

What's wrong with these conference proceedings?*

Michael Berry

THERE are some experiences whose lasting value does not depend on a material record: bird-song at dawn, a good dinner, a kiss, the smell of herbs on a hot Mediterranean hillside . . . Most scientifically worthwhile meetings fall into this category. They are valuable because they provide the first intimations of new ideas, intense discussions, and the forgings of new contacts and research collaborations. Everybody knows this, but conference organisers persist in demanding "hard copy", in the form of written contributions to the proceedings. Until these are published, several months or years later, the meeting is not regarded as complete.

We are all expected to comply, and most people do so out of politeness, often complicated by an understandable sense of obligation to one's host, who has gone to enormous trouble to arrange the meeting, frequently in an attractive and exotic resort. Nevertheless, few people regard published proceedings as serving any useful purpose, other than perhaps the transitory "unique snapshots of the current state of this rapidly expanding field" — at least in those areas of physics I am familiar with. One's carefully crafted lecture was intended to be heard, which requires a quite different technique from something designed to be read. The lingering pressure to write up that which may not even have been written down can sour the memory of an otherwise enjoyable and instructive conference. When the proceedings finally appear, they are rarely consulted, and can be hard to obtain. The quality of the contributions is often poor, perhaps because they are written in haste and not refereed.

So whence the pressure to go through with this ritual? Often it comes from those who sponsor conferences. Sometimes (as with certain NATO meetings) this is such a strict requirement that organisers who lack the stomach to resist feel forced to adopt the obnoxious practice of withholding contributors' travelling expenses until a paper is handed over. I suppose it is the fanciful image of an outsider to imagine five-star generals, or company executives, pointing with pride to shelves of thick volumes as evidence of the philanthropy of their organisation. Nevertheless, somebody should explain to them how much misery and wasted effort is caused by their corporate vanity (if such it is), and how little scientific benefit derives therefrom.

Faced with a demand for a conference paper, there are several possible responses, all unattractive. I confess my guilt in having,



over the years, perpetrated all of them. First, one can provide a summary of work that will be (or, more reprehensibly, has already been) published "properly", that is, with full details, in a refereed journal. This is bad because it constitutes multiple publication, clogging up the literature and serving no other purpose than to swell the author's CV.

Second, one can present genuinely new work. There are two difficulties with this: if one has the good fortune to attend a dozen meetings in one year (not necessarily all on the same subject!) it is impossible to have that number of new ideas. And in my experience it is a waste: after trying it for several years, I found that the new ideas were simply lost — nobody cited those papers, and the work was later duplicated by others and published in journals.

Third — and this is a suggestion beloved of conference organisers — one can write a review of the subject, rather than present one's latest ideas. But if one has already written several reviews in the last few years, this too is inappropriate (in any case, a good review takes months of work).

Fourth, one can simply refuse to collaborate in the charade, thereby preserving one's honour and avoiding wasting time better spent otherwise, but risking the opprobrium of the organisers, who are usually one's colleagues and often one's friends. Refusing to collaborate can take several forms. Most shameful is to mumble ambiguously, in a way that gives the organiser hope that a paper might appear,

and then simply never deliver one, thereby substantially delaying the appearance of the volume. Or one can announce in advance that no contribution will be forthcoming; this is my preferred option nowadays. Most extreme, of course, is to decide not to attend the meeting; if more people did that, perhaps the pressure I am complaining about might diminish.

If one weakens, and does decide to provide a written contribution, the problems of presentation begin. Most proceedings consist of camera-ready papers. In the days before word-processors, to insist on this was effrontery on the part of publishers, exploiting and torturing secretaries to perform intricate and exacting work that ought to have been the responsibility of the printers. Nowadays almost everybody who writes a physics paper has access to a word-processor, and the demand is less unreasonable. Most of us type our own papers anyway, and are enjoying discovering and learning the finer details of mathematical typography.

But many publishers still give old-fashioned and quaintly detailed instructions for the preparation of papers with electric typewriters. Usually this is followed, almost as an afterthought, by a few sentences of inadequate and often inconsistent advice for those eccentric and daring individuals who might contemplate using a word-processor. Sometimes these publishers' instructions are delivered in a hectoring and even threatening tone (Springer is particularly authoritarian). There is not a hint of the gratitude due to authors who labour unpaid at work which at least in principle will be profitable for somebody else (the publisher); instead the implication is that it is we who ought to be grateful, for the privilege of having our papers published.

A particularly pernicious practice, in my view, is the growing insistence of some publishers and editors on one particular method of presentation, namely T_EX. This attempt at standardisation is premature. Although Knuth's development of the software, and his donation of it to the scientific community, must be regarded as an act of extraordinary generosity, the language cannot be considered anything other than a temporary solution to the problem of producing technical papers. It is too firmly anchored in the bad old world where perfectly straightforward operations are executed by meaningless and easily forgotten control codes.

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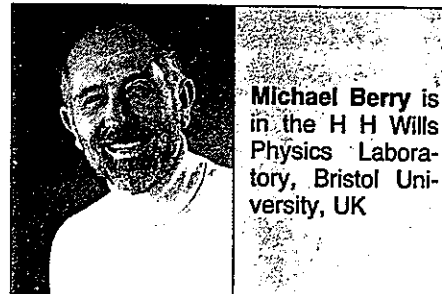
It is a language for hackers, and not at all friendly – most implementations are not even WYSIWYG (“What you see [on the screen] is what you get”). I recently refused point-blank to learn T_EX in order to write my contribution to the Les Houches Lectures in theoretical physics. Why should I, when I had already mastered a different system which produces output of equally high quality? In retaliation for this discourtesy, the publishers (North-Holland) fed my paper through some sort of automatic translator, from which it was regurgitated and returned to me (unread) with almost as many incorrect symbols as correct ones.

I do not wish to be entirely negative. Some organisers and publishers have found an acceptable way of recording the delibera-

tions at meetings. This is to publish conference papers as special issues of regular journals, which has the advantages that the material is refereed and accessible in the archives. In my experience such contributions are often superior to the usual ones. Journals employing this commendable option include *Proceedings of the Royal Society*, several Institute of Physics journals and *Journal of the Optical Society of America* (for meetings organised by those societies), and *Physica D* (for meetings or festschrifts celebrating somebody's birthday).

Most praiseworthy are the meetings where the tradition is simply not to publish any record at all: the Gordon Conferences in New England, and the Dynamics Days in the USA and Germany, are of this sort. I beg

those who are contemplating the painful but creditable course of arranging a meeting to consider them as models worth imitating. They would thereby save themselves a lot of work, and gain much gratitude from those who participate.



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Physics in the balance

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IN PHYSICS everything is relative. The subject is a delicious mix combining the intellectual power of theory with the art of experimental discovery; as practitioners we can place ourselves somewhere on the wide spectrum. Few of us believe we have the understanding of Feynman, but we are all capable of enjoying the subject. Similarly, we may not be able to match Masaccio, but we may relish painting.

Professor Sir Sam Edwards has declared that “a substantial proportion of capable students achieve only a hazy understanding of much of physics, and are left with an undeserved impression of inferiority”. I am not acquainted with the teaching at Cambridge University, where conceivably the atmosphere is a little rarefied, but I do not think this is a fair description of undergraduates at London University, and I doubt if it is at other universities.

When I survey the physics undergraduates at Royal Holloway I see a band of young people of various abilities (ranging from AAA to DDD grades at A-levels insofar as these are useful measures) proceeding with their studies. The best master their work superbly; the middle ranks appreciate with less depth and some gaps; the weakest survive (usually). But the morale of the class as a whole remains buoyant. The London University course unit structure with its choices seems to ensure that everybody is satisfied at his or her own level. In practice the first two years provide a foundation for the third-year options and projects which all undergraduates seem to anticipate with pleasure.

However, we must always examine our teaching and look for improvements, especially to allow for the experience and aspirations of teenagers. It is therefore right that a working party (representing the Institute of Physics and university and

polytechnic physics departments) should have deliberated and produced a report (*The Future Pattern of Higher Education in Physics* August 1990), but it must also be right that there should now be a wide debate on the proposals. My own reaction is that Sir Sam's pessimistic view has been too dominant.

To summarise, the working party has concluded that physics undergraduates have to work too hard and that too much material is now taught in three years. Furthermore, if moves succeed to broaden the sixth-form curriculum, undergraduates will enter universities knowing less physics – which will then require making up. If less material were taught at university there would be more time for reflection, relaxation and inclusion of teaching of communication skills. It is hoped that such a recipe will entice more sixth-formers to read physics; (at present only about 2000 so read, although 40 000 a year take A-level physics). There is also concern about comparability with standards in Europe, the USA and Japan. It is proposed that the present physics syllabus at universities should be diluted, that a new three-year BSc (Hons) degree programme be designed which should teach in three years about two-thirds of the physics now presented, and that all university physics departments should be obliged to comply.

It is hoped that the government will be prepared to fund a fourth year for a keen minority of students (20% maybe) who will then receive further material to provide a basis for professional work in physics and be awarded eventually with a Masters degree (MSci). In spite of the absence of a promise of such extra funding, and although the Secretary of State for Education and Science has announced no changes to the A-level examinations, a 1993 commencement is audaciously urged for the new (3 + 1) scheme.

It is widely believed – and is drummed into sixth-formers – that scientists should base their conclusions on evidence, and it is somewhat eccentric that the working party has now produced a document with no supporting evidence. For example, there has been no survey of undergraduate opinion. Will 80% of a class appreciate being deprived of the opportunity to taste the final fruits? Will lecturers be able to justify this lacuna with their first-year tutorial groups?

Physics is an inspirational subject; its development by its heroes is an extraordinary story. Every advance has been made by individuals, and the sequence of Nobel prizewinners encourages even those of modest ability to dream of making a fresh discovery. The ablest students will be undeterred by the prospect of a fourth year, but will the less confident sixth-formers welcome the hurdle at three years where they may well fall? It may be wrong to ignore the wishes of those who achieve lower second or third class honours. And what of the brightest students who currently are quite capable of getting on top of their material? Will they relish a slower pace, albeit with practice on essay writing?

An important consideration is the comparability of national qualifications in Europe. It is often said that the standards reached by the first degree are higher in France, Italy and Germany (see Jean-Patrick Connerade's article in *Physics World* April 1990 pp29–34). The German Diplom nominally takes five years but in practice seven is the usual time. It is tough right from the start and it is difficult to see how the working party can suggest that in England, after the proposed dilution of the curriculum, the new MSci will be of equivalent attainment. If we are to design a new degree structure some effort might be put first into finding out the actual levels of attainment in Europe. As a start