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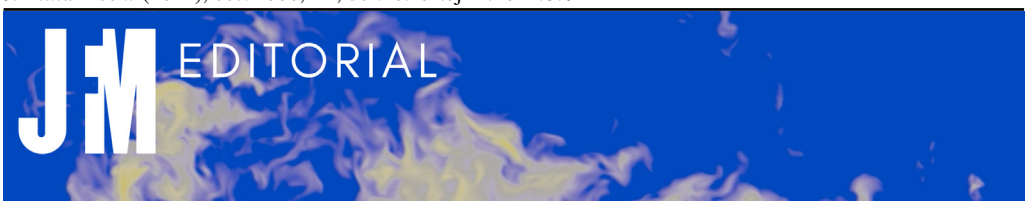

The early years of the *Journal of Fluid Mechanics*

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1. Introduction

A history of fluid mechanics must rely primarily on written evidence, whether in the form of printed sources or surviving correspondence. But recent history may also rely to some extent on the memories, however fallible, of those who were involved in the events described. Thus it seemed appropriate, when invited to contribute to this special issue of *Journal of Fluid Mechanics*, or briefly *JFM*, that I should attempt to provide some account of the early days of the *Journal*, based on my own memories, such as they are, and on correspondence that survives from the early years of *JFM*; I restrict myself to the period 1955–1962. This correspondence is archived in the Wren Library of Trinity College (where the Founder Editor of *JFM*, George Keith Batchelor, was a Fellow from 1947 until his death in 2000). The complete collection provides a valuable resource for future historians of science of that era.

I was privileged to become a research student in 1958 at Trinity College, Cambridge, under Batchelor's guidance; I was awarded my PhD in 1962, and had been recruited by him even a year earlier to help with copy-editing for the *Journal*. I thus worked 'at the coalface' for several years, prior to becoming co-Editor with Batchelor in 1966, a position that I then held until 1983. I was thus able to observe with ever-growing admiration and respect Batchelor's handling of *Journal* matters over an extended period.

A colloquium that focused on the history of fluid mechanics during the century 1870–1970 was held in October 2016 at the Institut de Mécanique des Fluides de Toulouse. One important historic event that fell within the century 1870–1970 on which we were then asked to focus was the creation in 1956 of *JFM*, widely recognised now as the leading international journal covering fluid mechanics in all its aspects. Batchelor's achievement was of great importance for the subsequent development of our subject. I therefore lectured at that colloquium on events surrounding the early years of *JFM*, and this lecture was published in *Comptes Rendus Mécanique*, **345** (7), 2017, 498–504. It seemed appropriate to simply offer this paper for re-publication in this historic issue of *JFM*; this is therefore re-published now with minimal change, and with the permission of the Académie des Sciences, Paris.

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2. The editorial team

The first issue of the *Journal of Fluid Mechanics* appeared in May 1956. On the inside cover of this first part, we read: 'The JOURNAL OF FLUID MECHANICS exists for the publication of theoretical and experimental investigations of all aspects of the mechanics of fluids, and is issued in six parts per volume.'[1] The Founder Editor was named as Dr G.K. Batchelor, whose address at that time (three years before the foundation of the Department of Applied Mathematics and Theoretical Physics) was given as the Cavendish Laboratory, University of Cambridge (he was actually a Lecturer in the Faculty of Mathematics and a Fellow of Trinity College, Cambridge). There were three Associate Editors: Professor G.F. Carrier (Harvard), Professor W.C. Griffith (Princeton University) and Professor M.J. Lighthill (Manchester University) (see figure 1). The journal was initially published by Taylor & Francis Ltd, and the price was '£1 ($3) per part'; the exchange rate of pounds to dollars has decreased over the last 60 years!

Two 'Assistant Editors' were also named on this first part of the journal: Dr T.B. Benjamin and Dr I. Proudman, both colleagues of Batchelor in Cambridge. Brooke Benjamin was in the Engineering Department of the University; he would later serve for a period (1961–1965) as co-Editor with Batchelor. Ian Proudman was the son of Joseph Proudman (1888–1975), early pioneer of dynamical oceanography; Ian had been Batchelor's first research student, and had just completed with him a major work (Batchelor & Proudman 1956) on the large-scale structure of homogeneous turbulence; he was destined to leave Cambridge in 1963 to take up the Chair of Mathematics at the new University of Essex in Colchester. I will comment on the role of the Assistant Editors in § 6.

We may suppose that Batchelor must have conceived the idea of a new international journal of fluid mechanics in the spring or summer of 1955, perhaps earlier. It would have been prudent to consult widely with colleagues, both national and international, and to persuade the chosen publisher Taylor & Francis that this was a viable project. Batchelor was already established as a leading authority on turbulence, having published a series of important papers in this field (several in collaboration with Alan Townsend) since 1946, culminating in his 1953 CUP research monograph *Homogeneous Turbulence*. By 1955, at the age of 35, he had already developed a vision for the subject of fluid mechanics, both as to how it should be taught, and as to the manner in which research in fluid mechanics should best be promoted. He recalled much later (Batchelor 1981) what motivated him to found the *Journal*; I quote here the relevant paragraph in full.

> 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

[1] An announcement had earlier appeared on the inside front cover of the September 1955 issue of the *Philosophical Magazine*. It stated: 'Taylor & Francis, in consultation with the Editor of the *Philosophical Magazine*, propose shortly to start a new international journal dealing with Fluid Mechanics, under the Editorship of Dr G.K. Batchelor (Cambridge). A notice giving particulars of the policy of the new journal will appear shortly.' The Editor of *Phil. Mag.* at that time was Nevill Mott, FRS, Professor of Physics and Head of the Cavendish Laboratory in Cambridge.

George Keith Batchelor
1920–2000
Founder Editor of *JFM*
1956–2000
Aged 36 in 1956

Michael James Lighthill
1924–1998
Associate Editor of *JFM*
1956–1978
Aged 32 in 1956

Wayland Coleman Griffith
1925–2003
Associate Editor of *JFM*
1956–1988
Aged 31 in 1956
University Archives Photographs,
UA 023.024 item #023127,
Special Coll. Research Center,
NCSU Libraries

George Francis Carrier
1918–2002
Associate Editor of *JFM*
1956–1986
Aged 38 in 1956

Figure 1. The original JFM editorial team.

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.

It is true that pre-existing journals containing papers on varying aspects and applications of fluid dynamics were scattered over a wide range, both geographically and by subject speciality. Journals referenced in that first part of *JFM* included the following.

National Academy Journals: *Phil. Trans.* A; *Proc. Roy. Soc.* A; *Austr. J. Sci. Res.* A; *Can. J. Chem.*; *C. R. Acad. Sci. Paris*; *Proc. Acad. Sci. Amst.*
Journals covering special fields that included fluid dynamics: *J. Aero. Sci.*; *J. Roy. Aero. Soc.*; *J. Met.*; *Quart. J. Roy. Met. Soc.*; *J. Coll. Sci.*; *Z. Phys. Chem.*; *J. Appl. Phys.*; *Phys. Rev.*; *J. Chem. Phys.*; *Ann. der Phys.*
Journals having broad coverage of mechanics and mathematics: *Quart. J. Mech. Appl. Math.*; *Z. Angew. Mat. Mech.*; *Proc. Cam. Phil. Soc.*; *Commun. Pure Appl. Math.*

Strong competition might therefore be expected for anyone daring to set up a new journal encroaching on the various specialist fields of application, and the indications are that Batchelor did indeed run into some opposition as his intentions were recognised. He was also apprehensive about competition from *Physics of Fluids*, established so soon after *JFM* and apparently in competition with it. He eventually recognised that the threat of competition was not serious, and that there was room for both journals. Batchelor wrote to Hans Liepmann on 27 September 1962 (the second paragraph of a three-paragraph letter):

> You certainly should not apologise for submitting a paper to the *Physics of Fluids*. I may prefer my own way of running a journal, but there is room for the two journals and I am quite happy to see good papers published in *Physics of Fluids*. NEVERTHELESS we are not satisfied with anything short of the best, and look forward to seeing a paper from you in the not-too-distant future.

Despite the competition from pre-existing journals in 1955, Batchelor pressed ahead with his plans, gaining first the support of James Lighthill (FRS 1953), aged 31 in 1955 and already renowned for his pioneering work on aircraft noise. Few letters relating to the *Journal* survive from that period, but among them is the following from Lighthill to Batchelor on 11 October 1955:

> Many thanks for your letter of Oct. 7th. I quite understand your remark about the position regarding an American Associate Editor. I am mentioning the new Journal to all fluid dynamicists whom I meet on my various travels about England, but I am at present holding up writing to Americans until after you have seen Griffith on Friday, because it is quite likely that you may accept him as an Associate Editor on that day. I can then write in more definite terms to my American friends about the new Journal, and about the possibility of their contributing to the early numbers.

So Lighthill was evidently already ‘on board’ as an Associate Editor, and an approach to Wayland Griffith was imminent; no doubt he was due to give a fluid dynamics seminar in Cambridge on the Friday of that week (13 October). In the event, Wayland did indeed agree to become an Associate Editor, in which capacity he served for 31 years until 1987. The

approach to George Carrier was presumably made at about the same time; he also accepted appointment, and served until 1986. The relative youth, enthusiasm and extended loyalty to the *Journal* of the initial editorial team gave it an exceptional stability, encouraging others also to accept editorial responsibility as it expanded in later years.

3. The first issue

From October 1955 on, events moved rapidly: papers were commissioned for the early parts of the journal and subjected to what must have been a fairly rudimentary peer review; and the *Journal* was in effect launched with publication of the first part in May of the following year. This first issue consisted of 128 pages and contained 8 papers, all 'received' in the period November 1955 to January 1956. These papers, by an impressive group of authors, covered a good spread of topics from gas dynamics and shock waves at one end, to turbulence in a meteorological context, geophysical fluid dynamics and boundary layer theory at the other, as well as Lighthill's own paper 'Drift', the most compact title that has ever appeared in the *Journal*, perhaps indeed in the whole corpus of scientific literature.[2] Here are the contents, and I include the institutions of the authors as recorded at that time.

Van Dyke, M.D. [AMES Aeronautical Laboratory, Moffett Field, California] The slender elliptic cone as a model for non-linear supersonic flow theory.
Saffman, P.G. & Turner, J.S. [Trinity College, Cambridge] On the collision of drops in turbulent clouds.
Lighthill, M.J. [University of Manchester] Drift.
Woods, L.C. [University of Sydney, Australia] On the thrust due to an air jet flowing from a wing placed in a wind tunnel.
Blackman, V. [Princeton University] Vibrational relaxation in oxygen and nitrogen.
Crease, J. [National Institute of Oceanography, Surrey] Long waves on a rotating earth in the presence of a semi-infinite boundary.
Glauert, M.B. [University of Manchester] The laminar boundary layer on oscillating plates and cylinders.
Rott, N. [Cornell University] Diffraction of a weak shock with vortex generation.

Thus four from the UK, three from USA, doubtless reflecting the choice of Editors, and one from Australia. For what it's worth, the numbers of Google citations of these papers as at 6 October 2016 were respectively 29, 1127, 146, 3, 294, 42, 108, 140; quite a spread if we take this as a measure of 'impact'!

A letter from Milton Van Dyke to Batchelor (1 October 1955) survives, giving the flavour of the initial correspondence with early contributors to the *Journal*:

> Dear George,
> Your good letter has shamed me. I had of course meant to write long ago to thank you for your extraordinary kindness to us, which helped to make our stay such a pleasure. . . .
> I was pleased a few weeks ago to see in *Phil. Mag.* an announcement that your *JFM* is to be a reality, and now your enquiry about contributions is most flattering. Nothing would please me more than to mark my break with the past by sending you a nice paper on viscous cross-flow, but it's unfortunately still a bit too early for that. I am however trying to write up a remnant of my past of high-speed flow and second approximations . . . I seem not to be able to think of a less ponderous title than 'The slender elliptic cone as an example in nonlinear supersonic flow theory.'

[2] *Dust* by Caroline Steedman is a competitor, but this is a book, not a scientific paper, and it also has a subtitle!

> . . . I might send it to you in a month or two if you think it might be suitable – or would you suggest your associate editor M.J.L. since it's more in his line, and we once discussed it?

4. The fertile Cambridge environment

Batchelor was not of course working in a vacuum. There was a strong tradition of research in fluid mechanics in Cambridge, with roots going back to Isaac Newton, and nourished since the 1840s by Stokes, Maxwell, Lamb, Rayleigh, and more recently G.I. Taylor, Batchelor's research mentor in the immediate post-war years. Taylor was an acknowledged genius, who worked on his own in a small room in the old Cavendish Laboratory, with little need for collaborative interactions. Nevertheless, his reputation was such that others would flock to Cambridge to be near this source of inspiration. Batchelor had, from the early 1950s, great ambition to build a research group in turbulence, and he succeeded in doing so, with research students, research fellows (post-docs) of Trinity College, Cambridge, and a flow of distinguished senior visitors from overseas. Figure 2 shows a rather ill-focused photograph of this group as it was in 1955, with G.I. Taylor third from the left in the front row, flanked by Batchelor and Townsend. On Batchelor's left (i.e. to the right in the photo) is Fritz Ursell, who later became Beyer Professor of Mathematics at the University of Manchester (succeeding Lighthill, who moved to become Director of the Royal Aircraft Establishment in Farnborough); and on his left is Milton Van Dyke who was then spending a sabbatical year in Cambridge. Behind Batchelor is Owen Phillips, in the centre of the back row is Philip Saffman, and in the right-hand corner of the back row is Stewart Turner, all three still then at various stages in their PhD research. Saffman's work with Taylor (Saffman & Taylor 1956) marked him as a rising star; and Phillips and Turner were soon to make their marks with pioneering work in the emerging field of geophysical fluid dynamics. We may be sure that this group of extremely talented people provided Batchelor with all the support, both moral and practical, that he might have needed in the process of setting up the *Journal*.

5. Change of publisher

All was not well, however, with *JFM* in the first two years of its existence. Batchelor was evidently unhappy with its slow initial growth, and particularly the difficulty of attracting new subscribers, and he ran into problems with the publisher Taylor & Francis, that, based in London at that time, seemed relatively remote and impersonal. Batchelor's concerns are evident in a letter to Professor T.A. Cherry of the University of Melbourne (Batchelor's alma mater) in response to his enquiry concerning the progress of *JFM*. Batchelor wrote to Cherry on 24 April 1957:

> Dear Tom,
> Thank you for your very kind remarks about my election to the Royal Society. I was honoured to have as company Sir Lesley Martin, who taught me a good deal of modern physics when I was an undergraduate.
> I am interested to hear that the Australian Mathematical Society is thinking of starting a journal. I do not think that the *Journal of Fluid Mechanics* has yet been in existence for long enough to make its present circulation significant, but, for what it is worth to you as information, the circulation is now between 400 and 500. It is rising rather erratically, but if I smooth out many of the fluctuations, I find an average rate of increase of circulation of about 40 per month. Whether this will soon fall off leading to some kind of upper limit, I cannot tell. I hope

The Fluid Dynamics Group at the Cavendish Laboratory, 1955

Back row: Ian Nisbet, Harold Grant, Anne Hawk, Philip Saffman, Bill Wood, Vivian Hutson, Stewart Turner

Middle row: S.N. Barnes, David Thomas, Bruce Morton, Walter Thompson (G.I.'s technician), Owen Phillips, Freddie Batholomeusz, Roger Thorne

Front row: Tom Ellison, Alan Townsend, Sir Geoffrey Taylor, George Batchelor, Fritz Ursell, Milton Van Dyke

Figure 2. The fluid dynamics research group, Cambridge 1955.

> not because we need many more subscribers than we have at the moment for the journal to pay its way.
>
> Financing a new journal is a real headache, and an editor needs to spend a good deal of his time persuading the publisher that he will get his money back in due course. I doubt whether these figures will give you much of a guide about the probable circulation of your own proposed journal, since the *Journal of Fluid Mechanics* is not only mathematical but is also directed to experimental workers and readers. Moreover we probably get many of our subscribers from the applied sciences such as aeronautics, hydraulics and meteorology.

Dissatisfied for one reason or another with Taylor & Francis, Batchelor used his growing influence and reputation to persuade Cambridge University Press (CUP) to take over from Taylor & Francis, who were no doubt happy to be rid of this troublesome and exacting upstart. CUP took on the *Journal* from Volume 3 in 1958, by which time a number of outstanding papers had appeared, and it was gaining a firmer foothold in the publishing marketplace. Among the authors of papers in the first two volumes, one may find, in addition to members of the Editorial team themselves, well-known names such as M.S. Longuet-Higgins, Donald Coles, John Laufer, W. Chester, G.B. Whitham, C.S. Yih, R. Betchov, W.V.R. Malkus, A.A. Townsend, H.P. Greenspan, R.W. Stewart, J.A. Shercliff, J.B. Keller, L.M. Milne-Thomson, B.R. Morton, J.R.A. Pearson, A.J. Favre, J. Proudman, L.S.G. Kovasznay, T.H. Ellison, D.W. Moore and R.S. Scorer; some of these attained great subsequent eminence. The *Journal* was clearly off to a very good start, and the progress did indeed accelerate when CUP took over the publishing risk and responsibility, a move

that was to pay great dividends in subsequent decades. (*JFM* has been one of the largest and most successful journals published by CUP for several decades past.)

6. Editorial process

There was no 'Board of Editors' for *JFM*. It was a matter of policy that each Editor and Associate Editor should act independently, receiving papers directly, choosing referees, corresponding directly with authors, and taking the ultimate decision on each paper to publish or not to publish. An author could of course appeal against a negative decision, but this was a rare occurrence, the reasons for rejection being usually unanswerable. Accepted papers often went through a lengthy revision process in response to referees' reports, two or three versions of the paper being submitted before ultimate approval. I recall one extended correspondence with an author from Argentina, which led to five re-submissions over a period of three years before the paper was finally accepted. This was of course in the days when all correspondence was by airmail (sometimes surface mail for bulky packages), with consequent extended delay; the fact that papers could as a result have a long gestation period led in most cases to beneficial streamlining of the final published product!

Despite Batchelor's firm rules of procedure, a degree of flexibility is apparent in some early correspondence. Here, for example, is Batchelor's reply (26 October 1960) to a well-argued plea from Hans Liepmann for accelerated publication of his paper:

> Dear Hans,
> In answer to your letter of 10 Oct., I think we should (as editors of *JFM*) always be willing to hustle a paper through a little quicker than usual when this is warranted by the scientific interest in it. From what I have heard from you and other people, I feel sure that your paper on flow through an orifice at lower densities is in this class. With any luck we shall have it ready for the printer during this week, and I would expect proofs to go out to you in December. The first available number in which it could appear will then be the February 1961 number, and I shall try to arrange for it to appear then.[3]

Later in the same letter:

> All the people whom Favre, Kovasznay and I decided to ask to act as section chairmen for the meeting at Marseilles have agreed to do so, I am glad to say, and we are now well prepared for a really good meeting. Les Kovasznay tells me that he has sent you the document that I prepared as a kind of guide to the type of meeting we have in mind and I hope you agree with the ideas I expressed in it. It is always a pity to spoil the summer by having to do some travelling, but I think we must make an exception for this meeting.

Here, 'this meeting' was the *Colloque International sur la Mécanique de la Turbulence*, Marseille, 1961, organised by Batchelor, Favre and Kovasznay, about which much has since been written (see, for example, Moffatt 2012).

Once papers were accepted for *JFM*, they passed to the Assistant Editors for copy-editing. I was myself drawn into this process in 1961 even before I had completed my PhD thesis; Batchelor plunged me in at the deep end, asking me to copy-edit the famous paper of Kolmogorov (1962), a translation from Russian of the paper that he had presented at the above Marseille turbulence colloquium; my task was to ensure that the

[3] It did appear in the promised number: Liepmann (1961), *Gaskinetics and gasdynamics of orifice flow.*

grammar of the paper conformed to standard English usage, that the editorial conventions of the *Journal* were respected, and that any obscurities of presentation were highlighted for clarification at the proof stage by the author – a daunting task! The work done by the Assistant Editors in these early years played an important part in establishing the uniformly high standards of the *Journal*.

But it was of course Batchelor who dictated these standards. He was himself meticulous with regard to every small detail. (For example, he would insist that the full-stop should precede the closing bracket in any 'full sentence' parenthesis such as this.) On the other hand, the closing bracket should precede the full-stop in parentheses forming part of a sentence (such as this). As part of the *Journal* style, Batchelor chose the Harvard referencing system, so that, for example, Kolmogorov (1962) is referred to in just this way, rather than by the distinctly uninformative alternative such as [6] – a wise decision, because a well-informed reader of the *Journal* will immediately recognise a reference such as, say, Taylor (1923), without the time-delaying inconvenience of having to turn to the reference list at the end of a paper to find out what an obscure coding such as [17] might refer to.

7. Book reviews

Book reviews were to play an important part in the *Journal* from the outset, and, as the following letter indicates, these were intended to be reviews of substance, giving the author of a review an opportunity to express his own philosophy; in effect, to write an extended essay on the subject of the book under review. Batchelor wrote to S.C. Corrsin, Johns Hopkins University, on 6 December 1956:

> Dear Stan,
> I am writing to ask if you would care to undertake a book review for the *Journal of Fluid Mechanics*. I have been trying in vain for some time to persuade either Sydney Goldstein or C.C. Lin to write a review of Surveys in Mechanics: the G.I. Taylor 70th Anniversary Volume. For various reasons (none of them very sound, if you ask me) they find themselves unable to do so. Would you care to have a go at it? . . .
> I do not think I have told you about the kind of review that we like to have for the journal, but you may have got some idea of this from the one or two that have already appeared. The general idea is that a book review for the Journal should make interesting reading, even for people who are not specially likely to buy the book concerned. We regard reviews as providing reviewers with opportunities to make interesting observations about the subject as a whole, other comparable books, the trend of recent work, and anything else that they believe to be worth writing about. Reviewers have as wide a scope as they like, both as regards content and as regards length; the only stipulation that I make is that it should not be too short (say, not less than about 1500 words) . . .

Corrsin's six-page review of this book, considerably longer than the minimum set by this stipulation, duly appeared in August of the following year (Corrsin 1957); he discussed nearly all the survey papers in the book, and it is interesting to note that his criticisms of the paper on 'Turbulent diffusion' by Batchelor and Townsend are particularly acute!

Batchelor himself wrote several book reviews in the early volumes, exploiting the 'wide scope' that he allowed himself. These still make absorbing reading! His review (Batchelor 1956) of *Momentum Transfer in Fluids* by Corcoran, Opfell and Sage is a classic example, expounding his view on how an author of a book on turbulent flow could most usefully

proceed, and how these authors conspicuously failed to do so. Batchelor writes of their attempt to cover turbulence:

> Such a venture would seem to demand courage, but in fact a reading of the book suggests that it is the courage of the blind who do not know what pitfalls lie about them. At a brisk pace they cover a wide range of topics of alarmingly different degrees of difficulty, and spend little or no time on discussion of the pros and cons of different ideas. When an hypothesis is introduced, the responsibility is pushed back on its originator; von Kármán said this, Prandtl said that, this is Schlichting's result, that is Pohlhausen's, and so on. This is the handbook manner, which is very useful in its place, but which leads here to a disturbing lack of discrimination in the selection and presentation of the available information about turbulence.

Such pointed criticism is rare in reviews of scientific books (although frequently encountered in the humanities), but Batchelor, rather than provide merely an anodyne commentary, was clearly trying to set an example of serious criticism, an example that subsequent reviewers would be encouraged to follow. With wry humour, he completes this review with the paragraph:

> It is of course always possible to learn something from a new book. I myself picked up one little wrinkle, which I have stored away mentally for use on occasions when I wish to impress students with my progressive outlook; it is that when one is establishing the equation of continuity, it is nowadays desirable to exclude the possibility that nuclear reactions are going on in the volume element under consideration.

Even more scathing was Batchelor's denunciation (Batchelor 1957) of *Viscous Flow Theory, II – Turbulent Flow* by Shih-I Pai, where he writes:

> The present book, like a good newspaper, can fairly be said to be comprehensive in scope, up to the minute, impartial, and discursive. It also has the defects of some journalistic reporting – a limited respect for the truth, haste in composition and printing, some irresponsibility, and an apparent reluctance to think about the material for longer than it takes to read it and write it down in different (not always!) words.

And after several pages of detailed criticism, we come to this remarkable closing paragraph:

> As a less sober postscript to this review, I, like the reviewer of Part I of the present work (*J. Fluid Mech.* **2**, 1957, 515), was struck by Professor Pai's diverting habit of incorporating slices of other people's papers in his book. This is perhaps carrying the reporting technique a little too far. I was able to recognize three whole paragraphs and some odd sentences from two of my own papers in the chapter on diffusion, and more of my own paragraphs in other chapters (with no hint that a direct quotation was being given and often without reference to the source). He even uses this strange technique on his own writing, and on page 146 reproduces what he has given on page 10. How many other writers are represented in this book? As a method of constructing a book it is so absurd that one cannot [but] be indignant at being copied in this way. Scissors and paste may be useful accessories at a lower level of writing, but at this level it is impossible to integrate existing pieces of prose into a continuous reasoned narrative. An author cannot avoid mastering a subject and rewriting it himself.

Wow! Comment would be superfluous.

8. Conclusion

As far as the history of fluid mechanics is concerned, the foundation of the *Journal of Fluid Mechanics* in 1956 must be recognised now, 60 years later, as a huge achievement for the promotion of research in our subject. The *Journal* has continued to grow in strength, in volume of publication (now arguably too voluminous!), and in influence, and is widely regarded as the premier journal for publication of papers in this broad field. The Editorial team has grown in proportion to the number of papers handled, but the declared purpose of the *Journal* is still to promote 'the publication of theoretical and experimental investigations of all aspects of the mechanics of fluids', and the principles developed by George Batchelor to achieve this purpose continue to govern editorial policy. For that, and for the very existence of the *Journal*, the worldwide fluid dynamics community has good reason to be profoundly grateful.

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