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Preoccupations of a journal editor

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The dissemination of scientific results is a vital part of the advancement of knowledge. And disseminate the *Journal of Fluid Mechanics* certainly has done, in the 25 years of its existence. The first part of volume one of the *Journal* was published in May 1956, and by the end of April 1981 a total of 4662 papers concerned with fluid mechanics had been published in 105 volumes and distributed to universities, laboratories, institutes and individual subscribers in all countries of the globe. Watching that torrent of information flow past, controlling and regulating it, and at times diverting it, has been an extraordinary experience. It has been a pleasure and privilege to have been involved, even though I have sometimes wished that all research funds would dry up temporarily so as to give editors an interlude in which to reflect on what they are doing.

This 25th anniversary of the birth of *JFM* provides an incentive, if not an opportunity, to take stock of the situation and to think about the general issues involved in the communication of scientific ideas and results from one research worker to another. Any editor is constantly brought up against these issues, and it is important that they be discussed and that the practices of a journal be examined from time to time in the light of the current needs and views of the scientific community. I shall try to describe these issues, as I see them, in relation to the practices of *JFM*. Fluid mechanics is of course only a small corner of physical science. But the subject is large enough for a person to spend a lifetime happily roaming within its boundaries, it has the unusual feature of embracing pure and applied science, and it spreads into oceanography, astrophysics, physical chemistry, and all branches of engineering. Any conclusions reached for fluid mechanics are likely therefore to have some validity for other fields.

I have in mind particularly those aspects of the communication process that lie between the preparation of a paper by an author and its publication. There are aspects of the subsequent stages which also are important, many of them concerned with methods of bringing a published paper to the attention of an interested, or potentially interested, reader. It has been estimated that during the past 15 or 20 years as many new papers have been published in scientific journals as had been published throughout all previous history. The difficulty of making use of this enormous amount of published information is acute. To be in a position to find some piece of information you want, or think you want, or might want, it is no longer sufficient to make a weekly scrutiny of the current journals in the library of your institution and maintain a card index of potentially useful papers. Much thought is being given to practical aids, such as classification systems, key-word and subject indexes, search and retrieval systems, abstracting and translation services, international copying facilities, etc. But they lie outside the range of editorial experience, and I propose to consider only the earlier stage of communication in which the readable form of the scientific results is produced.

This earlier stage is less often discussed, which is surprising because most scientists are passionately concerned about the publication of their own papers and many devote a good deal of their working time to the scrutiny of papers submitted for publication by others. The reason may lie in the elusiveness of the principles involved in the decision on whether a given paper should be published and in the fact that much of the action takes place behind the scenes. Perhaps I can lift the curtain a little on the action behind the scenes of one journal. And I may as well admit immediately that I have liked being an editor, that on the whole I approve of what has happened behind the scenes of *JFM*, that I am pleased by what has been accomplished, and that in consequence much of what I write will seem maddeningly complacent. The object of this article is to discuss the issues that arise in editorial work generally, but since nearly all my editorial experience has been with *JFM* it is inevitable that I draw most of my data and examples from that journal. So for any reader who is associated editorially with another journal I have a double apology, for blowing the *JFM* trumpet and for basing opinions on lengthy but restricted experience.

The scope of a journal

Does the existence of a journal influence the development of some scientific field? It is an interesting question, worthy of serious investigation, although any influences which exist are likely to be subtle and difficult to reveal. Bearing in mind the casual and unco-ordinated way in which many new journals come into existence in the capitalist world, it is very important that we should know whether the establishment of a new journal is likely to have some consequences, favourable or unfavourable, for the way in which the corresponding scientific field develops. The result might be to give a boost to this field, by making readers more aware of the potentialities for new developments or of the availability of new techniques; or perhaps, as a consequence of new subject boundaries being drawn and strengthened by the specified scope of the journal, cross-influences might be diminished and growth inhibited. It would be good to know the truth, but at present we have only unsupported opinions.

Of course I have *JFM* particularly in mind when raising this question. I remember that before 1956 I was disturbed by what seemed to me to be an unnatural and harmful three-way split of the literature on fluid mechanics into theoretical and mathematical papers in the first group, experimental and observational contributions to basic research in the second group, and applications in the third group. There were journals for each group separately, but none which embraced all three except some journals of very wide scope covering much more than fluid mechanics. It was annoying to have to chase after so many different journals in libraries in order to keep up with developments, and when it came to submission of a paper by one of the turbulence group at Cambridge there never seemed to be a journal that was wholly appropriate. I especially disliked seeing theoretical investigations of fluid-mechanical problems published in journals of a mainly mathematical character, because that seemed to me to be denying, or regarding as irrelevant, the physical significance of the investigation, and to be telling young readers that theory and experiment are different and are naturally kept apart. It was clear to me that the existing set of journals did not cater well for people with an interest in fluid mechanics as a whole, although whether that actually had a harmful influence on developments in fluid mechanics is uncertain. I persuaded myself that it did, and since there is nothing like righteous indignation as a generator of action I was able to persuade others to join me in setting up a new journal which would bring together these formerly separate divisions of fluid mechanics and which would focus on the subject itself rather than on the methods of investigation or on the use made of it in different fields of application. Similar thoughts were probably in the minds of other people at that time, because the birth of *JFM* was followed, a little later, by *Physics of Fluids*, a journal which like *JFM* embraces all aspects of fluid mechanics and which, reflecting its American Institute of Physics parentage, includes molecular dynamics and plasma physics as well.

Looking back now on that tremendous storehouse of knowledge represented by the first 105 volumes of *JFM*, I think the mix of papers is roughly what was intended. Experimental, theoretical, analytical and numerical investigations are recorded side-by-side, and, one hopes, are seen as contributing to the common purpose of improving understanding of the mechanics of fluids. One noticeable feature of the mix is that the number of experimental papers is rather less than the number of purely theoretical or analytical papers. This may simply be because there are (I believe) more theoreticians than experimenters at work on fluid mechanics. And it may be that some small imbalance in the numbers of experimental and theoretical papers that are written has been turned into a larger imbalance in the papers submitted to *JFM*, as a consequence of potential contributors making wrong inferences from what they see in the *Journal*.† Regardless of the reason, I can say categorically here that experimental papers are no less welcome than those of any other type.

A wide range of applications of fluid mechanics has been represented by papers in *JFM*, although I personally would like to see more. Many areas of application such as convective heat transfer, fluidization of particles, non-Newtonian fluid flow, hydraulics, aircraft aerodynamics, turbomachines, dynamical meteorology, have their own specialized techniques and body of background information, and are catered for by specialist journals. Without wishing to trespass, I hope that a small fraction of the many papers on those applications of fluid mechanics will continue to be published in *JFM* so that *JFM* readers are aware of the existence of, and state of play in, the many applied fields which have fluid mechanics as a base. I am a strong believer in the role of a journal as an agent of 'cross-fertilization' (provided that the gap which the ideas must cross is a smallish one only – it is unrealistic to expect more than this), and I believe that many of the excursions of distinguished scientists into what for them is a new area of application of their subject are initiated by sight of a paper by a specialist in that area.

Another interesting subject for study by those with a taste for sociological enquiry

† The fact that a journal can publish only the papers that it receives is not always remembered by readers. Editors of *JFM* are continually being surprised to hear it said that the policy of the *Journal* is evidently to discourage papers of a certain type or about certain subjects. Such beliefs begin as speculation from the number of such papers in *JFM* and are fed by the occasional rejection of a paper of the type in question for reasons to do with its quality rather than its type. There is in fact no *JFM* policy, explicit or implicit, concerning the type or subject-matter of papers which may be accepted, except that contained in the rubric at the head of the editorial page of every volume.

is the diffusion of scientific knowledge across linguistic boundaries. Important developments seem to cross these boundaries fairly quickly in these days of frequent international conferences. Nevertheless, I have the impression (again hard information is lacking) that although second-rank developments are integrated into the literature of a particular language almost immediately it takes several years for them to filter across to the literature of other languages. One of the brave ideas that we had in the early days of *JFM* was that we would be willing to publish papers in any language, in order to hasten this inter-language spread of information. We have remained willing, but so far as I can recall have never once been asked to publish a paper in a language other than English! Many papers have been submitted from France, Germany, Italy, U.S.S.R. and other such countries with their own languages, but their authors submitted to *JFM* in English precisely because they wanted their papers to be read easily by English-speaking readers. Their response to the problem represented by the linguistic boundaries is more sensible than the one we had in mind.

It is thus difficult for a journal to avoid being identified with a particular language, but I am sure it should avoid national associations as far as possible (and the linguistic identification does itself sometimes carry national associations) since these are quite inappropriate in science. The *Ruritanian Journal of Physical Science* is wrong on two counts; there is no such thing as Ruritanian science, and physical science is so broad that a given reader could not expect to find in it more than the odd paper in his own field of interest. But when the first error of the national association has been committed, the second one follows inevitably because there would not be enough papers submitted from Ruritania to make a viable journal unless the field is made very broad. These are platitudes, but new journals with national associations nevertheless continue to be established. Most scientific societies are national in character, and many of the older ones publish a journal in the scientific field of the society. The national association of such journals limits the number of contributors and subscribers, and societies are having increasing difficulty in meeting the high cost of printing. But a society which has published a journal for many years would lose much of its raison d'être if the journal were stopped, and will try hard to continue it.

The proliferation of journals with an unduly restricted scientific scope is a more serious issue. There are more than 30000 scientific journals in existence, and the number is still rising despite the virtual constancy of research funds during the past ten years. When a scientific topic becomes large enough to involve a number of people in different institutions over several years, it is fatally easy for them to persuade themselves that 'their' subject deserves more recognition than it is getting and that a special journal is needed. And then, if one of the more energetic ones among them happens to meet a publisher's scout (who will be on the look-out for new journals, because they provide a regular base-load of printing work with reasonably certain sales to libraries), a new journal can be conceived and born within a year or two without the proposal being considered by anyone other than the specialists in that particular field. The new journal becomes public knowledge only after its existence and title and scope have been determined irrevocably. I think it is disturbing that a new scientific journal may be launched so casually with a variety of motives having little to do with the needs of the broad field to which the journal belongs. There can be no doubt that there are already too many different journals (fewer and larger ones would suffice), and since new journals are easily brought into existence but not readily terminated the number is continually increasing.

A badly chosen field for a new journal will no doubt lead ultimately to its death, but while it lives it may have a harmful distorting effect on a subject. Adaptation rather than extinction is usually preferred by a journal with a narrow scope which has outlived its appropriateness, but change of a journal is painful and slow. There were many journals of aeronautics in the thirties and forties, and when it became clear in the fifties that the subject was no longer growing some added the word 'aerospace' to their title. Other examples of journals which began with an unduly restrictive scope and had to undergo painful transformations later may be found in the occupational divisions of engineering – civil, mechanical, hydraulic, sanitation, chemical. Can considerations of wave motion be separated from other aspects of mechanics sufficiently to make a journal called *Wave Motion* appropriate? And now, carrying subdivision even further, there is a journal called *Nonlinear Wave Propagation*. Presumably a paper on fluid mechanics by someone who used a computer in the course of his work would be suitable for *Computers and Fluids*, and unsuitable if he did not use a computer; is that a rational basis for the scope of this journal? Heat and mass transfer, boundary-layer meteorology, geophysical fluid dynamics, hydronautics, multiphase flow, industrial aerodynamics, non-Newtonian fluid mechanics, ocean engineering, non-equilibrium thermodynamics, astrophysical fluid dynamics, physico-chemical hydrodynamics, numerical methods in fluids (*sic*), all now have their own journals; is it not scientifically harmful for the minds of readers to be channelled so narrowly? And how can a collective opinion in favour of restraint be conveyed to enthusiasts *before* they launch a new journal in order to put their field of interest on the scientific map?

I suppose that some kind of centralized regulation of the establishment of new journals would safeguard collective scientific interests, but it would be difficult to operate. Central decisions could not be enforced internationally and would only be recommendations, to be accepted voluntarily. An international scientific organization with sufficient authority to ensure that its recommendations on the establishment of new journals are taken seriously is by no means impossible, but it does not exist yet. Moreover, there are well-known problems with international organizations and committees, in that they tend to be manned by the more senior and venerable members of the scientific community and to be strongly conservative in their decisions. Control by an international body might lead to no new journals being established (not even *JFM*! – I do recall that there was little or no support for the idea of a new journal in fluid mechanics in 1956 from people over 40 whom I consulted), which would be going too far in the opposite direction. It is difficult to see what should be the appropriate agency to consider and advise on proposals for the establishment of new international scientific journals. No existing body or organization seems to be suitable.

The unit of communication

I have always been intrigued by the remarkable ubiquity of that unit of communication, the scientific paper.† It seems extraordinary that, for very many years and in most fields of science, a paper recording the results of, say, two to twelve man-months of work has been by far the most commonly used vehicle for the dissemination of new knowledge. In science the annual printed output of papers in journals probably exceeds that of all other forms of communication on paper lumped together, conference proceedings, monographs, textbooks, general magazines, etc. It makes one wonder whether there is in some sense a natural quantum of progress. Perhaps there is something about the standard scientific paper which fits the human mind and which makes it such a handy unit. The average paper can be written in a manageable length of time (you do not have to shut yourself off from society for very long in order to complete it) and it can be read in a sufficiently short span of time to allow assimilation of the paper as a whole (unlike a book, which has to be assimilated in bits). And at the same time it is usually long enough to contain a substantial development which gives both author and reader a sense of satisfaction, a sense of having learnt something worth while, when it is grasped fully.

In fluid mechanics in particular there has been very little change in the form and nature of published papers over the past century or more. Stokes's paper on flow past a sphere at low Reynolds number in *Transactions of the Cambridge Philosophical Society* in 1851, Rayleigh's paper on the instability of jets in *Proceedings of the London Mathematical Society* in 1879, Reynolds' paper on lubrication layers in *Philosophical Transactions of the Royal Society* in 1886 – all these have a familiar form and would be at home in *JFM* today; only the more mannered and controlled style of writing would distinguish them. I think the same could be said about other subjects in physical science. And a glance at journals in chemistry and geology and physiology and botany suggests that there too a paper of about the size that we are familiar with is the preferred vehicle for the communication of results.

There are nevertheless some minor trends in the form of papers published in

† The first publication resembling the modern scientific journal as a collection of papers appears to have been the *Philosophical Transactions*, which was established by the following order of the Council of the Royal Society on 1 March 1665: 'Ordered, that the *Philosophical Transactions*, to be composed by Mr. Oldenburg, be printed the first Monday of every month, if he have sufficient matter for it; and that the tract be licensed under the charter by the Council of the Society, being first reviewed by some of the members of the same. . . .' On being invited to contribute to the new *Transactions* by the editor, Mr Oldenburg, Robert Boyle wrote to say that he would not wish to 'neglect the opportunity of having some of my Memoirs preserved, by being incorporated into a Collection, that is likely to be as lasting as useful', and he promised he would 'from time to time contribute some short Papers'. In its early years the *Philosophical Transactions* was established as the prototype edited scientific periodical, containing papers, with a recorded date of receipt, which had previously been scrutinized by referees, all significant developments which were later widely adopted in all countries and in all fields of science, and which have remained the pattern for the publication of new scientific knowledge in the subsequent three centuries. The French *Journal des Sçavans*, which was also established in 1665, likewise disseminated reports of scientific observations and experiments, but it served several additional purposes and was not so clearly a forerunner of the present-day journal.

The above quotations and much other interesting history of the *Philosophical Transactions* may be found in the article by E. N. da C. Andrade in *Notes and Records of the Royal Society*, vol. 20, 1965, pp. 9–27, and in *The Corrsepondence of Henry Oldenburg*, edited by A. A. Hall & M. B. Hall, Univ. Wisconsin Press, 1966.

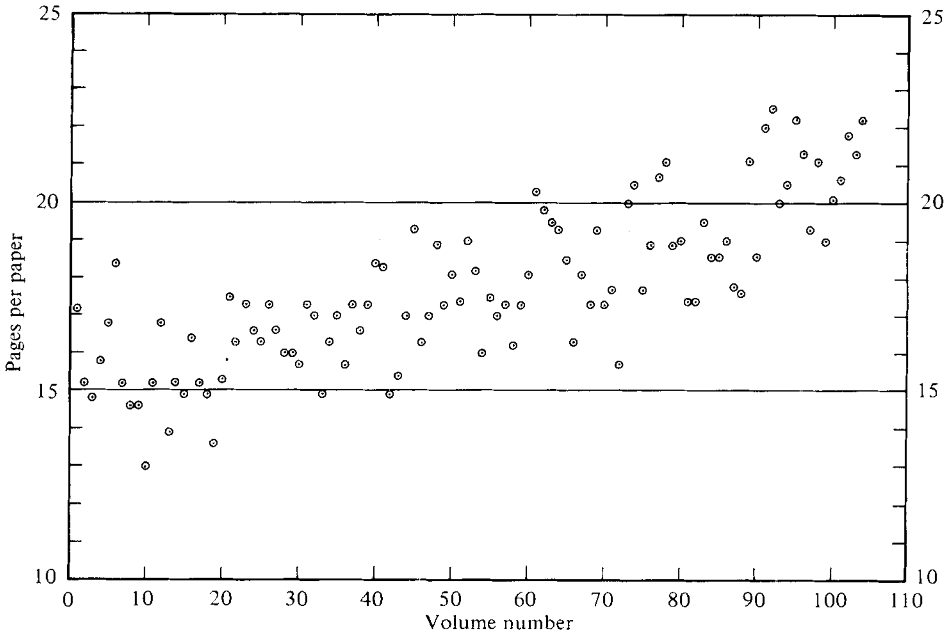

FIGURE 1. The number of pages per paper averaged over the papers in a volume of *JFM*, from volume 1 (1956) to volume 104 (1981).

journals. More diagrams and graphs and photographs are included now than a century ago, possibly as a consequence of improvements in printing techniques. Coming closer to home, figure 1 shows a slow but quite definite rise in the number of pages per paper in a volume of *JFM*. There are big fluctuations from one volume to another, but the upward trend is evident. It seems that over a period of 25 years the average length of papers in *JFM* has risen from about 15 pages to 21 pages. I cannot account for this change. Editorial policy has always been to consider all papers on their merits, regardless of length, and to require only that papers should have value and interest commensurate with their length. Many journals encourage brevity, and some impose an upper limit on length; is it possible that as *JFM*'s more lenient policy has become widely known authors have prepared for *JFM* long papers which previously would have been split up into parts?†

The universality of the scientific paper as the preferred form of communication of new knowledge suggests we should think carefully about its construction and preparation. The contents of a paper are of value only to the extent that they can be understood by others. It is therefore vitally important, for the general progress of science, that papers should be written clearly, precisely and attractively, so that readers are helped to comprehend the new developments presented in them. Clearly and precisely – everyone would assent to that, at any rate in principle. The desirability of the writing being attractive is less often referred to, but it is just as important.

† My co-editor, Keith Moffatt, believes that a significant part of the increase in the average length of papers in *JFM* may be attributable to the vast amount of data (both experimental and theoretical) that is increasingly made available through the use of high-speed computers and to lack of appreciation by some authors of the need to be selective in the presentation of such data.

Reading a paper is a voluntary and demanding task, and a reader needs to be enticed and helped and stimulated by the author.

A paper which describes some useful development, in a way which enables a reader to understand it and see its implications and be pleased he has done so, does more than contribute significantly to the progress of science. It also contributes greatly to the standing of the author and the esteem in which he is held by colleagues all over the world. We commonly accept a person's list of publications as the sum total of his useful work during his lifetime. A few typed sheets represent the fruits of his arduous and devoted labours over decades; indeed, for colleagues at some distance who have little or no personal contact with the author, the list of publications *is* the reality behind the name. An author desperately wants his papers to be read and used and admired – quite naturally, because his career prospects, as well as the esteem of colleagues, depend on how his list of publications is assessed.

An author thus has two powerful incentives to make the message in his paper accessible and interesting: by doing so he will contribute to scientific progress and he will contribute to his own reputation. One might suppose he would therefore do his best to make it a minor work of art. As any editor knows, the truth, alas, is usually otherwise. In the *JFM* editorial office, where accepted papers are prepared for the printer, we reckon we are fortunate if a paper has the basic requirements of double-spaced typing on one side of the paper, legible mathematical symbols, and a text which with some effort can be understood. There are of course many papers which show clear signs of having been prepared with care and craftsmanship, but the average level of composition in papers submitted to *JFM* is disturbingly low. One of the objectives of *JFM* editors from the beginning has been to encourage better writing. We have tried to do this in two different contributing ways. One way has been to require a (rather low) minimum standard of accepted papers, and the other has been to employ an editorial assistant with scientific qualifications who reads every sentence of an accepted paper and amends the wording where necessary to make the sense clear, in addition to putting references in order and giving directions about how mathematical equations and tables and figures are to be printed. In the earlier years of *JFM* we hoped to be able to do more than this and to make positive suggestions for improvement, but with the present turnover of papers the editorial assistant has time only for the elimination of grammatical errors and changes which are essential if a sentence is to be understood.† I like to think that the standard of the writing in papers published in *JFM* is acceptable, but the gap between that standard and what I know is achievable by a good writer is nevertheless disturbingly large.

† In an amusing article entitled 'What happened to my paper?' in *Physics Today* (vol. 22, May 1969, pp. 23–25), the then editor-in-chief of American Physical Society publications, S. A. Goudsmit, describes what goes on in the editorial office of the largest A.P.S. publication, *The Physical Review*. Papers are given a similar treatment there, the process being very aptly described as 'laundering'. Goudsmit lists some verbal infelicities from papers submitted to *Phys. Rev.*, all of which can be matched by extracts from papers sent to *JFM*. One of the more harmless and comic examples quoted by Goudsmit which turns up often in slightly different forms is 'Let the applied field direction be in a direction parallel to the x-direction'. The author of that sentence presumably did not read his own paper after writing it. Any careful reading, as distinct from skimming down the page, inevitably shows ways in which the odd word, phrase or sentence can be improved, but the pristine state of many of the type-scripts received by *JFM* editors suggests that the authors did not think scrutiny was needed.

Why then do authors not respond to the two strong incentives to make each of their papers a literary and intellectual gem? The first reason, I believe, is that most scientific workers feel an even stronger incentive to get on quickly to the *next* paper. The peak of satisfaction in scientific work comes at the moment when one suddenly understands something or realizes the implications of an observation or sees how some difficult calculation can be made. After that moment of discovery there comes the less exciting task of exploring the consequences of the new insight and writing it up for others to read about. It requires patience and dedication in any circumstances to carry that demanding task through to completion, and, if the author already feels the excitement of the chase after another discovery, writing up the previous one is unlikely to get his undivided attention for very long. This association of 'writing up' with the relatively dull post-discovery phase poses a psychological problem for scientists. It is not found to the same extent in the humanities and other literary disciplines, because there the creative act lies much more in the writing itself and the excitement is sustained to the end of the composition. (Personally I find that the creative process *is* continued into the writing-up stage of the more theoretical type of scientific paper. Clear writing is possible only on a foundation of clear thinking, and my attempts to draft a paper usually lead to considerable clarification of my thinking about the problem and often to further useful developments.)

A second reason why so few published papers are a pleasure to read is that most authors lack the ability to make them so. That is a harsh and ungenerous statement, but I think it needs to be said openly so that the fact itself will be taken seriously. The level of ability of scientists to write with clarity, precision and elegance is regrettably low, for reasons which lie at least partly in the nature of their work. They get little practice at prose composition either during their training period or in employment in universities, government institutions or industrial laboratories. Moreover, it is a curious feature of scientific thinking that it may proceed by logic and physical insight and analytical formulation without the need for explicit expression in words. We 'see' things in non-verbal form. Conveying the same insight to a close colleague is likely to involve sketches and equations and key-words at a blackboard, with relatively few verbal constructions being used. Writing up work for publication is an occasional and unfamiliar task needing skills which are not otherwise cultivated.† As if this were not handicap enough, one sometimes also hears misguided advice – such as: aim at conciseness above all else, avoid explanatory interpolations, never use the first person – from people who suppose that a standardized telex style is essential for clarity.

If the present dismal standard of composition in scientific journals is to be raised, and if the preparation of a paper is to be turned into a minor art form, as is desirable in view of its dominant role in scientific communications, we shall need to proclaim openly and often the importance of good writing. And we shall need to find ways of showing young scientists how to present their work, just as we teach them other relevant skills. It is sometimes maintained that the inclusion of courses on the humanities in all undergraduate curricula would make engineers and scientists more literate, but I believe the help should be more specific (both as to who is taught and as to what is taught) and more intensive. I look forward to the time when instruction

† Contributors from non-English-speaking countries of course have formidable additional handicaps, and I exempt them completely from my strictures.

in the preparation of papers is included in the training of research students and is regarded as a vital part of that training. And of course it is essential that we convey to research students an appreciation of standards and a conviction that the way in which the written word is used for communication does matter.

The statistics of acceptance of papers

Thoughts about the effort that an author invests in the writing of his paper lead us naturally to the next stage in the publication process, viz. the acceptance of a submitted paper. Like most other scholarly journals, *JFM* makes a selection from among the papers submitted to it; some are accepted for publication, perhaps after revision which in some cases amounts to complete rewriting, and some are rejected. The ensemble of decisions taken on a large number of submitted papers represents the publication policy of a journal, statistically speaking, and it is this publication policy that, more than any other single factor, will give a journal its 'image'.

In the case of *JFM*, each of the two Editors and eleven Associate Editors acts independently and takes his own decisions on the papers submitted to him (or transferred to him, with his permission, from another editor). Every editor carries a sizable load of papers, and none of the names on the cover of *JFM* is there for decoration. By delegating the responsibility for each paper to one of a group of editors spread over different countries and different fields of research, we increase the possibility of an author being able to have his paper considered by an editor who lives in his own country, and so is more likely to know the author and to have some acquaintance with the background of the work and the institution in which it was done, and who has some specialist knowledge of the relevant area of fluid mechanics or the approach adopted in the paper. And the spreading of responsibility over editors in several countries makes the *Journal* genuinely international, like the subject.

But although each editor of *JFM* carries full responsibility for the fate of the papers submitted to him he is not alone. He can seek the opinion of another editor on tricky cases or novel issues (and there are many), and if he wishes he can turn unusually difficult decisions, perhaps involving a disgruntled author, over to one of the two Editors-in-chief. Meetings between two or more editors at conferences provide many opportunities for exchange of views about current problems. On 1 January and 1 July of each year every editor compiles a list of all the papers he has received during the previous six months and sends a copy to all other editors. This shows any trends in the submission of papers (e.g. growth or decline of certain subject areas, overloading of certain editors), and it catches the occasional innocent author who tries submitting the same paper to two different editors (yes!).

From the numerical data of these lists we work out for each editor the ratio of the number of papers accepted to the total number of papers considered over a sufficient number of years to smooth out fluctuations, and circulate the results so that each editor may see whether the standards he is applying are in line with those of other editors. If they are not he modifies his standards slightly so as to bring his acceptance ratio closer to the mean for all the editors. Some variation of the acceptance ratio as between one editor and another is inevitable, perhaps owing to differences in their 'catchment areas', and we see a need for some action only if an editor's acceptance ratio differs from the mean for all editors by more than 0·05. By keeping in touch with each other and achieving through discussion a common view of the general objectives of *JFM*, the editors have been able to maintain approximately the same acceptance ratios. At any given time there are likely to be only one or two editors (not always the same ones) whose acceptance ratios for the preceding five years are out of line.

From 1 January 1967 (the earliest date from which we have complete records of submitted papers) to 30 June 1980, 7254 papers were submitted to *JFM*. By the end of 1980, 3378 of these papers had been accepted for publication, giving a mean acceptance ratio of 0·47 for all editors. The year-by-year acceptance ratios fluctuate about this 14-year average but do not show any systematic trend.

This mean acceptance ratio of 0·47 for *JFM* appears from the available information to be lower than that for most other journals. From the *Year Book of the Royal Society* for 1979 and for 1980 I find that 600 of the 721 papers in the physical sciences received during the three years ending 31 August 1979 were accepted for publication in the Society's *Proceedings* or *Philosophical Transactions*, corresponding to an acceptance ratio of 0·83. An author wishing to have his paper published by the Society must find a Fellow of the Society who is prepared to communicate it to the Society, and it is probable that this procedure cuts out some substandard papers which would be rejected, thereby pushing up the formal acceptance ratio by an amount which is not easily estimated. Another journal for which data is available is *The Physical Review*, a very large journal which with its several sections publishes about 6 per cent of the world's journal literature in physics. Two sociologists, Zuckerman and Merton, have investigated the influence of various factors on the decisions made on the 14512 papers submitted to *The Physical Review* during the nine years 1948–1956, and, in the course of their extensive and interesting report† (which I shall want to refer to again later), they reveal that 95 per cent of the multiple-author papers, making up a little less than half the total, were accepted for publication, and 80 per cent of the remaining single-author papers were accepted. The authors mention that the multiple-author papers were often reports of experimental results, which they think are less likely to be found contentious.

In the same report the authors show the results of a survey of acceptance ratios for a number of journals in the humanities and the social and natural sciences, some of which are reproduced in table 1. From the names of some of the journals mentioned by Zuckerman & Merton I infer that most, and perhaps all, of the journals surveyed are published in U.S.A. No information is given about their size, and the averaging over various journals in the same group appears to give each journal the same weight. No engineering journals were included in the survey, which is a handicap to a comparison with *JFM*.

The most striking feature of this table is the contrast between the low acceptance ratios for journals in the humanities and social sciences and the high acceptance ratios in the natural sciences. Zuckerman & Merton base many of their remarks and conclusions on this contrast, which they formulate as a rule: the more experimentally and observationally oriented the journal, and the greater the emphasis on rigour of observation and analysis, the higher the rate of acceptance. They ascribe this variation in acceptance ratios in part to differences in the measure of agreement on standards of scholarship and research in the different disciplines. In the natural

† 'Patterns of evaluation in science: institutionalisation, structure and functions of the referee system', by H. Zuckerman & R. K. Merton, *Minerva*, vol. 9, 1971, pp. 66–100.

Field	Number of journals	Mean acceptance ratio
History, language, literature, philosophy, political science	15	0·14
Sociology	14	0·22
Economics	4	0·31
Mathematics, statistics	5	0·50
Chemistry	5	0·69
Biology, geology, geography	16	0·72
Physics	12	0·76

TABLE 1. Acceptance ratios for journals in different fields in 1967, quoted by Zuckerman & Merton.

sciences, they argue, the merit of a paper is more readily recognized by both author and editor, and authors tend to submit only those papers that they know are likely to be found acceptable. Another relevant factor suggested in their article is the larger amount of money for research in the natural sciences; this subsidizes publication in various ways and leads to more journal pages being available, thereby allowing a higher acceptance ratio.

Zuckerman & Merton may be correct in supposing that scholars in the humanities and social sciences are more naturally disputatious, but I do wonder whether the ordering *within* the natural sciences shown in table 1 is typical. I also doubt their hypothesis that there is wider agreement on suitability for publication in the sciences characterized by emphasis on rigour of observation and analysis. Note that the relative order of the three groups 'Mathematics, statistics', 'Chemistry', 'Biology, geology, geography', is in conflict with their rule; only the position of 'Physics' at the bottom of the table, with the highest acceptance ratio, conforms to their rule so far as ordering within the natural sciences is concerned. I think it possible that the position of 'Physics' in the table arises from the special practices and traditions of the community of physicists, in particular those in U.S.A. where the American Institute of Physics exercises such a strong influence; heavily subsidized journals (especially those supported by 'page charges', that anti-international device much used by the A.I.P.) do naturally come to the view that publication of a paper which later is seen to be of no value matters little and that of greater concern is the risk of rejecting a paper which might be of value. An indication of this carefree attitude, and of the considerable difference between *The Physical Review* and (I suspect) most other journals of physical science, is provided by the information from Zuckerman & Merton that between 60 and 70 per cent of all the papers submitted to *The Physical Review* during the period 1948–1956 were judged immediately by one or both of the two editors without the advice of referees. Each editor evidently dealt directly with an average of 10 papers a week, and in these circumstances it would hardly be possible to base a decision on a critical assessment of the scientific contribution made by the paper.

It is of course true that in the more exact sciences there is less scope for argument about what is correct and what is incorrect, but I think Zuckerman & Merton err in supposing that this is accompanied by wider agreement on suitability for publication. A paper should be interesting and scientifically significant, as well as being correct, and in my view a judgement of these qualities is *more* difficult in the more exact sciences and in the more theoretical areas of science because there it is more difficult to see the implications of results. I think this accounts for the position of 'Mathematics' at the low-acceptance-ratio end of the scientific fields in table 1. It has a bearing also on the position of the *JFM* acceptance ratio (0·47) relative to those given in the table.

Further information about acceptance ratios has been compiled recently by Elizabeth Nejman in a thesis to be submitted to the University of London for a Ph.D. Miss Nejman wrote to the editors of 75 leading journals in the physical sciences published in the Western world asking to be told, in confidence, their mean acceptance ratio over a period of several years. Even though she said that the information would be used solely in statistical summaries, only 40 editors provided data, and it was clear from the comments received that editors are sensitive to the possibility of the information leaking out; an editor of a journal with a high acceptance ratio does not want it to be thought that he welcomes whatever is submitted! Miss Nejman's histogram for a total of 44 journals shows that the acceptance ratios of about two-thirds of them are distributed fairly evenly over the range 0·67–0·86. The three lowest values, one of which was supplied by *JFM*, lie in the range 0·44–0·50. The overall mean (counting each journal as one, without regard for size, although divisions such as *Phys. Rev.* A, B, C and D are counted separately, and leaving aside the Royal Society publications in view of their special features) is 0·71. This is consistent with the figures in table 1 obtained by Zuckerman & Merton, since Miss Nejman's survey included both the high-acceptance A.I.P. journals that dominated Zuckerman & Merton's smaller group of 12 physics journals and a number of more theoretical and mathematical journals likely to have smaller acceptance ratios.

The evaluation of papers for JFM

I have not yet said anything about the criteria that must be satisfied for acceptance of a paper for publication in *JFM* or about the extent to which the acceptance ratio mentioned above results from a positive influence of the editors. We like to think that the decisions taken by the editors on the submitted papers are based on certain agreed standards, and that these standards reflect our ideas on what would best serve the interests of the fluid mechanics community. We certainly knew, right from the beginning, that we wanted to aim at acceptance standards higher than those currently used by other journals and that the editors would take a more positive role than is customary in determining, with the help of expert referees, what is published and the form in which it is published. The same criteria for acceptance of papers, the principles on which they are based, and the consequential relatively low acceptance ratio, have been maintained consciously and consistently over the 25 years of *JFM*'s life. This is quite long enough for a thorough test of the acceptability of these standards to the fluid mechanics community, and the generally favourable reactions of referees, readers and, not least, authors of submitted papers, have encouraged us to think that they are indeed acceptable, and even welcome.

It is not easy to give a specific and meaningful description of the criteria determining acceptability of a paper for publication in *JFM*, because value judgements are involved. My formal statement would be something like this: a paper is acceptable if it appears to make a significant contribution, directly or indirectly, to knowledge of fluid mechanics or to the application of fluid mechanics, and if it is likely to be readily understood by qualified readers. I add the words 'or indirectly' because a paper which puts forward a new experimental or analytical or numerical technique might well be valuable inasmuch as it enables others to make significant advances. Note also that a survey paper or a paper which interprets or illuminates previously published work might be acceptable, despite the fact that it contains no new development in itself, if it adds to the knowledge in the heads of readers. But when we consider a typical paper recording some development, an experimental or theoretical investigation of some flow system or process or phenomenon not previously studied, the application of the above criteria will clearly turn on the interpretation of the words 'significant contribution' and this will inevitably be subjective to some extent. Thirty years' experience of trying to answer the question 'Is this worth publishing?' put to me by research students and colleagues, as well as by authors of papers submitted to *JFM*, have given me some expertise of a practical kind in the taking of the decision, but I still have considerable difficulty in explaining to others exactly why I think a contribution is significant or not significant. A significant contribution often conveys new physical insight, but I have learnt to be cautious in using that as a test because sometimes the new physical insight has come years after the publication of what seemed initially to be a description or solution of a rather idealized problem.

I digress for a moment to illustrate by concrete example what I mean by this quality of significance, which I believe to be of the greatest importance in a highly developed and mathematicized (to coin a word meaning that the basic concepts and processes and their consequences have been given mathematical expression) science such as fluid mechanics. The example comes from the work of G. I. Taylor who, more than any other person known to me, had the ability to recognize phenomena or mechanical processes which everyone would later see to be important in the fluid-mechanical scheme of things. In 1952 G.I. learnt that an animal physiologist was measuring the rate of flow of blood in the arteries of animals by injecting a small volume of highly conducting liquid at some point and observing the variation of conductivity with time at an electrode placed some distance downstream. The physiologist had calibrated his measuring system using an independent means of obtaining the flow rate, but in conversation between them there arose the question whether the flow rate could be deduced from the measurements of conductivity as a function of time. G.I. perceived that it was an interesting question involving the combined effects of convection and diffusion of the conducting liquid in the tube, and he also saw that it would be worth while to investigate the way in which the conducting liquid spreads out longitudinally as it moves down the tube.

By some careful experiments and some novel mathematical analysis of a rather heuristic kind, he showed that, far downstream from the source in a straight tube, the instantaneous distribution of concentration of the conducting liquid has a Gaussian dependence on distance in the flow direction and that the Gaussian packet increases in width as $t^{\frac{1}{2}}$ about a centre which moves downstream at a speed equal to the mean speed of the fluid over a cross-section, these results being valid for both steady laminar flow in the tube and for statistically steady turbulent flow. The fact that the injected fluid ultimately occupies a Gaussian packet of width increasing as $t^{\frac{1}{2}}$ implies that the longitudinal spreading is equivalent to a diffusion process, and G.I. obtained estimates for the longitudinal diffusivity (which has the striking property of being inversely proportional to the transverse diffusivity – molecular or turbulent – essentially because the transverse spreading limits the differential convection) for both laminar and turbulent flow in circular tubes.

Prior to 1954, when G.I.'s first paper on this topic was published, the notion of a longitudinal diffusivity was not known. But since then it has been recognized as being relevant in a wide variety of contexts, in flow in rivers and estuaries, in oil pipe-lines, in water mains, in innumerable pneumatic and hydraulic industrial devices, in blood vessels, in tubules in plants, and a whole industry of extensions and generalizations of G.I.'s simple results for steady flow in a straight circular tube has grown up. There is no doubt that G.I.'s investigation of longitudinal dispersion was 'significant'. That is now obvious from the wide applicability of the concept and use of the results. The referee who received G.I.'s first paper on longitudinal dispersion could not be expected to have anticipated the many later applications, but he probably recognized the fundamental character of the result that differential unidirectional convection and transverse diffusion together yield a longitudinal diffusion process far downstream. 'A nice result!' he would have said, meaning that the understanding of the result requires some thought and gives satisfaction and aesthetic pleasure. The physical chemist J. W. Gibbs wrote, 'One of the principal objects of theoretical research in any department of knowledge is to find the point of view from which the subject appears in its greatest simplicity.' This provides another test of significance of a contribution, and on this test also G.I.'s papers on longitudinal dispersion would have passed.

If there were many papers with the same clear significance as this work by G. I. Taylor, the task of an editor would be an easy one! But in fact there are very few. The typical paper published in *JFM* makes a rather modest contribution to knowledge of fluid mechanics or to its application, and the identification and evaluation of that contribution requires a considerable amount of work by an editor and his referees. A little more than half of all the papers submitted to *JFM* did not, in the judgement of the editors, make a contribution worth publishing. Some were logically unsound or mathematically incorrect, some reported observations made with insufficient care, some were incomplete or too slight, some were based on unjustified hypotheses or approximations, and some described work which although correct appeared to be of too little interest or value.

This last category of paper presents especial difficulty for an editor because one cannot always measure interest or value, or significance as I have called it, objectively. But however difficult the making of a reliable assessment may be, it is important that it be attempted. I believe that the editors of a journal have a responsibility to select and publish those papers that are of interest and value, on the basis of the best available advice from within the scientific community.† There will

† A similar description of journals as being responsible for implementing the wishes of the scientific community on quality control was put forward by S. Pasternak, then editor of *The Physical Review*, in 'Is journal publication obsolete?', *Physics Today*, vol. 19, May 1966, pp. 38–43. The query in the title of the article reflected Pasternak's concern that the quality-control role of journals was being threatened at that time by two separate developments, one the wide-spread distribution of unrefereed preprints which were finding their way into reference lists, and the other a proposal for a government-funded scientific information centre in U.S.A. which would include all these preprints and institution reports.

be some who feel uncomfortable about this assumption of judicial powers by editors; and the fact that editors cannot readily be called to account for their actions may increase their discomfort. Such reactions are often accompanied by the view that a journal should provide 'open house' for all contributions which are not clearly incorrect or unsound; some will prove to be of no value but will be forgotten, so the argument goes, and waste is part of the price for ensuring that no good work is suppressed. For myself, I unhesitatingly reject this view. It may perhaps have been appropriate 50 or more years ago when research was carried out by a small number of people of proven ability, but is quite inappropriate in the present era in which research is cultivated on a large scale by government and industry, as well as by the greatly enlarged universities, and is an occupation for a very large number of people, relatively few of whom have real originality.

The maintenance of high standards is essential for the health of science, and journals are in a position to make a vital contribution to the maintenance of standards by careful and critical selection of the papers they publish. Papers of poor quality do more than waste printing and publishing resources; they mislead and confuse inexperienced readers, they waste and distract the attention of experienced scientists, and by their existence they lead future authors to be content with second-rate work. I believe that a large proportion of papers are submitted prematurely and that an appreciable proportion of those that do find their way into the scientific literature are not worthy of publication. In these circumstances the suppression of a number of papers by editors is a positive service to science.

I have often heard it said that an editor might fail to recognize the value of a paper and so a journal with a high rejection rate might unwittingly suppress an important development; and that this risk is so serious as to justify a less rigorous selection. Well, there have admittedly been some examples, in the history of science, of a piece of research being so original and unconventional in its approach and so novel in its conclusions as to arouse incredulity and even hostility among those who first heard about it. I do not know of any fluid mechanics papers of this type which were not ultimately published, but I can imagine that, for example, the editor and referee for L. F. Richardson's paper entitled 'Atmospheric diffusion shown on a distance–neighbour graph' (*Proc. Roy. Soc.* A, vol. 110, 1926, p. 709) had difficulty in assessing it. I had occasion, many years ago, to struggle with the unorthodox and idiosyncratic (and in some cases incorrect) ideas on turbulent diffusion put forward in this paper, and I am not surprised that its value remained unappreciated until many years after publication when it was noticed that Richardson's empirical $\frac{4}{3}$-power dependence of the relative diffusivity of a cloud of particles due to turbulence in the atmosphere on the cloud diameter coincided with the prediction from Kolmogorov's similarity theory. Now the point about important developments that are difficult to appreciate, like this work by Richardson, is that they are made by people of ability; and ability is *not* difficult to recognize. Richardson's inventiveness stands out like a beacon in this paper, and although the referee, if there was one, may not have been able to relate the paper to the state of knowledge in 1926, I feel sure he would have seen it as thoughtful and stimulating.

What is the chance that among the 5300 or so papers rejected by *JFM* between 1956 and 1981 there are some unrecognized significant developments? There is no way of getting hard information, but my guess is that the chance of some important work being unappreciated is small. An acceptance ratio of 0·47 may be small compared with that for some physics journals, but the standard of papers at the threshold of acceptance is nevertheless rather low on any absolute scale. We speak of a policy of 'rigorous selection', but again it is a relative term. For those who have looked at hundreds of papers in the light of reports from referees, two, three or even four to a paper, and have searched for signs of originality, insight and depth of understanding, the suggestion that some gems may have been overlooked would seem hardly plausible – there are not many gems even among the papers that are accepted! It could happen, I suppose, that an editor receives an obscurely written but worthwhile paper in a field far away from his own and chooses unsuitable referees who fail to convey to the editor their lack of competence and who advise rejection through being unable to see anything of value in the paper. But I cannot recall hearing subsequently about any such cases, and it would be no disaster if it did happen since there are many alternative journals to which a paper on fluid mechanics could be submitted. I feel more concerned about the possibility of many of the accepted papers being valueless than about the risk of overlooking the odd useful paper.

Despite these bold words about the desirability of editors being ruthless in their selection of papers for publication, I am in favour of editors being accountable for their actions in some way. (I, or my successors, may later regret giving away that hostage to fortune.) Editors have the right to decide on the publication of submitted papers by virtue of being representatives of the scientific community served by the publication, not as agents of the publisher, and I believe that the exercise of that right should in principle be subject to scrutiny by that same scientific community. The publication of new developments is an essential part of science and of scientific life, and it is important to all of us that it should be well managed. And if the author of a rejected paper seriously believes that the reasons for the rejection are ill-founded or inadequate, I think there should exist some agreed procedure whereby his complaint and the action of the editor could be examined by an arbiter. Scientists are remarkably content with the current arrangements, probably because the multiplicity of journals in every field of science allows a dissatisfied author to submit his paper elsewhere, with a high probability of success in due course. I do not regard this as an entirely satisfactory way of ensuring that a paper of value finds its way into print somewhere, because the existence of any error in the assessment of the paper is then less likely to be noticed. If the rejecting journal really has made an error of judgement, it would be to the benefit of the journal, as well as to the author, if that error was revealed and acknowledged, and if the assessment procedures were examined and perhaps modified in the light of the error. I think the idea of an arbiter, or arbitration committee, for each journal is worthy of consideration, although I have not thought out the details of how an arbiter would operate. A procedure which allowed the author of every rejected paper to appeal to the arbiter would of course not be workable; some way of selecting the genuine cases of possible misjudgement would need to be devised.

The role of referees

The practice of seeking advice on the publishability of a paper from qualified referees (or reviewers, as they are often called in North America, although this word does not seem as appropriate) is of such long standing, and is so widely used, that we take it for granted. It is a remarkable practice, unique to science in its scale and universality, and it is the effectiveness of this referee system that enables a journal to maintain high standards. In his book *Public Knowledge: The Social Dimension of Science* (Cambridge University Press 1966, p. 148), J. M. Ziman goes so far as to say 'The referee is the lynchpin about which the whole business of science is pivoted'. Referees certainly play an important part in the dissemination of the results of research, and editors would be helpless without them. As well as paying a tribute to referees – a group which includes a large part of the scientific community – for their willing and invaluable help, I should like to consider briefly the way in which the system works.

A rough quantitative idea of the scale on which the referee system is used at the present time can be gained in several different ways. The simplest is to note that if α is the average number of papers submitted for publication per research worker in fluid mechanics per year, and β is the average number of referees per paper, then on average each of those research workers is called on to referee $\alpha\beta$ papers per year. (One of the less enthusiastic referees on my list interprets this formula on a personal basis, and uses it as a defence against taking on any further papers when his quota of reports for a year has been filled. But he overlooks the fact that the burden of refereeing work is not, and cannot be, spread evenly and 'on average'; it falls more heavily on the people who, like himself, are recognized authorities on certain areas of the subject – and who inevitably are also asked to do a disproportionate share of the other odd jobs in a scientist's life, examining theses, supervising graduate students, writing testimonials, advising on research proposals, planning symposia, etc.) I would guess α is about 2. The average number of referees consulted per paper lies between 0 and 1 for *The Physical Review*, is probably between 1 and 2 for many journals, and is between 2 and 3 for *JFM*; and if a paper is resubmitted in revised form the same referees may be asked to look at it again. We could take $\beta = 2$ as a crude estimate for mechanical science. That makes $\alpha\beta$ about 4, and so, if the subset of referees is about a third of the total group of authors of papers, the average established research worker known to journal editors will be asked to be a referee for about 12 submitted papers per year. That looks about right to me, although I know that there are wide variations from one individual to another and that the figure is higher for people who have become well-known specialists in particular fields.

A referee needs to read a paper carefully and to think about it, and the total time needed for the preparation of a report on a normal paper for the editor takes at least several hours, possibly one or two days for a long or difficult paper. A task of this magnitude coming once every four weeks represents a fairly heavy load for busy people, and it says a great deal for the spirit of the scientific community that referees undertake it willingly, at a cost to their own free time (that is, at a cost to their own research), and for no reward or recognition, and without regard for the nationality of the author, editor or publisher. Referees no doubt gain a little from the opportunity to read about some current research several months before anyone else, and the task of examining critically some work in the referee's own field of interest is self-rewarding to some extent, but I believe that on the whole their help is given selflessly and freely as a contribution to the common purposes of advancement of knowledge and the maintenance of standards; long may it continue.

I have mentioned in passing that the average number of referees asked to report on a paper submitted to *JFM* is between two and three, and the fact that this is a larger number than for any other journal for which we have information perhaps calls for a comment. In the early days of *JFM* it was agreed among the editors that two referees should normally be consulted. This was larger than the norm for other journals, but we believed it was necessary to have two independent reports on a paper in order to be able to justify to authors the hard decisions associated with a relatively low acceptance ratio. Then during the sixties I became convinced that an even larger number was needed to give a reliable and complete picture of the merits of a paper, and I experimented for some time with three and sometimes four referees to a paper. The results were disturbing, in one sense, because they showed that significant variations of opinion on a paper can occur, and that an inaccurate impression can be gained from only one or two reports. Of course there is usually complete agreement on a paper which is first-rate as it stands or on one which is clearly hopeless. But for the broad intermediate group, comprising about three-quarters of all papers submitted to *JFM*, referees are likely to vary in their opinions both on whether the paper contains material worthy of publication and on how it might be improved. My experience showed me that the contribution made by a third (and even a fourth) referee was seldom redundant. As well as increasing the reliability of the overall assessment of the value of the paper the additional referee usually provided different suggestions for improvement of the paper. The different opinions, which were not necessarily incompatible, usually arose from referees with different interests and skills concentrating on different parts or aspects of the paper. Early in the seventies I therefore urged my fellow editors also to try consulting three referees for each paper. That number is now the norm for most *JFM* editors, although there are some who feel that the gain does not compensate fully for the extra burden on referees and that it adds to the difficulty of finding enough qualified referees in certain specialized areas.

I am in no doubt that there is a connection between the number of referees per paper and the acceptance ratio of a journal. For the period 1948–1956 covered by the Zuckerman & Merton survey, about two-thirds of the papers submitted to *The Physical Review* were judged without the help of referees and over 90 per cent of these were accepted. Near the other end of the scale is *JFM* with between 2 and 3 referees per paper and an acceptance ratio of 0·47. Zuckerman & Merton sought an explanation for the variation in acceptance ratios in the nature of the subjects of different journals, but I think it is likely that the number of referees per paper is an equally strong determinant. The number of referees per paper is of course not an intrinsic factor, and one may ask how variations in this factor from one journal to another arise. As with so many other questions about editorial procedures, we do not have enough information to be able to give an answer.

It is often supposed that the task of the editor and referees together is to give a straight yes-or-no decision on a submitted paper; and perhaps this is the case in certain fields. But in a highly developed scientific field like fluid mechanics, in which the fundamental principles have been established for a century or so and research is concerned with the interaction of processes understood separately and with applications to novel physical conditions or engineering needs, it is more complicated than that. The typical report from a referee on a paper submitted to *JFM* is to the effect that some parts are interesting and some are not, the results would be more valuable if certain extensions could be made, the author has overlooked some relevant earlier research, parts of the analysis could be improved by use of an alternative method, a hypothesis or approximation is not given adequate justification, etc. The task of a *JFM* referee, as interpreted by nearly all the referees for papers submitted during these past 25 years, is, first, to advise the editor on whether the paper is suitable for publication, either as it stands or after appropriate revision, and, second, to point out the weaknesses of the paper and indicate how it might be improved. The great majority of the papers published in *JFM* have needed some modification of the first submission, usually a sufficiently extensive modification to justify a second consultation of at least some of the referees who reported on the first submission. The vast amount of constructive work by referees that led to these improvements in submitted papers is as valuable a part of their work as the recommendation on whether to accept.

It has been a pleasant experience to find that on the whole authors do value the critical comments made by referees. There are of course some authors who are emotionally attached to the form in which they conceived their papers and who find it difficult to accept criticism, especially when doing so might cause a little delay in publication, but these are a small minority. Most authors are delighted that someone has read their paper carefully (and I sometimes wonder if there will be another equally careful reader of the whole paper after it is published) and have thought about it and are willing to make suggestions for its improvement. The referees usually express their criticisms bluntly, as is appropriate in a report to the editor, but my own practice is to show authors an unexpurgated copy of the report unless it is likely to wound unnecessarily. This willingness of referees to give, and of authors to accept, criticism of research contributions shows an ability of men to co-operate in an impersonal cause which is rare in human affairs and is in my view an example of the elevating character of scientific work.

I hasten to add that I know that referees are only human, that they are creatures of their own scientific training and experiences, that they have vested interests in certain approaches and points of view, and that they may make only a superficial assessment of a paper if they are short of time or plain lazy (all good reasons for not relying on the opinions of one or two referees alone). In the article in *Minerva* referred to earlier, Zuckerman & Merton made a systematic analysis of the files on 1057 single-author papers submitted to *The Physical Review* and sent out to 354 referees, in order to see if there was any evidence of bias either in the choice of referees or in their recommendations on the papers. It is a thoughtful and interesting study, for which scientists should be grateful, although the sociological terminology is at first a little strange.† ('The referee is . . . an example of status-judges who are charged with evaluating the quality of role-performance in a social system.') They wished in particular to investigate the relevance of the scientific rank or status (as measured by the usual public criteria) of the author to the choice of referee and the influence of the ranks of the author and the referee on the decision made on the paper. For this purpose they divided up the authors of the 1057 papers and the 354 referees each into three groups, a small group of physicists of internationally recognized distinction, a larger group of those judged important enough to be included in the A.I.P. archives of contemporary physicists, and a much larger third group of those

† A short version of the article has been prepared for scientists by the same authors and is published in *Physics Today*, July 1971, pp. 28–33, under the title 'Sociology of refereeing'.

remaining, and then they examined statistically the interaction of each of the three groups of referees with each of the three groups of authors.

The conclusions turned out to be mostly negative, and can be summarized as answers to three questions. First, 'are there patterns of allocating manuscripts to referees variously situated in the status-hierarchy and are these allocations related to the status of authors?' Zuckerman and Merton could find no pattern, and although referees tended to outrank authors this was no more than would be expected from the selection of referees on the basis of their expertise and competence. Second, 'does the decision to accept or reject a paper depend on the status of an author?' This was a difficult question to answer from statistics alone. The figures showed that the status of an author was positively correlated with acceptance and that age of an author of given rank was negatively correlated – both being just what one would expect if referees based their recommendations on the quality of a paper alone. Third, 'are there any differences in acceptance ratios associated with the *relative* status of referees and authors?' Again the answer appeared to be no; the chance of a paper by an author in any one of the three status groups being accepted was not affected by the status of the referee, neither favourably, as one might suppose in the case of a lower-ranking deferential referee, nor unfavourably, as one might suppose in the case of an equal-ranking competitive referee.

Zuckerman & Merton's discussion of the role of referees is interesting, but their extensive investigation of the files of *The Physical Review* yielded very little new information. It is perhaps unfortunate that they based their enquiry on a journal which in many respects is untypical, even among journals of physical science. One cannot expect to be able to find much evidence of bias in the selection of referees and in the recommendations of referees in the case of a journal which consults referees for only a minority of the papers submitted and which accepts between 80 and 90 per cent of all submitted papers.

One suggestion for improvement of the referee system which is raised from time to time, and is strongly supported by some, is that the identity of referees should be revealed to authors. The argument behind this suggestion is that referees whose names are revealed will be more likely to take their job seriously and to make a careful and thorough assessment of the paper and at the same time less likely to take a partisan or mischievous stand. There are also some referees who feel that anonymity is incompatible with the ethos of rational debate in science. I respect all these arguments and feelings, but I think there are even stronger arguments in favour of the present well-tried and reasonably successful system of depersonalized reports from referees. It is true that a report on a paper often reflects strongly the special opinions and previous publications of the referee (not always to the author's disadvantage) and may exhibit partiality, but an editor who has consulted this referee before is able to see this and to allow for it. Those who believe that a named referee would be more likely to make a careful assessment of the paper are probably thinking mainly of encouraging, complimentary, helpful remarks; will named referees also be willing when appropriate to be severe and critical and to recommend rejection? An open refereeing system might work tolerably well for a journal which accepts nearly every paper it receives (*The Physical Review*?), but I do not think it would be possible for *JFM*. Rather than face the hassle to which a signed critical report would expose them, many referees would either decline to act or suppress their criticism, either of which would have serious consequences for *JFM*. (However, if after preparing his report a referee would like his name to be disclosed to the author, either on general principle or because he thinks it would be profitable to discuss some issues raised in the paper with the author directly, I see no objection to that.)

As for the conflict between the anonymity of referees and the scientific ethos, I accept that there is need of accountability in the system. But I think it is the editor who should be accountable to an author for the decision on his paper, not the referee. The referee is advising the editor, and it is the editor who takes the responsibility for accepting or rejecting the paper. Provided that the referee's report contains a reasoned justification for his conclusions or recommendations, so that it can be examined as evidence concerning the value of the paper and can be argued against by the author, I do not regard the withholding of the referee's name from the author as unethical. It may not be pleasant to be passing judgement on one's colleagues,† but I think ethical considerations should be concerned more with accessibility of the full referee's report, including the arguments on which the recommendation is based, than with revelation of the referee's name. I may say that in my experience authors do sometimes argue vehemently against the criticisms and conclusions of referees on their paper. When that happens, it should be the responsibility of the editor to try to justify to the author the decision made on the paper. The editor might ask the referees if the author's reply affects their opinion of the value of the paper, but it must be the editor who is saying no, or yes subject to some conditions, and not the referees. The author is then confronting an actual person, not an unknown faceless critic.

A more novel suggestion for change in the mode of operation of the referee system was made in 1978 by a publisher (Elsevier North-Holland) considering the establishment of a new journal in fluid mechanics. In a letter soliciting the views of a number of fluid dynamicists, the publisher explained the proposed editorial policy as follows:

(1) 'Adverse reviewers' reports on matters of opinion or judgement would not be grounds for rejection of a paper' (nothing was said about the grounds on which a paper might be rejected);

(2) 'The reviewers' reports would be published together with the paper', with the reviewer's name attached or not at his choice;

(3) 'The author of a paper would have the opportunity to add a reply to the reviewer's comments or to withdraw the paper.'

The publisher's letter added 'It is believed that this policy will encourage the circulation of new but controversial ideas, permit public access to the often informative exchanges between authors and reviewers, provide readers with independent assessments of the paper and encourage constructive criticism by enabling referees to obtain credit for their efforts.'

† Some journals have tried removing the name and address of the author from the title-page of copies of a paper sent to referees, presumably in order to lessen the discomfort felt by some referees when being critical of a named person and to eliminate any influence which the author's status might have on the referee's recommendation. I think all these experiments have lasted for a short time only, for the obvious reasons that if the author is unknown to the referee the anonymity is unnecessary and if the author is known to the referee it is usually possible to deduce his identity from evidence within the paper. Even if the author's identity could be concealed in practice, I doubt if the device would achieve anything except some apparent symmetry or evenhandedness in the exchange between author and referee.

Well, if one assumes that the typical paper in fluid mechanics contains new and potentially important although controversial ideas, that the typical criticism of a paper made by a referee is a product of an *alternative* point of view or opinion, and that juxtaposition of the paper and the referee's comments would provide an interesting and illuminating clash of opinions, there might be some point to this editorial policy (aside from the dubious idea of referees gaining credit from the publication of their criticisms). But who on looking at the papers that are submitted could accept those assumptions? The reality is that significant new ideas, whether controversial or not, are rare, that the typical paper needs considerable improvement before it is worthy of publication, and that the typical criticism of a referee is useful to readers only in so far as it leads to amendment of the paper before it is published. I think most people would prefer to be able to read a published paper in the form which author, referees and editor collectively believe to be optimum, rather than see the paper in the originally submitted form together with criticisms and suggestions from referees as to how it should have been written. Nothing came of the Elsevier North-Holland proposal for a new journal so far as I know. The suggested referee procedure may perhaps be worth trial for the few papers that contain new ideas in need of discussion by specialists, although the selection of certain papers for such special treatment would raise problems.

The future of journals

The future of conventional scientific journals has been said to be uncertain for many years. Journals have remained in much the same form for more than two centuries, thereby providing a standing invitation to reformers to think of some better way of disseminating new scientific knowledge. Some serious disadvantages of the journal system are evident: printing is very expensive, especially when mathematical equations and photographs are involved; storage in libraries of the large amount of paper contained in runs of several decades of all the journals in a particular field is also expensive; publication of journals is a commercial enterprise in many countries, and may be subject to pressures unrelated to scientific needs. The economic disadvantages are mainly a consequence of the inherent inefficiency of a process of expensive reproduction and distribution of all (acceptable) papers in order that each of them will find its way into the hands of the very small number of people who wish to read it.

I remember a very critical analysis of the journal system by J. D. Bernal in his book *The Social Function of Science*† which was the bible for young scientist-radicals in the late thirties and early forties when the need to be anti-fascist made left-wingers of us all. Bernal proposed an alternative and apparently more rational system which would ensure that papers were reproduced for, and only for, those who declared an interest in them. The idea was that after scrutiny and acceptance by an editorial board the type-script of a paper would be deposited in some central archive, with photographic copies made available to individuals on demand after publication and distribution of classified sets of summaries of papers. It is tidy and economical, but it has the serious drawback of not providing for unexpected cross-links of thought which arise when one glances through a paper without intending at that moment to

† Routledge and Sons, 1939.

study it carefully. The chance sight of a photograph or diagram or formula frequently generates an interest in a paper which was not expected from the summary alone. Browsing through journals in libraries is both pleasant and stimulating, and the opportunity to browse is a considerable merit of the present system. Since Bernal wrote his book there has been an enormous increase in the number of scientific workers throughout the world, and – bearing in mind the paper-collecting propensity in most younger scientists revealed by availability of the modern copying machines – it is possible that the volume of paper needed for Bernal's scheme would be greater than that required by the journal system and that the cost would not be any less.

Variants of Bernal's idea of a central archive, with individual papers available on demand, which make use of computer facilities are currently being discussed. The Primary Communications Research Centre at Leicester University has recently published a study† of the possibilities opened up by the ability of microprocessors and distributed computer facilities to store, release and reproduce information. This study envisages an 'electronic journal', which is effectively a computer system taking the place of Bernal's central archive. A newly submitted paper is fed into the computer system, perhaps at the author's own institution, and all subsequent communications between 'editor', referees, author and readers can be conducted via terminals of the computer system. When a paper has been 'accepted', perhaps after revision suggested by referees, it is put on one of the regular lists promulgated by the editorial centre, and readers in universities and research institutions can then call up the abstract, or the whole paper if they wish, on the display screen of their communications terminal. For those who like to do their reading in different circumstances, e.g. while travelling or at home with their feet up, a hard copy of the paper can be obtained from the terminal.

This modern version of the central archive scheme has the improvement of allowing a form of browsing through papers at a terminal. (Peering at a screen full of those ugly little letters and trying to remember what one saw on the previous 'pages' falls well below looking through an attractively printed journal as a pleasurable experience, but we must allow for developments in computer displays.) It appears to solve completely the storage problem, since libraries need hold on paper only lists of the titles and authors, and perhaps also abstracts, of papers and associated reference information. It has great flexibility, and would allow later revision of papers in the light of further developments (or more sensible second thoughts). Readers would have immediate access to other papers referenced in the paper being studied. Literature searches could also be made at a terminal as soon as large numbers of papers had been fed into the system. International distribution would presumably be via space satellites. The initial cost of providing the required very large number of communications terminals with compatible facilities would of course be enormous, and probably prohibitive in the near future.

A by-product of a scheme of this kind, with consequences which are difficult to predict, would be the blurring of the distinction between papers which are 'accepted' and those which are not. Preprints would no doubt be distributed by the computer system once it had been set up, and the only visible distinction between an accepted

† *New Technology and Developments in the Communication of Research during the* 1980*s*. Available from the Centre, £3.00.

paper and one fed informally into the system would be the appearance of the former on the list of accepted papers; the mark of approval conferred by the editorial board on accepted papers would thus be given little in the way of tangible expression, and some readers might choose to ignore it. Moreover, since the cost is associated almost wholly with the initial provision of equipment and not with the feeding-in and storage and retrieval of additional papers, the editorial board could no longer cite cost as a reason for declining papers. In these circumstances the role of the editorial board might become mainly organizational, with all submitted papers which pass some perfunctory test of value being fed into the computer system and distributed to all who wish to see them. A good thing or a bad thing?†

Personally I should regret the introduction of any system which did not allow me to hold in my hand, and take into a quiet corner of my own choice, a thing like a book with paper pages which are printed in attractively designed type with equations, tables, diagrams and photographs and which describe good work on fluid mechanics in language composed so as to please and enlighten. The present journal system does that rather well, and it will take some beating. The point on which the conventional system has hitherto seemed to be most vulnerable is the high cost of printing. But microprocessors are revolutionizing printing, along with other industries. The preparation for printing need no longer consist of casting metal type and arranging it in rectangular frames corresponding to a page. Instead, a person feeding in the text at what looks like a typewriter keyboard activates a computer-controlled laser beam which imprints on a photographic plate an image identical to the final printed page, and this master plate is then duplicated in materials suitable for the actual printing. As envisaged with the 'electronic journal' mentioned above, the keying-in of the text of the paper might be done at an early stage, perhaps at the author's own institution, and stored on magnetic tape or floppy disk which is delivered to the editorial office. This single record of the paper could be used to provide hard copies for referees and then, after revision by the author if necessary and after amendment in the editorial office, could be used to drive the printer's photo-typesetter. There is here the possibility of an appreciable part of the cost of typesetting being passed back to the first and only keying-in process, although there are formidable problems of standardization of the recording of mathematical formulae to be overcome, aside from the capital cost of the word-processor equipment, before journals will be able to realize these economies.

Those of us who are fond of reading the printed word in its familiar form hope that journals will survive for many more years. *JFM* seems likely to outlive me, and I look forward to reading, 25 years hence, about the things on the mind of the Editor at that time.

† The editor of *The Physical Review* in 1966 saw a rather similar threat to the traditional role of scientific journals from some other current developments (see the footnote to page 15), and left readers in no doubt about where his preferences lay.