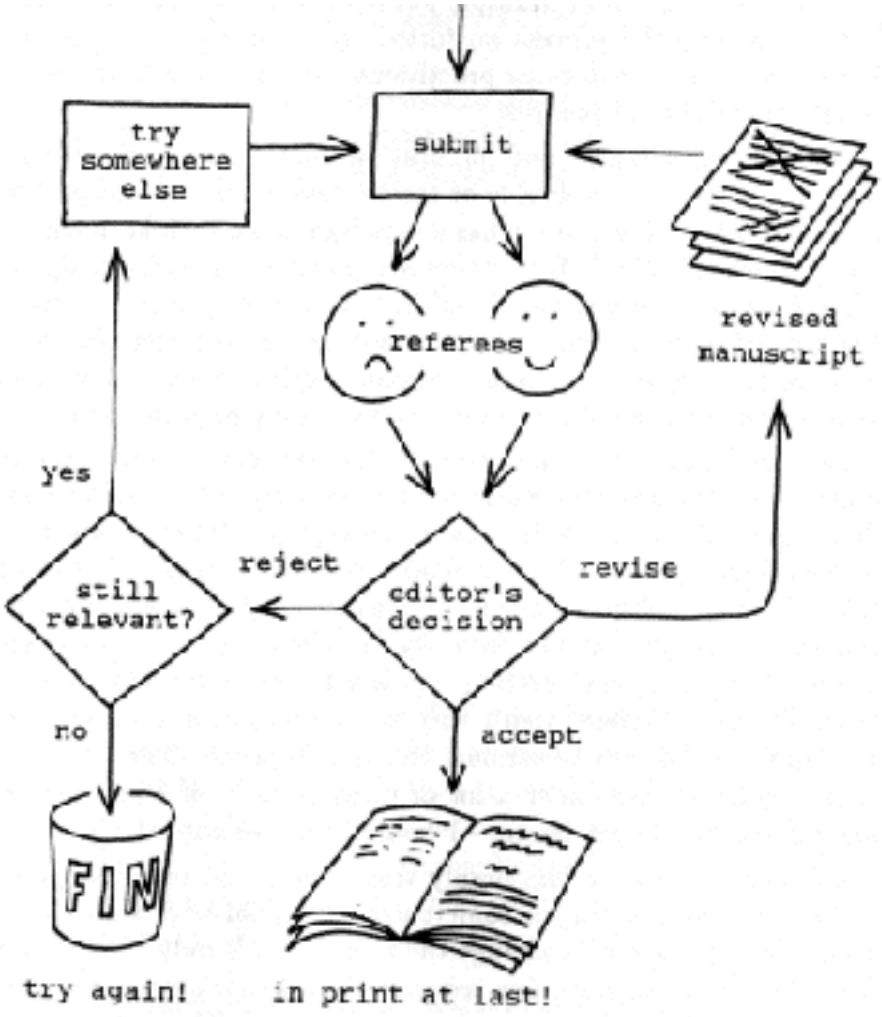
Once you have chosen a journal or conference to favour with your paper, you send it off to them. Is that all? Not really: your paper is now about to be 'processed', a treatment from which it (and your ego) may emerge battered, if at all. This 'process' has a number of phases—Submission, Refereeing, Revision, and Acceptance or Rejection (the figure which follows is an obvious little flowchart). 'Processing' is a game with a number of players, typically one Author, two Referees, one Editor and one Publisher: although the last of these is not usually an individual, and there may be a different number of referees[1]. The editorial function can also be split between an Editor and an Associate Editor, or a member of the Editorial Board: there are an endless number of fine distinctions and fine names that these people call themselves[2]. Let's look at these stages in turn.

Submission

Some people suggest submitting a preliminary proposal to the Editor of a journal, before writing the paper. I don't think that's a very good idea. You are likely to get a rather non-committal reply, but if you do get advice and follow it, you could end up with a paper

[1] If the number is zero, then your paper may be published awfully easily (like this book), but such publication is unlikely to be widely read or admired (which is where I hope the parallel ends).

[2] My favourite is "Editor-in-Chief"; it is impossible to think of him without a hat full of feathers, while mere "Editors" have only one feather apiece. If the supremo's job is shared, there is a natural reluctance to revert to the single feather, so journals with multiple "Chief Editors" are not unknown. That is inflation. I look forward to seeing a journal appoint a "Super-Chief".



Paper's Progress: one of those Unfashionable Flowcharts

that's not saleable elsewhere, if things go wrong. An alternative—although I must say I've never seen it—would be to write asking whether "the enclosed *finished* manuscript *would be of interest*". If the Editor condescended to reply to this (rather than sending it on to referees willy-nilly) then his reply would be useful, especially if you had another promising destination lined up. If you got discouraging noises, you might not be able to tell whether the paper really wasn't relevant to that journal, or whether the Editor had taken against it. But either way, it would be unlikely to get in, so you could then try your second choice, having saved a lot of time.

All journals, and most conferences, have elaborate 'Instructions to Authors'. I shouldn't, but in honesty I have to, say that unless you know that the journal or conference really insists on this stuff, you shouldn't take too much notice of it. Most journals will accept papers in any form—even bound technical reports—and the changes to accommodate 'house style' can be made later, when you've been accepted. Obviously, if the paper is rejected, it's just been a waste of time, whether or not you subsequently send it elsewhere. (Exceptions to this rule are conferences with deadlines shortly ahead of the event, where your manuscript—if accepted—will be used as camera-ready copy.)

In the same vein, papers and conferences give elaborate instructions about illustrations, involving "high-contrast black-and-white prints mounted on thick card" and heaven knows what else. Again, there's usually plenty of time for all this later, and if the paper is a long time in refereeing, I'm afraid that it's not unheard-of that the carefully produced high-contrast prints get lost or damaged.

However, I *do* suggest that you supply the number of copies requested: firmly stapled, too: to Hades with the Air Mail cost. If you send fewer copies than you are asked for, you probably won't receive a complaint; the Editor will quite likely make more copies of your paper rather than face the chore of writing to you to ask for the right number. But he, or his minions, will probably do a sloppy job—out of pique. Referees will get nasty copies, especially if you've included any photographs or anything in colour. What's worse, is when the copies sent to the Referees are incomplete. They'll enjoy returning them: because it's a foolproof way of putting the whole business off for a month or so. That's not so funny for you.

Multiple Submissions

As an Editor, I have more than once found myself reading a paper in another journal and thinking that it looked familiar; that was because it was out being refereed for my journal. On other occasions, referees have tactfully asked whether authors would like to highlight the differences between the paper they are being asked to look at and this other paper that has recently appeared.... These are cases of attempts at multiple *publication*; a more subtle scheme is to parallelize the process no further than acceptance; once one journal takes it, the devious practitioner withdraws it from all the others as speedily as possible.

Multiple submission—and publication—do go on; it's an obvious way for authors to try double or treble their chance of acceptance, and extend their CVs; but it has a very bad name with both editors and publishers. Why? Well, there are possible copyright problems if publication is actually achieved in two or more journals; but, as I suggested, double publication may not be the objective. In either case, what everyone resents is someone hogging two or three times their share of the available resources: especially referees' time.

Because many technical papers are by their nature positioned in quite a small niche in their field, it is quite likely—if the Editors are doing their job—that it will go to the same referee from two journals. You can bet that that referee will holler; he or she doesn't like being taken for a ride either. Your name may be circulated around some murky cartels, you may be formally or informally blacklisted, and generally you may find getting published a more rigorous process than hitherto. If there aren't very many journals and conferences in your field, that can be serious. But in mitigation I must say that some academics are under a lot of pressure and—if I just needed one more paper to get tenure—I would be very tempted.

Of course, not everyone simply tries sending an identical paper to two or more journals or conferences. A trivial trick which gives a lot of camouflage is to change the title, and it's only a short step from there to changing a few sections. Surely that's what everyone does anyway when they write a series of papers? This is the sort of thing the cliché *a grey area* was coined to describe, and the eternal vigilance of the guardians of the media is strained, and can back-

fire. I have heard of people having papers which *were* substantially different rejected merely because they had rather similar titles and were submitted to different places at around the same time. 'Appealing' is difficult because the reasons for these decisions may be disguised and—as far as I know—there's no legal way to force a conference or journal to take your stuff, even if you fancied a court case. So, to repeat what was said in the previous Chapter, try and aim to maximize the distinctness of papers. If you *do* smell possible trouble, you can sometimes defuse the situation by sending copies of the related paper—whether reprints or manuscripts—along with the submission[3].

Of course, as I've also suggested elsewhere, there's nothing to stop you openly trying to get a second publication of your paper after it has been accepted or appeared in one place.

Refereeing

If this were a mystery story, the mystery comes now. Who are these Referees? Most journals have a number of regular referees who are sometimes called The Panel[4]. The names of the referees used by a journal or conference may be published in bulk as a fairly feeble acknowledgement of their efforts. But normally you won't be told who refereed your paper. Sometimes you can guess (especially if there is a list to limit the possibilities), or you may be able to recognize the handwriting, or the Editor leaves their names on by accident. (That's rather fun, but embarrassing for the Referee, if he let himself go.) Otherwise, you have no idea. Usually, the most annoying comments come from referees whose identity is a complete mystery. They could be someone's research student (students are often asked to referee papers by their supervisors), or someone who's not in your field at all, but is actually on the Panel to look at quite different sorts of papers; or maybe the Editor's Secretary got

[3]Some journals make you sign a statement which says that you *are* including all relevant papers; this is in addition to the more common requirement that you warrant the paper to be "previously unpublished".

[4]Editors get new referees by seeing who writes what, so when you've had a few papers published I'm afraid you will soon get *your* collar fingered.

Jones the aerodynamics expert confused with Jones the molecular dynamics expert, and Jones passed it on to a research student, who was too shy to say it wasn't in the right field, and did it anyway.... It is frustrating not to be able to resolve these teasing questions.

One of the worries people often express about the refereeing process is that they are giving an early view of their work to someone anonymous who may make unscrupulous use of it. In the extreme, suppose the Referee writes his own version and sends it off to another journal (run, of course, by a convenient colleague) while asking incredibly awkward questions and generally conniving at the delay of your paper, which is eventually rejected because this other paper has now appeared. Arrgghh! This is the stuff of rumour only, as far as I know; start worrying about it and you'll get less sleep than Macbeth.

However, I did once receive a letter from a research student asking for further details of a paper of mine. Such flattery is rare, but what was significant in this case was that the paper in question had not actually been published. It was in fact being refereed, and the student's supervisor was—you guessed it—one of the Referees[5]. That unfortunate episode was, I think, bad practice, rather than an attempt at highway robbery. While it demonstrates that untoward things do happen, it also shows why wholesale piracy as outlined in the last paragraph is not generally feasible[6].

Occasionally, you come across an oddity, like myself, who believes in 'open refereeing'; I tell the Authors who dun it[7]. Sometimes there is a little friction, but by and large I like this method, as it avoids misunderstanding. Also, I think I am more careful with *my* comments: the Authors know who the idiot is, I know they know; but this certainly doesn't stop me recommending rejection.

This approach has a certain self-advertisement value and may be criticized as brash, but at least it eliminates the temptation towards

[5]Of course I made a fuss, and the paper was subsequently published with unusual expedition.

[6]Another simpler reason could be referees' low opinions of the offerings sent to them.

[7]And just to make sure the Editor doesn't try to disguise the vile quality of his Panel of Referees by covering my name up after all, I send a copy of my comments to the Authors under separate cover.

clandestine dagger-work. However, there are few of us, and I can see that in general the anonymity of referees is necessary. Otherwise, the person who already has a wide reputation in the field would find it too easy to railroad through further publications of any quality, as many potential referees would consider offending this guru to be poor career development.

Apparently, some journals try to make the Authors anonymous, as well as the Referees, by removing their names from the manuscript. This can only be partially effective. If half the references are glowing citations of Excellent, the referees can make a good guess that this paper is by Excellent, too.

The Referees are usually asked to fill in some sort of form[8]. These are signed, and won't be forwarded to the Author. If the Referee is any good *and* your paper is any good—but not perfect—he or she will usually prepare a separate report of a page or two which doesn't have his or her name on and is ready for forwarding to you. Some of these look like papers in their own right, with diagrams and everything. I've done a few a bit like this myself, and they take ages: hats off to the Referee if you get one. I think you can infer quite a lot about the quality of the Referee's advice from the detail with which it's presented. People don't usually glance casually at a paper, and then prepare half-a-dozen pages of algebra and so on.

So, the Editor gets these back from the Referees (eventually and if he's lucky) and then he dips his quill in bat's blood and sends you his Decision. That's easy when all the Referees say the same, but difficult when they don't. He may be cautious and get more opinions, or he may actually look at your effort himself. Of course, in a journal with wide coverage, his opinion will only be that of an expert on a few of the papers coming through.

So eventually he writes to you, enclosing (unless they're too rude, or too fatuous) the Referees' comments. Almost invariably, his Decision is one of the following:

- *Reject:* often, the Editor will employ some euphemism such as "We are pressed for space", or "I'm afraid your paper is not

[8]The figure shows one I invented; I have tried—in my eccentric way—to avoid the vague "Poor—Average—Good" categories which usually make such forms so difficult to complete.

REFEREE'S COMMENTS ON A RESEARCH PAPER

Please tick the boxes which best describe your reaction to the paper; extra detail can be written on the right of the form, on the reverse, or prepare a separate report if you prefer.

Paper Reference

1. ORIGINALITY
   [ ] Never been done before.
   [ ] Never been done this way before.
   [ ] Minor variation on a known technique. Reference?
   [ ] Re-invention of a known technique. Reference?

2. SIGNIFICANCE
   [ ] Important problem [ ] of current interest.
   [ ] Part of a problem [ ] of current interest.
   [ ] An interesting insight.
   [ ] Recreational.

3. SOUNDNESS
   [ ] Obviously sound.
   [ ] Probably sound.
   [ ] Contains errors of detail. What sort?
   [ ] Seriously flawed. Where?

4. DETAIL
   [ ] Unnecessarily detailed. Where?
   [ ] Enough for a graduate student to repeat the work.
   [ ] Enough for the referee to repeat the work.
   [ ] No-one could repeat the work. What's missing?

5. REFERENCES
   [ ] Too many background references of marginal value
   [ ] Virtually the same references the referee would have cited.
   [ ] Out-of-date references: to old work only.
   [ ] Shallow references: to new work only.
   [ ] Totally inadequate references. What should be cited?

6. COMPREHENSIBILITY
   [ ] Understood at first reading.
   [ ] Several readings required.
   [ ] It would take a week to understand this paper.

7. PRESENTATION
   [ ] Paper is too long. Where?
   [ ] Paper is well-balanced.
   [ ] Paper is too short. What's missing?
   [ ] Rearrangement needed. How?

   [ ] Title not descriptive. What should it be?
   [ ] Abstract not descriptive. What's wrong?
   [ ] Poor figures. What's wrong?

8. RECOMMENDATION
   [ ] Accept as is.
   [ ] Minor revision: [ ] of content; [ ] of presentation.
   [ ] Major revision: [ ] of content; [ ] of presentation.
   What MUST be done before acceptance?
   [ ] Reject. Main reason is?

REFEREE ..... SIGNED ..... DATE .....

A Referee's Report Form

A Referee's Report Form

quite appropriate for this journal" and so on. Well, at least you got a clear decision, even if the reason for it was about as truthful as the excuse the announcer makes when your train is late.

- *Reconsider after revision:* that means, you are invited to prepare a revised version and submit it again. It will probably be sent out to at least one of the original Referees; possibly to more than one and possibly to a fresh Referee. Then there will be another Decision.
- *Accept subject to revision:* that means, providing you make the changes that the Referees recommend in a sensible way, the paper will be published. One of the Referees may be consulted again, or the Editor may eyeball the revised paper himself. Often it's just a case of checking minor corrections off on a list.
- *Accept:* hooray!

Now, what do you do next?

Revision

If the decision was "Accept subject to revision" then you should almost always get on and do it. It is most unlikely that there will be any substantial changes required, or any change to the overall direction of your paper. Often the 'corrections' are a long list of typos which you were too lazy to proofread out. Under your breath—or in the Acknowledgements if you prefer—thank the Referees for this and get on and make the changes. You may also have received some advice on your English. This can be a considerable help to people who are not native English speakers, as there is a limit to what a sub-editor who doesn't understand the technical sense can do. Referees who are not native English speakers will also offer advice surprisingly frequently; even if your first language *is* English, remember that the Referee was actually taught the grammar and you weren't, so the comments are probably right[9].

[9] *In fine*, if you must use Latin tags, be prepared to be put in your place by almost anyone.

Sometimes, an "Accept subject to revision" will involve a shortening of the paper—or perhaps that is all that is wanted. Quite often this will be the Editor's idea after he's balanced the Referees' comments against the weight of your manuscript. A reduction of up to—say—25% can usually be achieved remarkably painlessly, and almost always improves the readability of the paper anyway. It's also good for your writing, so get on with it. Beyond that, you should consider carefully whether your paper can be shortened without losing your original intent.

Things are more difficult if the Editor's decision was "Reconsider after revision". Sometimes the Referees' lists of suggested changes are extremely long. They may want whole sections to disappear, new ones to be written, more diagrams and references. And it may not stop at the way that the paper is presented. They may be asking for quite a lot more work: extra proofs, more examples, or further runs of a program on different sets of data. If there has already been a considerable delay, then you can reasonably expect another when the paper is submitted again. And how long will it take you to do these extras? Is that where your work was going anyway, or is it a red herring? And is it worth putting more effort into a paper that's a wasting asset anyway? In this situation, you have three options:

- *Treat as rejection* and withdraw the paper; this is hard to do but may be the best plan.
- *Accept at least 80% of the Referees' comments,* and make the corrections to suit.
- *Argue the toss:* challenge the Referees strongly. But it is not much use trying to change *their* minds[10]; they were asked to referee your paper and have given their opinions. Why should they recant, especially as you don't know who they are? The aim of your challenge must be to change the *Editor's* mind, if necessary by partially discrediting the Referees. This can be done, and is made much easier if only one Referee is making the trouble. Submit a point-by-point refutation of that Referee's comments[11]. Suggest the Referee is "probably

[10] If any.

[11] Sometimes the Referee is not merely obstreperous, but completely wrong about some technical point; this has happened to me as a referee. With Open Refereeing, such problems are sorted out much more easily.

> familiar with another area". You may even be able to guess what it is; in this way, you can try to implant the idea that the choice of referees wasn't as good as the Editor had originally thought. Try phoning the Editor up (did I really say that?) to make your point. This is a high-risk strategy but editors can be—have been—persuaded to 'revise' their decisions radically.

Some authors try to mix the second and third possibilities; they make a few half-hearted changes, and return the revised paper with some deliberately cryptic comments. This only works if the journal or conference is desperate for papers. Then the fact of a revision—however notional—will pass. Otherwise, being oblique is just asking for trouble.

In fact, by seeing how vague the Referees are being, you can often decide whether you are have already entered this quagmire: so look at how *specific* their comments are, not so much how *favourable*. If the Referees know their own minds, then you have a fixed programme of work to repair the paper which you can accept or reject. If the comments look vague, then you won't know quite how to change the paper, and as like as not the Referees will come back with a quite new cacophony of moans about the revised version. Then the whole 'process' may disappear into a morass of recrimination—and no publication.

In my view, an absolute must is to send the revised paper in with a list of the Referees' comments and, against each, a résumé of the changes. A quick and effective technique—although not quite in the spirit of the Information Age—is to make a copy of your new paper, and of the Referees' comments. Then, cut out each of the comments in turn and, for each of them, cut out the piece you changed in the paper, and paste them on to clean sheets. Where appropriate, you should also cut and paste in the old version from the original paper. Make it clear which is which (it helps if you and the Referees all used different type sizes or faces), and write notes in the margin if necessary. In a few minutes, you can assemble an impressive-looking collage which explains what's been done at a glance. This is quicker than saying in words what you did for each change, although you may need to provide a gloss in places, especially if the changes are enormous. If you can do the collage, the Editor, and the Referees if they're asked to look at it, don't need to keep turning to your

old and new texts to check all the points. Checking two complete versions against two or three sets of Referees' Comments takes a lot of time, and therefore is a job that people put off: to your cost. Done thoroughly, these collages can convince an editor not to send the paper back to referees after all, but to accept it on the spot.

The worst thing to do is just to send back the revised version without any comment. That's sure to go out to the Referees again, and if they lost the original copy, they may just start looking at it afresh; and that's when you get a flood of quite new comments....

Acceptance

If your paper has been accepted, then uncork *some* of the fizz—you are nearly home and dry. However, many journals have long waiting lists of accepted papers, and yours will join the queue. You should have checked on that in advance (see Chapter 6).

Now is the time to send in master copies of the figures, colour slides and other 'artwork'. At this stage, you may also have to change your references and so on to the standard form that you were asked to follow at the start (but it's well worth having postponed the work, if you got away with it). For conference proceedings, you may well have to make a master at this stage, or arrange to have it made, following the conventions supplied. That's not usually too difficult on a word-processor, although the 'typing kits' of special paper etc. which some conferences send out may force you into having the final version typed up by hand. For a journal, and if you are lucky, many of these changes just happen, performed by the luckless 'Sub'. Since the Editor will have totally forgotten about your paper by now, it pays to establish good contact with the production staff, if possible; they are probably much more professional and helpful than the grouchy old Editor.

Eventually you will usually receive a set of galley proofs, which show exactly what the paper will look like in print; you are only meant to correct printer's mistakes on these; you *can* correct gross errors that you suddenly spot, but resist the temptation to revise the paper on the galleys[12]; everyone will get very cross, and possibly

[12]This is a problem with a paper that gets accepted very quickly because

even try to charge you money. For some reason, even if it's taken years to get to this stage, Production Departments always want galleys back in FOUR DAYS, or something like that, and they stamp this fact all over your paper in violet ink. Of course, they have a production schedule, but my theory is that the time that you're allowed is pared to the bone to discourage you from tinkering.

Next you see your effort in print. There's always someone else's paper in the same issue of a journal, or the same conference proceedings, that is on the same subject, looks much more rigorously argued, is shown to work better, or is already a commercial product. Never mind: you should be thinking about your next paper; you now have 1799 to go to equal von Nell-Breuning.

In due course, the better journal will send you a few copies of the issue in which your paper appeared, and a few dozen 'offprints', which are printed copies of the pages on which your paper appeared, but not the rest of the journal. Usually these are unbound, but superswish journals[13] may put them in the same covers as that issue. The idea is that researchers who are interested in your work can send you curious cards from all over the world requesting copies of your paper, so that they can pore over it nightly, long after their libraries have closed. In some fields—such as medicine—it seems that you are overcome by hundreds of these things: wonderful for stamp collectors. I only ever receive a few, but I almost always use up all the reprints eventually: sending them to people I think should have read my article but—judging by *their* papers—obviously haven't.

Rejection

Whether rejection is a disaster depends on how long it took. The worst scenario is to be rejected *after* a long time and *because* someone else published an identical paper in the meantime. Maybe they

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the Referees were too lazy to do their job properly. I feel much happier at the galley stage if my paper's already had a pounding from some really nitpicking Referees.

[13] e.g. the *IBM Systems Journal*, from which you also receive a framed certificate incorporating a colour reproduction of the journal's cover. Wow! I hope they keep on selling enough computers to pay for all that....

submitted it after you, but your journal or conference was slower. In cases of this sort, journals will sometimes publish a paper that was in process when the second paper came out (if the Referees' comments are any good) in the interests of fair play; but sometimes they won't, and there's not much you can do about it. There are occasions where you just have to throw the paper away[14].

At the other extreme, the journal rejects you quickly, maybe for a 'technical' reason (e.g. the Editor has decided on a change of direction for the journal, and you fall outside the new scope). And now, maybe, you've also got two or three referees' reports that you can use to improve your effort. Do it! Then stick it back somewhere suitable as soon as possible.

If you find yourself half-way between these extremes, then a compromise is to lower your expectation; a middle-of-the-road conference gives you a reasonable chance of publishing such material before the sell-by date is finally past.

[14]No: throw nothing away. Even if your method or results are now truly superseded, you may be able to use the paper another way. For instance, if you used an obscure programming language to implement an algorithm that is now old hat, maybe there's still some mileage in the implementation. So how about a journal dedicated to that language? Or keep it for a journal that's doing 'a look back' in ten years time. Or be a cynic like me, and wait until the whole topic is reinvented. There is a theory doing the rounds that topics recirculate every seven years. This is of course a load of superstitious nonsense; to predict the actual interval, see my new astrology column....

9

Packaging and Presentation

Have you ever been told—in a romantic moment—that your most attractive feature is your left eyebrow, or the mole on your chin, while you thought it was the splendid muscle tone that you had spent hours developing at the gym? You cannot make people look at your papers in the way that you want, either. The first thing they will see is a *Title* and some *Authors*, and maybe ignore the marvel of lucidity that is the text you spent so long concocting. So we'll look at the chrome and bodywork first, and the engine and transmission second.

Authors

You might imagine that choosing the Authors is usually not a problem, but sometimes it is the worst of all problems. For instance, if someone provides the initial idea for a paper, but does nothing else, should their name be on the front? The difficulty is that the alternative—being mentioned in the Acknowledgements—is a demotion usually reserved for the person who looks after your computer, or your Head of Department who allowed you to get on with the job by being kind enough not to fire you. There is no real half-way house[1].

[1]Except, perhaps, the 'Private Communication', which appears in the References. That is not, however, altogether satisfactory either as an acknowledgement or as a citation.

The message conveyed by the 'Private Communication' is more usually something like: "I'm in cahoots with X who (is a big-shot and) has read this paper and it's okay". If you're a big-shot too you may armour-plate his papers in the same way.

Usually I try to err on the side of generosity, as adding an extra name may make little difference to the credit the real Authors get, but will keep the peace. The problem is, sometimes it *does* make a difference. For instance, some university departments (are told by some governments to) give you proportionately more credit for single-author papers than (even the appropriate fractions of) multiple author ones. They like to be sure who's doing what. In this circumstance, a research team can elect to appear as sequential Sole Authors, but this is risky!

A more common way to try to tackle issues of prominence of authors' names is to tinker with their order. The default arrangement is to have the names in alphabetical order, but the major contributor to a multiple-author paper can be promoted to the front as a reward. Having a name that starts with a letter near the end of the alphabet is sometimes considered a disadvantage. Not so! If your name is Zygon, and appears at the end of the list of Authors, everyone will assume that that's because alphabetical order is being used, but when Zygon is at the front everyone knows you were the kingpin. Conversely, if your surname is Aardvark, it will always be assumed to be at the front merely on alphabetical grounds; when it's at the back everyone knows you did nowt!

Titles

To get on to titles, there are many ways to try to generate witty ones, such as alliteration, bad puns, rhymes, and paradoxes. The UK newspapers seem for the moment to have specialized in the 'bogus quote'—"Tale of two Sillies": that sort of thing. I think that a few papers with witty titles (together with a few speculative ones, and a few in slightly different fields) are excellent ingredients of the Renaissance CV. But two sillies (or thereabouts) are enough. In general, it is the best plan to try to make your title as good a compromise between being short and being descriptive as you can manage.

Probably the most difficult decision is how 'general'-sounding to make the title. If it is very general, e.g. "A computer system", then it is obviously undescriptive but—more seriously—other people

working in the field are likely to think that, because you chose such a title, you don't know what you are talking about. They could be right. (Portentous and general titles are sometimes used deliberately as an attempt to capture a lot of high ground in one paper. You probably need to be fairly well-known and to have a certain amount of gall to try this, and it can still backfire.)

At the other end of the spectrum are titles which are too specific. Typically they emphasize the solution, rather than the problem (e.g. "A low-entropy simulated annealing packing algorithm extended to distorted prolate spheroids" rather than "How to pack pear-shaped objects into a box"). This difficulty is compounded if you use important-sounding jargon which is not as widely known as you think. Referees are usually asked what they think of the title, and titles do get changed.

Choosing a more conventional title is relatively easy, because there are a number of set incantations which can easily be grasped by looking through the journals and proceedings in your field. That these moulds exist is not so surprising if you recognize that the constituent phrases of titles typically express only a small range of concepts. These are descriptions of:

- The problem.
- The method.
- The solution.
- The application.
- The way the paper is presented.

Often the title is just one such phrase, e.g. "On Excellent's problem". However, a number of conventional composite phrasings seem to be used more frequently. Many of them boil down to one of two formats. One is used before, and one after, the brow of the hill has been reached for the problem in question. To put it another way, are you announcing another failure to solve a well known problem, or riding on the coat tails of an existing solution? The respective titles are:

Attack on || *unsolved problem*
Improvement on || *solved problem*

With variations, these cover a lot of papers. The *attack on* phrases include: 'an approach to', 'an investigation of', 'a proposal for', 'the representation of', 'a study of', 'speculations on', 'a theory of', and 'towards a solution to'. Admitting some bias towards computing, some *improvement on* phrases might be: 'an algorithm for', 'an analysis of', 'the development of', 'a framework for', 'a language for', 'a procedure for', 'an introduction to', 'a review (or survey) of techniques for' and 'a system (or prototype system) for'.

There are also meta-titles which modify one of the above titles: for instance by specifying which part you are working on, advertising your named technique, saying what application you have in mind, or linking two titles into one. These might be:

Aspects of || *title*
Name or acronym |:| *title*
Title || *for application*
Title 1 |and| *title 2*

There are also some common link words (somewhat different in every field, I'm sure) which merely say 'good' or 'academically respectable' in the same way that advertising copy uses 'new' or 'unbeatable'.

Examples are: 'efficient', 'fundamental', 'general', 'integrated', 'new', 'optimal', and 'unified'.

Naming

The final aspect of choosing a title is perhaps the most fascinating. If (or rather, *if and only if*, as the mathematicians say) your work makes some sort of a dent on the collective consciousness of the people working in your area, how do you want it to be known? How you word the title can have a decisive—but not the only—effect on this.

Some possible ways to try to generate a handle for your work are:

> *Acronyms:* Acronyms are all over computing, and elsewhere of course. They may be genuine assemblies of the first letters of a phrase (e.g. CAD: Computer-Aided Design), or a sort of contraction, (e.g. FORTRAN: FORmula TRANslation).

Invented names are the next possibility (e.g. Magic Cube: a parallel processing topology). An extreme case is the introduction of Greek letters (e.g. β-spline: a sort of curve) or special typography, when the name really becomes a logo (e.g. TEX: the typesetting language used for this book). This causes confusion, for instance in indexing (e.g. entries might appear under 'Beta-spline' and 'Tex'), and is best avoided unless you're famous enough to make everyone do it your way.

Biographical names are a relatively recent variant on the invented name in computing. Examples are the programming languages Ada and Pascal, which are named after the great and dead. Trying to draw legitimacy from past achievement is slightly distasteful[2], and this fad looks to be passing.

Descriptors are probably the most common technical names e.g. 'binary tree'. These are the most self-effacing. They are absorbed into the argot so quickly that it is often forgotten that anybody invented them.

The Author's name is the other extreme: getting an even half-decent technique that you have invented named Excellent's Technique has been observed to have a transforming effect on your acceptability as an invited speaker at conferences etc., even long after you started working on something completely different (and vastly less successful). It's quite a rare event and, I suspect, rarely done on purpose. However, I see no reason why it should not be connived at. Your paper needs to be about something significant, of course (although 'significance' is not too easy to predict). Given a certain germ, there seem to be two keys to its correct presentation:

- Do not provide any other peg, in the title or the text, on which the technique can be hung.
- Be lucky enough (or possibly arrange) that one or more further papers by other authors appear quickly calling your technique "Excellent's Technique"[3].

[2]Especially in the case of *Ada*, which is after all the *first* name of Babbage's assistant Ada Lovelace, and thus involves a familiarity that would have surely been greeted with disapproval at the time.

[3]I am currently considering a much more blatant exercise in which I offer to

> Well, give it a try. Descartes hit the big time with his Cartesian Coordinates; but whoever invented the polar ones obviously did something wrong, and was left out in the cold.

The Text

As observed in Chapter 3, the technical paper is not a very challenging literary form. It is almost invariably constructed around an Introduction, a 'Body', and some Conclusions. These parts do not have a very profound function; they are for telling your readers what you're going to tell them, telling them, and then telling them what you've told them. There is also the Abstract, in which you tell them what you would have told them if they'd had time to read it.

Abstract

I have often converted my first attempt at an Abstract into the Introduction[4]. It is easiest to write the Abstract last, but some conferences ask you for Abstracts or 'Extended Abstracts' ahead of the main paper, which leads to some baroque constructions. Keep the Abstract *brief.*

Introduction

The Introduction is an important part of your paper. The few who read beyond the Abstract, or skim in more detail than the Figures, will probably start reading here. That's as far as many of them will get, but give them as little excuse for stopping as possible. There are quite a few things not to put into the Introduction:

> *Puff:* the Introduction is not a trailer for the contents ("We expect our technique to have a profound effect...."), nor for your programme of work ("The PANCAKE-Worx Project will usher in a new era of...") or even for your institution ("Sited in a region[5] renowned for its hi-tech industry, within easy reach

remove my name from a collaborative paper in exchange for the other authors blasting "Woodwark's Method" all over the title. All I need is a really good Method....

[4]Some authors do this, but never rewrite the Abstract: not recommended.

[5]Usually a 'corridor' or 'triangle'.

of the International Airport at....”). Even toned-down versions of this sort of stuff will cause readers to start to foam at the mouth. Maybe that is what you want, but remember that the Referees will be similarly affected.

Millenial polemic: this is similar. Many areas of science and technology become wonders for a while; always when world-shattering developments are *about to occur*. The excitement may be local and some fields seem to need a buzz-word every couple of years (e.g. in manufacturing engineering: ‘just-in-time manufacturing’, ‘zero defects’, ‘total quality’, ‘concurrent manufacturing’, etc. etc.). Associating your paper with these buzz-words will enthuse only the more excitable readers, and is certain to make it look ridiculous later (just when you wanted to put it in your DSc submission). Concerted polemic by lots of people working in a field tends to produce alternating expectation and disappointment leading to boom-bust cycles in funding (e.g. AI, robotics).

Retelling of history: make a reasonable assumption about what your readers know. Introductions often contain a full history of the field, complete with references-of-obeisance to well-known pioneers and so on. This is tiresome and can give the impression that the Authors have just made a personal discovery of all this exciting stuff. In review papers a bit of history is appropriate; otherwise, sketch the historical perspective lightly using just a few scene-setting references as props.

The Body of the paper: just as the Abstract is sometime retitled as the Introduction, the Introduction can occasionally be recycled as the Body of the text, and replaced by a new one. This is likely to be a less satisfactory renaming, and you should keep a target length continually in mind.

So, what *should* you have in the Introduction?

An outline: of your technique. This is the place to say:

- What need it satisfies.
- What’s new about it.

Competitive analysis: this is also the place politely to demolish the competition. Phrases like “limited applicability” and

"excellent early work" generate the impression that they were doing well *in their time*. These recitations of past and alternative techniques are often as dull as a laundry list[6], and much more difficult to follow. A *table* comparing these efforts can transform the clarity of the section. It takes a little effort, which I suppose is why it is rather rare.

The best way to conclude the Introduction is, in a sentence, to *stake your claim*: without switching to polemical mode, restate the most salient feature of the wonderful achievement that the paper is about.

Body

It is difficult to pontificate about what the central section of a paper *should* contain, because this is obviously dependent on the technical message. At the risk of writing Chapter 3 again, here is a list:

Start with the detailed explanation of your technique: this is always necessary.

Link in any proofs, always ensuring that—like the Cold War strategy of *flexible response*—their weight is proportional to the extent to which they advance the argument.

Provide explanatory examples that are simple enough for you to take the readers through them stage by stage. It is difficult to select examples that are both simple enough to explain (and to draw simple diagrams about) and small enough to present fully, *without* raising the reader's suspicions that they contain special pleading, and even that your technique won't work in practice.

For instance, in geometry, an example in two dimensions is vastly easier to draw and to explain than one in three dimensions; but everyone knows that there are many techniques that don't scale up from two to three dimensions; so, if an author uses a two-dimensional example, he needs to address this scalability issue carefully in order to maintain confidence.

[6]In his sketch *Homer and Humbug* the Canadian humorist Stephen Leacock pointed out that the "Catalogue of the Ships" in the Iliad is a dull list of just this sort. Homer obviously decided against a table, so you're in good company if you persist in this approach. Try setting it in verse.

If applicable, also provide test examples to show your technique working on a full-size problem. You strengthen your hand immeasurably if:

- The examples are real cases, preferably obtained from some source outside your direct control, or standard 'benchmarks'.
- The examples are of significant size.
- You give information about the performance of your technique during the solution.
- You are able to supply the results in readily assimilable form, such as colour pictures, etc.

Conclusions

In the Conclusions, you should be prepared to repeat:

- A statement of your technique.
- A claim of utility.
- A claim of novelty.

It is not necessary to reword these statements cleverly; it is better to use the same phrases that you used in the Introduction, or wherever, so the reader will recognize that you are at least being consistent.

You should check at this stage that these claims, and any other conclusions that you draw, actually match the results in the body of the paper.

It is then common to go on to suggest further work. This is a traditional feature of thesis Conclusions that seems to have escaped out into technical papers. Of course, suggestions for further work add tone, but they are dangerous:

> If the further work is a sensible programme, why hasn't it been done already? Maybe you *have* done it, and you are just staging your publications? Referees don't like the idea that they are receiving a carefully-measured dose of your results, when your work is actually further advanced[7]. You could receive withering comments starting "The paper appears to be incomplete..." or "Further work is needed to resolve...".

[7]Although this is rather a paranoid assumption, as keeping 'results' in hand is a dangerous plan.

> Alternatively, you can outline an ambitious programme of further work, that could lead anywhere—here's that "robot research assistant in 2010" again. You obviously haven't done it (or you wouldn't be bothering with silly papers like this one). However, you may get some credit for 'vision', or even for 'inventing'—on the Leonardo model—the robot research assistant. But it easily ends up reading like science fiction. And, if the further work is fiction, the Referees may be tempted to conclude that the rest of the paper is, too.
>
> But what if your suggestions for further work are sensible but sufficiently far-reaching that it's obvious they can't be incorporated in the present paper?
>
> Well, if that's the case, why are you giving these goodies away?

So, soft-pedal the further work. Even if you can't, or don't want to, pursue a direction any further, much more may be extracted from really good suggestions if they are used as the basis of a speculative paper.

References

I think that references were originally meant to be pointers to books and papers that you had read—or at least consulted—in writing your own effort. This notion is long discredited. References are quoted for the following reasons:

> To provide 'tone': this should be done economically; grovelling at the feet of any and every 'guru' is demeaning.
>
> To indicate where results and proofs can be found, without the trouble of repeating them.
>
> To acknowledge competitive approaches: there is a nice choice here between citing your competitors and rubbishing them, or ignoring them completely. The former tactic avoids creating unnecessary holes in the references for the Referees to spot. The latter action—or rather inaction—is perhaps more satisfying, and if your reference list is already longish, it is often successful. But you may care to consider what will happen if one of these competitors ends up refereeing the paper.
>
> To advertise: advertisements for the Authors' own related work often dominate reference lists. To cite nothing of your

> own marks you as a tyro in the particular area of work. But if half the references (occasionally all of them!) are by the Authors, then neither Referees nor eventual readers will be impressed. They will be drawn to one of three conclusions: that you are unaware of what anyone else has done, that you are too self-important to cite it, or that the topic of the paper is so footling that only you are driven to bother with it, through lack of other inspiration.

The number of references cited at the end of a paper varies from none to fifty or more and, of course, yet larger numbers are common in review papers. You will never be sure that you have got all the references you might have, and if you had, it would often be more than a journal or conference would accept. If you need a guide, try the following balance:

2–4 background papers.

0–4 which supply details missing from your paper.

3–8 which itemize the competition.

1–4 about your own previous work.

A total of 6–20 is desirable, unless you are determined to pursue a 'heavyweight' image. If you must cite papers in foreign languages, then a translation of the title is helpful. And if you are giving references to great tomes, specifying the chapter, or even a page number, is a great convenience. To refer to a three-volume work as a whole is almost always bogus, not to say insulting, rather than impressive.

Appendices

Appendices are convenient places to put overflows from proofs or examples, in order to maintain the continuity of your argument in the Body of the paper. Be prepared for the Referees to suggest that something from the Body is put into an Appendix, or that an Appendix should be excised altogether.

In the former case, it may be quicker to offer to omit the material, if it's not really necessary. It is not unknown for material to be moved to an Appendix at the Referee's request (he thinks it's essential, but 'breaks the flow'); and then the same Appendix is

subsequently ditched at the Editor's request (it can't be essential, because it's an Appendix, and the journal is Pressed for Space—again).

10

Putting the Message Across

Once you've planned what sections to have in your paper, and what to put in them, there comes the bitter time of writing something. In this Chapter, we address two questions. How are you going to pitch it, from theoretical and elegant to practical and useful? And what means of communication, from artist's impression to ISO-approved computer code, are you going to use?

Theoretical vs. Practical

If you've done a piece of work, there are probably all sorts of levels at which you had to apply some intellectual muscle. Take a computer program. Perhaps there was:

- Some theory: maybe some algebra.
- A solution procedure: an algorithm.
- Some code, written in a particular programming language.
- An actual physical computer system, sold to you by a salesman.

It's not at all immediately obvious at which level to report your program. If the algebra was new, then you can say a lot about that. But if you were using a parallel processor, then the type of each processor, and even what configuration the salesman sold you, may (today) be of interest.

It seems trite, but it's actually extremely difficult to get this right. The usual solecism committed by someone who is entering a subject area is to pitch it too low: for instance, to present an algorithm by talking about a program. One of the reasons we find this slightly amusing is because it looks old-fashioned. You only have to hark

Great Thicknesses of Green-Striped Line-Printer Paper

back to the era (I can easily recollect) when research students bound great thicknesses of green-striped line-printer paper into their theses: an exaltation of the mechanism of computing which now looks ridiculous. The contemporary *details* of a technology are always disappearing over an event horizon into the black hole of solecism.

The general trend is also always towards further abstraction[1]. In computing, for example, we are always leaving behind the things we were researching yesterday (like compilers) as mere components

[1]In fact, papers can be *generated* by exploiting this trend. Find an old paper that's significant but rather agricultural in its presentation. Rewrite it in mathematical notation, and recast the terms used into the latest jargon, Finally, suggest that you are "generalizing" the original paper, or even that it was merely "tentative". There's a generic title that often gives this sort of paper away. It's something like: *Old title* | is really | *new title*.

of today's system, and heading into the rarefied heights of things like formal languages and algebra systems. Thus the fashionable approach is to try to look just that bit more advanced than the next paper, by going *ahead* of this process by as far as the Author dares. That doesn't make for legible papers, and of course sometimes people go right over the top, and an engineering subject is magically reborn as pure logic. Someone who defines a television set in terms of Information Theory almost certainly can't help you make one[2], and characterizing the design process as a finite-state machine probably won't help you to design your way out of a paper bag. Like the emperor's new clothes, too much sophistication is suddenly revealed as rather meaningless.

So, in practice it is necessary to choose some mean between the theoretical and practical schools of presentation. A few words on the theoretical approach:

> You need to impress by method, therefore your method must be impressive. If it's a hack, abandon the theoretical angle and concentrate on the impressiveness of the results.
>
> Definitions, Proofs and so on are impressive to look at. They are also compact ways to write down your arguments. As regards their internal consistency, they are resistant to carpet-bombing by the Referees. To be convincing, referees have to fight hand-to-hand on (quite literally) your own terms. Even if a referee has the energy to do the job, and spots fifty mistakes, he has *ipso facto* given you the wherewithal to fix them. (In short, and perhaps surprisingly, theoretical papers lend themselves to being written by the 'Quick and Dirty' schedules of Chapter 4.)
>
> One problem with an elaborate structure of proofs etc. is, where is the appropriate place to stop them? Russell and Whitehead's *Principia* is awfully thick. Once you start on the 'proof' business in what is usually thought of as an applied subject, you are open to the twin accusations of stopping too soon, or going on to ridiculous lengths. There is some opening here for a high-level attack by the Referees, but they still can't

[2] And I wouldn't watch the programmes he suggested, either.

> be too convincing without giving some detail about *why* you said too little or too much.
>
> The big worry about the theoretical approach, to my mind, is the nagging question, does this elaborate confection actually apply to the real problem, or at least to a significant part of it? Even proofs about nice non-oily things like algorithms are riddled with these problems, because you often have to assume a type of algorithm before you can prove anything about it. Then maybe someone takes a different approach entirely.... So a very theoretical approach to—say—engineering design, is bound to be open to criticism (if not guffaws). If your model is faulty, then reasoning about it is a waste of time.

And now to rubbish the practical approach:

> You are trying to impress by results, therefore your results must be impressive. If it doesn't actually work very well, give up.
>
> As I hinted above, the practical approach can look lowbrow. For papers destined for some quarters, a *hint* of commercial exploitation can do something to remedy this. Let the muse of economics substitute for her sister of mathematics.
>
> The big worry about the practical approach, especially in the limited scope of a paper, is that, since you've no general theory to prove it works, you need to give examples to show it does; so how representative *are* these examples of the run of practical problems (or are they rigged)[3]? That can be tricky to overcome, especially when you've got to keep the whole thing down to a reasonable length; even if you have a 'real' example, you may not have room to use it.

Of course, you can often use jargon as a code so that your paper transmits on more than one frequency in the practical/theoretical waveband simultaneously. The messages most usually transmitted are:

- *To the Referees:* you may not feel good about this, but it looks okay and will take you weeks to unravel it. Why bother?
- *To the uninitiated:* this is difficult, stick to your own stuff!

[3]The classic problem in writing a paper about Expert Systems.

- *To your peers:* my work is totally fashionable.
- *To your funding agency:* one day there'll be a whole new industry to your credit here. Just wait a little longer....

Modes of Communication

Finally, here is what you have been waiting for: an engineer trying to tell you how to write English! While 'natural language' (not necessarily English, of course) is most important in technical papers, that is not the only way to get your message across. Pictures are a more general method—even cavemen[4] understood pictures. On the other hand mathematics and computing provide a range of more specific notations. Ordinary language is sandwiched in the middle.

Diagrams

Pictures fill a need to punctuate a paper visually, as well as explaining things[5]. There are two sorts of picture that appear in the average paper. One is 'descriptive': an extension of the written description of your theory or technique. The other is a picture of a result: a photograph, graph or—increasingly—a computer-drawn picture. It's wise mentally to classify your diagrams into these two categories, because they both need filling intelligently. You may think that your paper has 'a lot of figures' but, if they are all of one sort, the substantial number you have may still be inadequate.

Many papers arrive as manuscripts with the most beautiful figures, and master copies are enclosed. I already mentioned that the Editor will probably lose them. Even if he doesn't, the artwork will be wasted if the paper is rejected, or substantial revisions are required. When you submit, you can get away with very rough figures—providing they're clear. I don't think that influences the Referees much. So, either draw nice ones when the paper is accepted, or let the publishers do that—they will sometimes redraw

[4] i.e. prehistoric engineers.

[5] For that reason, you are wise to put the figures roughly in position on the draft copies that go to the Referees, rather than having them all at the back, even though that is more convenient.

ones you naïvely thought were camera-ready, in order to maintain their house style.

Computer-drawn figures—as opposed to graphical output from a program—are usually rather nasty, and betray their origin. Often it doesn't take Sherlock Holmes to identify the particular type of computer. Unless you find hand-sketching very difficult, I don't recommend using these packages—at least until they get a bit better. Either a journal will redraw the figures, or they'll actually use them and your paper will look nasty. Nice computer-drawn figures *should* be easy, and I look forward to seeing better software for this job.

Tables and graphs are common, but perhaps look a little pedestrian, and some people seem to try to avoid them if possible. But they can both—tables especially—provide excellent *summaries* of complicated parts of the text, even if they *could* be omitted.

One useful test, for diagrams of all sorts, is to go through your completed paper, looking at the *diagrams only*. Do these tell a story; and is it the one you want? If it's not, then you'd better add some more (or rarely, remove some), because many of your so-called readers will probably be 'reading' this cartoon version of your paper; I know I often skim in this way.

English

Sorry about this title, but there's no point in my pretending to greater familiarity with other tongues than that necessary to get me to the station or the airport[6]. Obviously there are many good books entirely devoted to writing good, concise English. Gowers' *Plain Words* is the most frequently cited and perhaps the best. It's a very amusing read, but the problem I find with books of that sort is that there is so much advice, and so many examples of good and bad usage, that (when I've stopped giggling) I find it hard to remember enough of it to achieve spectacular reform.

The Information Age answer is to make the most of various bits of writing software that are now available. I presume that everyone uses a word-processor: with a spell-checker which may—or may not—also deal with oddities like repeated words and things like "a

[6]And that only if everyone speaks slowly for the poor Englishman.

emu". They are very useful, especially if you're a rotten typist. There are also things called 'style checkers' or 'grammar checkers' which take the spell-checker idea a bit further. Some of them have a bash at parsing the sentence; others have templates triggered by matching character strings. They all seem to do similar things[7]:

- Flag long sentences[8].
- Flag passive verbs; or should I say, passive verbs are flagged.
- Flag a whole raft of cliches, like "raft of".
- Point out other odds and ends like 'sexist writing' (that just seems to involve looking for words like "frogman" and suggesting "skin diver" etc.[9]).
- Check word frequency; this can also be linked to a thesaurus program; so that you get offered alternatives for words that you're always using. This is one place where technical writing is difficult for these systems to cope with. 'Elegant variation'[10] is deplored by the Authorities. But, in a crude form, and applied to common words, it is essential in ordinary English. If I used the word "perhaps" every time something uncertain was mentioned in this book[11], you'd go mad, and I'd be mad already. But technical phrases are different; if I'm talking about a picture on a computer screen as a 'digital image', I probably want to stick with that phrase, and not have it reborn as a 'digital picture', a 'pixel array' and so on[12]. Obviously it's

[7]The US ones also flag hundreds of 'which's and 'that's. "Wicked whiches" are a specifically American neurosis; East of Rockall, you can safely turn that feature permanently OFF.

[8]And also produce an annoying report at the end saying that this document was four times as difficult to understand as Magna Carta, or the American Constitution. This is amusing—once—but anything that isn't directed towards helping you make specific improvements is a waste of time.

[9]This way, frogs also get a fairer deal.

[10]A truck becomes a lorry becomes a pantechnicon becomes a juggernaut becomes the Chariot of Hammurabi: that sort of thing.

[11]Instead of one of the other 21 synonyms mentioned in my descendant of Roget. My favourite is D.V. (*Deo volente*), so see how I've kept myself under control.

[12]These are genuine technical alternatives. I'm not talking about calling it an "electronic chessboard of tiny coloured dots" and other 'fine writing'.

not easy to get a style checker to follow these nuances without an appropriate—and appropriately annotated—technical vocabulary.

Those are useful, but not exciting, functions, and you can actually improvise some of them without a special program. For instance, if you edit your text so that there's a blank line between every sentence, then the long ones are immediately obvious anyway[13].

In selecting a style-checker, there are two main things to look out for, and they may not be in the long list of 'features' on the box.

How bossy is the program when it's checking your text? The state of the art is such that many—if not most—of the things it finds don't need attention, or at least can't easily be changed without making things worse. And how easy is it to make your own changes, rather than those the program offers you? For instance, programs which persist in highlighting one problem after another (like the spell-checker on my own word-processor) are much less easy to use than those which highlight the problems on a full screen of text at a time, and don't force you to deal with them serially.

There are always rough edges (such as interpreting type-setting keywords as spelling mistakes) and the bossier the program is, the more headaches this gives. If a spell- or style-checker insists on mauling all the type-setting mark-up in a document, then it's useless; or you will have to run it on a special version with all the mark-up removed, which is more trouble.

How much can you change the program? Most spell-checkers allow you to add words, but not to subtract them from the default dictionary. That's very annoying if you've got a British English dictionary but in technical work want—for instance—to force yourself to use the spelling "program" and so you'd like the system to flag any "programme"s that creep in.

Even where there are quite large changes permitted, there are often no tools available for managing these changes. Can you

[13] One of the problems with punctuation is that all those little dots and dashes, that were so convenient for the copyists of antiquity, are not very *visible*. Another fun thing is to replace all your commas with vertical bars, like verse set for chanting. The result| I believe| is very useful| if your fault seems| perhaps| to be the over-use of commas.

How Bossy is the Program when it's Checking your Text?

print out a list of your spell-checker's dictionary? Without this sort of thing, putting in lots of new words is like writing a book and posting every sentence off to your publisher immediately after you've written it: difficult after a while.

Eventually, there will be splendid style-checkers available which will even understand the jargon of a particular subject. In the meantime, buy a nice flexible one, turn most of the options *off* and it will help you (a little bit) to write like Sir Ernest Gowers. But remember that, for the forseeable future, it's still *you* that actually has to do the writing.

Now some beefing about a few specific stylistic issues:

Hyphenation: all the best books say that "the modern trend is away from hyphenation", and that words are leaping straight from their open form (e.g. data structure) to the closed form (datastructure), and only for a week or two is the unfashionable hyphenated form (data-structure) around. Also, it seems, the trend is away from the hyphenation of noun phrases. Thus, we have 'neural net computer', rather than 'neural-net computer'. There's no such thing as a 'net computer'[14], the argument goes, and so there's no problem.

In technical writing, I think this is All Wrong. It is clear that not all phrases can end up as new words in 'closed-up' form. 'Cathoderaytube' looks just plain silly[15]. Therefore, you have the choice of 'Cathode ray tube', cathode-ray tube', 'cathode ray-tube' or 'cathode-ray-tube'. Well, they're cathode-rays, so 'cathode ray-tube' is out. By themselves, the others look okay. But how about 'cold cathode ray tube', or 'cathode-ray tube manufacture' You see what I mean; in context, this is probably technical rubbish, but the phrases 'cold cathode' and 'tube manufacture' certainly aren't. Just recognizing them is enough to break the reader's flow. A few of these infelicities in a row can render a passage very slow work[16]. have two choices;

[14]Unless it's fitted to fishing boats.

[15]In English. I know that—for instance—German has its own heroic approach to compound words.

[16]Complete ambigities, such as "Please order four colour brochures from the printer" are possible, if somewhat rare. In this case, the unhyphenated form

the first is to reword the offending phrase, and the second is to hyphenate. If you think that you are always going to take the former option, then you will have to be sure that you really are aware of how your readers will see what you write. However, while you are writing about cathode-ray tubes, the technology of tube manufacture is likely to be miles from your thoughts. But your readers are not necessarily so thoroughly immersed in the topic; yours may be only one of dozens of papers they're having to skim through. These problems are difficult to spot; and, in any case, rewording will take time and the result may be unsatisfactory in other ways.

As you will by now have realized, I'm all for hyphenation, particularly when the compound noun is a new one, and the bond between its component parts may still be weaker than theirs with adjacent words. I am also quite happy to see three-or-more-word hyphenation, if it is necessary. Even if the noun-phrase you are using is not a combination of words which you are going to use again then, if you use it as an adjective, you should hyphenate unless you are sure that no ambiguity can possibly exist.

Acronyms are the second moan. Acronyms are often introduced and then never used again, or used only once or twice. A good idea is to set a lower limit for the number of times that an abbreviation must be used before it's kicked out altogether. This may be as high as ten[17] for new or unusual abbreviations, but can be lower for abbreviations with which you expect your readers to be familiar already.

The only exceptions should be:

- Some well-worn (if grotty) abbreviations have a different 'flavour' to the phrase that they're meant to stand for, and you may need to use the abbreviation to get your true meaning across. For instance, 'CADCAM' is commonly used in contexts where "Computer-Aided Design and Computer-Aided Manufacture" would look overblown.

leads to a 'commonsense' interpretation (four coloured brochures) that is at variance to the likely one (four-colour brochures).

[17] A figure given in *The Technical Writer's Handbook*, by Matt Young, published by University Science Books, 1989.

- If you have an new abbreviation of your own that you are trying to hawk around, then of course you need to use it. In that case, though, you should probably be using it more than ten times anyway.

Other authors' names: this is a plea for mentioning even your most deadly rivals by name. It is easy for the phrase "Jones [1] and Smith [2] have vainly tried to..." to become "Other authors [1,2] have vainly tried to..." which in turn becomes "In [1,2] others have vainly tried to..." and finally we get the personification "[1] and [2] have vainly tried to...". Unless there are lots and lots of these authors, please give them their names. It's a courtesy to them, but much more of a courtesy to the reader. He may know Jones and Smith, but he certainly doesn't know [1] and [2] without the trouble of turning to the end of your paper. And even if you've got a long list of authors to cite, the phrasing "Many other authors [1–25] have reported their vain attempts to...", is better than "Other vain attempts [1–25] to..."; they are people, despite their failures[18].

Algebra

Mathematical notation is of course essential to most technological subjects; but the modest technologist remembers that it's a tool borrowed from real mathematicians, and continually checks that it is sharp and that he is holding the right end. Some people use maths notation all they can, writing $\exists$ in running text where the words 'there is' would do. Personally, I deplore this; like all really powerful incantations, maths notation is better if you use it sparingly.

What's worse, is launching into algebra without providing 'motivation': that is, some *words* which say what the magic symbols are going to do. Otherwise the poor reader doesn't know whether to expect a prince or a frog. Referees often moan about lack of 'motivation', and I sympathize with them.

Another symptom of notation mania is the use—either with or

[18]The only reason to avoid naming authors is because you're afraid of a libel action; it has to be said—if pompously—that technical papers are not the right place for scurrilous attacks.

without definition—of weird and wonderful symbols, such as ⨄[19]. This is especially easy in exotica like logic, set-theory or topology. The laser-printer is largely to blame; there is no limit to the iconography that is possible by putting one standard symbol on top of another, inventing new geometry, or even scanning-in images. Chinese and Japanese people probably cope much better with this stuff because they're more used to ideograms. I find it very tiresome, especially when there are half-a-dozen different examples of these cabalistic symbols floating around a paper. I think that the problem is that many people need to sub-vocalize the symbol to understand it. If I see '$a = b$', I say 'ay equals bee' to myself, but if I see '⨄ ℘' it's just a picture: in this case, not worth three words, let alone a thousand. So my golden rule is to think very hard before introducing anything that cannot be vocalized (i.e. other than Roman and Greek letters, and the common mathematical symbols). If you have to, or want to, introduce anything else, then give the reader a ready-made way to pronounce it. For instance, "We will use expressions of the form *ab* to mean *a produces the result b*." Even if your readers don't need to sub-vocalize, they may want to discuss your work with someone else, so give them some way to do it!

Code and Pseudo-Code

In this section, my own background in things to do with computing comes out: although lots of unfortunates who are not really interested in them still have to deal with computers sometimes, and write up their programs or results in their papers.

When you look at all the different computer languages there are, and all the pseudo-languages people invent, you can feel glad that real algebraic notation is (more or less) standardized. It's like railways and cars; railways are old, but at least have one gauge and one train on a line at once; cars are new, you can have whatever sort you like, but the result is often chaos.

'Pseudocode' is something that authors invent to illustrate their algorithms. Its legitimate use is to break up a long series of steps into manageable chunks. It is a very seductive medium, especially

[19]That just fell out of TEX; since TEX has it, it'll be legit, but I must say I don't know what it does.

for people who don't want the bother of writing sentences. It looks much more precise than a description in English; but often it is much less precise. Because it is invented, and doesn't have to meet the requirements of grammar, of mathematics, or of a compiler, it can cover more sins than Caligula's. A particular problem is pseudocode that 'calls' functions which are never defined elsewhere. Often, such 'functions' have names which are Pidgin English disguises for processes which would take a major breakthrough to implement. For instance, a program for assembling things together might have a few lines:

```
Define_shape(a)
Define_shape(b)
Locate_in(a,b)
```

`Locate_in(a,b)` is obviously finding some way that the two 'shapes' `a` and `b` mate. But this may be—in fact, is—a first-class research problem in its own right. If I had said "The program then finds how shapes `a` and `b` mate", it would (unfortunately) have been obvious that I was bluffing.

I hardly dare mention the *flowchart*, which is as unfashionable as the tail-coat. However, similar 'block diagrams', with boxes interconnected by lines (with or without arrows) are common. This is of course a *picture* of a sort, but its faults are those of pseudocode. In particular, although the 'boxes' in a block diagram *may* be labelled satisfactorily, the lines between them seldom are. They can represent anything from a logical or conceptual dependency to hydraulic oil at 100 bar, and in the same diagram, too!

Using an actual programming language is almost as big a solecism as using a flowchart. Also, simply because in a real language you *can't* roll huge processes up into a single function call, there's a strict limit to what you can present in the physical limits of a paper. But real languages are at least defined somewhere. Maybe formal methods—which look rather fierce at the moment—will eventually resolve the problem of presenting algorithms.

11

Conclusions

When I used to be sent on management courses, they would always end with a session for telling us "how to apply what you have learned in practice". If that means anything (and often it didn't), it means providing a series of steps—a recipe—by means of which the results described can actually be achieved. This book is intended to contain practical suggestions in a palatable form. It may be less palatable on a second reading; in order that you don't have to find out, the recipe follows.

Well, it's not so much a recipe, more a shopping list, or rather two shopping lists. Simplifying grandly, our Paper Mill has two components: things you write—papers—and places to which you send them—journals and conferences. So the recipe is simply to make lists of each of these ingredients, noting down useful attributes for each.

To make compiling the lists easy, I've drawn up the following two forms. You are specifically invited to make as many copies of each one as you may require; using different coloured paper for each would be pretty. Of course, you may prefer to prepare your own layouts, or do the whole job on a word-processor, database, or even a spreadsheet[1].

The first form, titled `JOURNAL OR CONFERENCE`, is a record of a possible 'home' for a publication. It includes the numerical 'scoring' suggested in Chapter 6, but presented slightly differently; in order to keep the forms separate, we assume a generic paper in calculating the score. Comparing every paper to every outlet is (an

[1]Health Warning to Perfectionists: the important thing is to *start*. Use my tacky forms for now and polish your own elegant system later.

- Dr Excellent's Publication Plan -

JOURNAL or CONFERENCE

Title

Flypaper Dates

Abstract due	Paper due	Event held	Proceedings appear

Merit

A. Relevant readers or participants:	
B. Kudos factor:	
C. Average shelf life - time to publication:	
D. Chance of acceptance:	
Merit factor (A x B x C x D):	

Attitude to Republication

Accepts previously published papers	Allows subsequent publication

Previous E perience or Hearsay

Good Home

Paper type:	Paper quality: Poor	Average	Excellent
Review			
Speculation			
Results			
Aspects			
Application			

FROM *How to Run a Paper Mill*, John Woodwark, Information Geometers 1992

O(nm) algorithm[2] and therefore) impractical. But a more detailed subsequent analysis of the candidate matches that drop out of this rough-and-ready process can follow.

This form is not very long; the headings on it are:

`Title`: the title of the journal or conference.

`Flypaper Dates`: the deadlines on those attractive flyers you were sent.

`Merit`: this is the bogus numerical rating. You may want to change the variables or the sum. At present the factors are:
`A`: An estimate of the *communication* value of the journal or conference, in terms of the number of *relevant* readers or participants.
`B`: The desirability of this outlet. Compare it to the most prestigious journal or conference that might take your stuff (be slightly realistic). Here's a way of quantifying the difference. Suppose you could bribe your way into that 'best' outlet with ten chocolate drops (or other currency unit of your choice), how much would you lay out in sweeteners to get into this one[3]?
`C`: An estimate of the timeliness of the publication; months are a convenient unit. If publication will take longer than the subject will remain current, then this number becomes negative. Maybe this exaggerates the problem. So invent a factor that suits your work better.
`D`: The probability that the paper will be accepted. You may need to distort this value if you think you can get a *decision* quickly, even if publication takes ages. If consideration won't take long, then rejection is not so important.

`Attitude to Re-publication`: try and find out if this journal or conference ever accepts papers that have appeared elsewhere, and if so under what conditions. Also try to discover whether they are 'sticky' about further publication after they have got (your) copyright.

[2]Where n is the number of papers, and m the number of outlets.

[3]This is purely a thought exercise; editors and conference organizers are of course unpurchaseable (*with chocolate drops*).

`Previous Experience or Hearsay`: this is the box in which to write down things that happened to other papers sent to a journal or conference in the past. It's useful if you are circulating these forms to colleagues, but in that case, don't be too rude, or you might end up in court.

`Good Home`: This final section is somewhere to record the types of paper that you think are appropriate for this journal or conference. Put ticks or crosses in the boxes. Possibly more important is to record the appropriate *quality* of paper that will be accepted easily but not too easily; so that you avoid throwing good papers away. The classifications of paper type and quality are the same on the second form, and this is an obvious handle to matching them up.

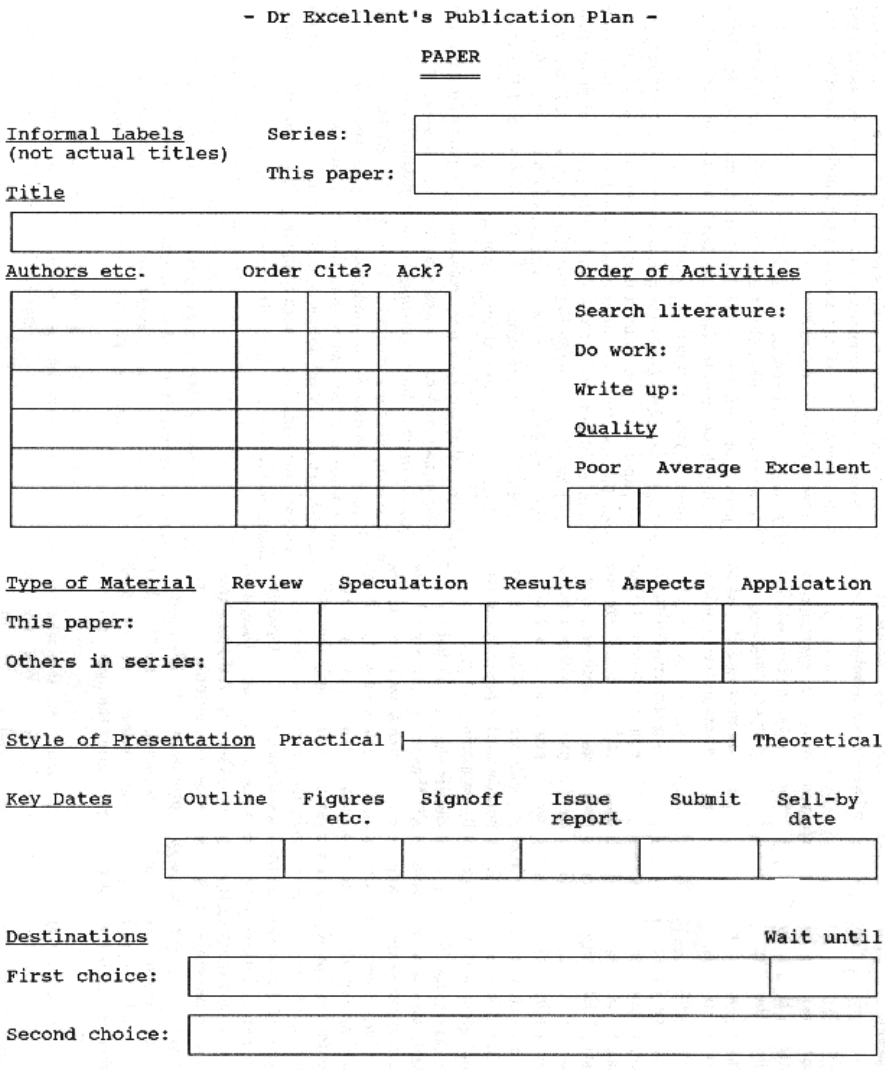
It is likely that you have some colleagues who patronize the same sort of journals and conferences that you do. If you are not at daggers drawn with them, you might find it instructive to persuade them to complete their own set of forms (no conferring!), and then combine the results. If you and your colleagues are feeling really keen, average out the results from the first round and send them out with yet more blank forms, and get further opinions. There's some spurious evidence that such 'Delphic' polls provide surprisingly reliable results.

The second form, titled `PAPER`, is a record of one possible publication, or a publication in progress. There are a number of boxes to be filled in:

`Informal Labels`: These are your own *aides memoires* to identify the paper and the line of work into which it fits. This allows the title to remain blank as long as necessary.

`Title`—when you've thought of it. You may find it best to dream up a title *after* you've decided where to try to publish this paper.

`Authors etc`: write down everyone who is concerned with the paper, and then juggle with the order in which to put their names, or whether to relegate them to a "Personal Communication" citation or an acknowledgement. If there are several marginal 'Authors', getting your plan accepted will require the negotiating skills usually reserved for economic summits.



- Dr Excellent's Publication Plan -

PAPER

Informal Labels (not actual titles)

Series:

This paper:

Title

Authors etc. | Order | Cite? | Ack?

Order of Activities

Search literature:

Do work:

Write up:

Quality

Poor | Average | Excellent

Type of Material | Review | Speculation | Results | Aspects | Application

This paper:

Others in series:

Style of Presentation Practical ⊢——⊣ Theoretical

Key Dates | Outline | Figures etc. | Signoff | Issue report | Submit | Sell-by date

Destinations | Wait until

First choice:

Second choice:

FROM *How to Run a Paper Mill*, John Woodwark, Information Geometers 1992

`Order of Activities`: try to decide which of the methods outlined in Chapter 4 is appropriate to this paper. Compare that order with what is actually happening.

`Quality`: frankly, how significant is this paper likely to be? And how confident are you *really* that you (and your collaborators) can write it up adequately? For instance, even if you've got an Einstein doing the work, and a Dickens writing it up, do they have the time to do a good job on this project? Mark content and presentation separately, and multiply the results, if you like.

`Type of Material`: write down what sort of material is in the paper. If there is more than one strong theme, then think hard about splitting the paper up. Conversely, if two papers in the same subject area look the same in this box, should you be combining them and trying for a better outlet?

`Style of Presentation`: put a cross somewhere on this line. It probably represents your 'default' style. Now ask yourself whether this is the best way of presenting the material in this paper.

`Key Dates`: try and be realistic about the dates when you might expect your paper to be ready. Allow for signoffs required, typesetting (of an internal report), and other bureaucratic and technical delays.

`Destinations`: it's best not to fill these in until you've done the last stage, which comes now.

Suggesting all this form-filling is not—of course—an attempt to turn *you* into that robot research assistant; but when you've got a complete set of forms, you should at least have a better picture of your present position and future publication plans. This needs to be interpreted with flexibility, but the obvious final step is to match up papers to journals and conferences.

What is still missing from Dr Excellent's analysis of Chapter 6 is the subjective match between paper and medium. However, you should now be able to rank the journals and conferences by the merit figure you calculated, and you can sort the papers by 'Quality' (if you've been honest). So start with the 'best' medium and see if you can match it with an 'Excellent' paper. If there isn't one, perhaps

you've been too honest). In that case, start on the 'Average' papers, and so on. When you've matched a paper to the most promising outlet, start on the next, and continue until all papers are allocated.

Purists will scornfully point out that the worst-case performance of this 'algorithm' of mine is still $O(mn)$, but in practice the number of comparisons you have to make shouldn't be too vast, unless you've got dozens of publications on the go, in which case you've been wasting your time reading this book anyway. Very likely, by the time the forms are half filled in, you'll have made some decisions. Perhaps you had a plan already, and this exercise hasn't changed it. But at least you've got your plan on paper. Try asking a reliable colleague what he or she thinks of it; the answer may be a surprise.

As more ideas occur to you, you can update the lists of sources and sinks, and thus maintain and regulate the flow between them. Well, that sounds good, but you knew when you started reading this book that it wouldn't actually write any papers for you (come back in 2010...). But perhaps you *are* feeling more enthusiastic? Excellent. *Get Writing!*