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Reflections of a new editor

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The success of the *Journal* since it began in 1956 is impressive; its founders would probably never have believed that some 6 metres of shelf would have been covered in 25 years in spite of adopting a sustained critical and selective approach to the acceptance of papers. Yet modified rapture may be in order. Has the *Journal* been going wholly in the right direction; are we adjusting to the changing nature of technology; do we know where we are going in the future?

At a time when economic progress is faltering in many countries and it is fashionable therefore to favour the producer of goods, I need make no apology for looking at the practice of fluid mechanics in general and this journal in particular mainly from the standpoint of engineering. Frankly the situation strikes me as rather disappointing. I can only speak about the position in the U.K., but I think that it would be generally true to say that here this journal is not the staple diet of most practising engineering designers concerned with problems which involve fluid mechanics. And the extent to which it *is* perused by academics in engineering departments might be construed as a measure of their remoteness from real engineering practice rather than of their enlightenment. Behind this is the fact that the *Journal* has found itself carrying many more papers in pure fluid mechanics or fluid mechanics applied to *natural* phenomena than papers of real interest to the engineering designer, the contriver of *artificial* phenomena. Why should this be?

Part of the explanation is the operation of a vicious circle. If in its early stages a journal appears to acquire a particular flavour and readership, good papers of a different kind will not be submitted and the pattern is confirmed and reinforced, whatever the founding editors may have wished. The scope for deliberately changing the trend by soliciting papers in other areas or by other means is very limited. Perhaps this birthday volume is one of our rare opportunities for influencing the course of the *Journal*'s development. I am assuming that it goes without saying that all devotees of fluid mechanics and readers of *JFM* would wish to see the subject blossoming in all possible ways and not least those which can lead to improved engineering practices that could yield benefits to society and wiser use of resources.

There is a genuine difficulty, however, which aggravates the gulf between the intellectual fluid dynamics world of *JFM* and the world of practical engineering problem-solving, that world where a reasonable answer *now* is vastly more valuable than a perfect answer someday. In the U.K. at least there is a strong tradition that the average professional engineer's education has need and room only for essentially one-dimensional fluid mechanics, based mainly on Bernoulli-cum-adjustments and simplified global momentum statements, and supplemented by qualitative notions (e.g. about separated flows) to aid mathematical modelling to the point where primitive fluid mechanics suffices. Nor is this to be scorned: academics usually tend to overrate the importance of sophisticated material for the real engineer on the job, even the research engineer. Moreover, I have myself frequently encountered advanced students in overseas institutions (known for their more academic and thorough mathematical and scientific training of engineers) who while having ϵ_{ijk} coming out of their ears were remarkably devoid of physical insights and ability to see the wood for the trees. It is easy to understand why the often very successful but relatively unsophisticated engineer finds the frequently mathematics-ridden pages of *JFM* not to his liking, and – more sadly – why the practical man with something interesting and novel in fluid mechanics to report, wrongly assumes that *JFM* is too rarefied a medium to publish it, but rightly assumes that *JFM* readership may not include the main audience that he wishes to reach. The same feelings are apparent to editors through the exchanges which we have with referees. How often have we had reports on papers which said something like: 'This paper is quite good stuff but is not up to *JFM* standards (of what? obscurity?) and so the author should send it to an engineering journal', seemingly using 'engineering' in a pejorative sense!

Referees and potential authors alike may feel the need for a clarification of the editorial position on papers on engineering fluid mechanics. I can only speak for myself, but essentially I think the position is that, of course, *JFM*'s normal high standards of scientific writing must apply (and the jargon-ridden, inept prose that disguises woolly thought in so much engineering reporting must be avoided) and that as well as a certain amount of novelty, of advancing the art, the paper should above all show physical insight. In the context of true engineering, i.e. design as distinct from engineering science, this ingredient is absolutely crucial because it is only through broad physical insights that creative manipulation of the physical world, the essential job of the engineering designer, becomes possible. There are regrettably few papers forthcoming in this category; nearly all of those with an engineering context are engineering science papers, in the sense that they apply the attitudes and methods of pure science to the phenomena which happen to arise in engineering systems. Among the reasons for this is the fact that genuine advances in engineering have commercial implications which inhibit open publication and render the practitioners too busy for scholarly pursuits. Another reason is that the topics may be extremely specialized and esoteric to the point where a broad-spectrum journal such as *JFM* would be unthinkable as a channel for communication. Moreover successful designs are often based on hunches, backed up by testing, and involve configurations that are too complicated for analysis of the 'respectable' kind which finds its way into scholarly publications.

The point that needs reiterating in relation to the acceptability of papers in *JFM* is that they need not involve sophisticated, or indeed any, mathematics. But a frankly empirical paper will get short shrift if it fails to compare its results with theoretical predictions which are already in the literature or shows no physical insight.

There are other reasons for wishing to promote the appearance of engineering fluid mechanics in leading journals and at conferences and symposia wherever fluid dynamicists congregate. I have in mind particularly the cross-fertilization which can occur from a mingling of practitioners of mathematical, natural and artificial fluid mechanics. Sometimes the cross-fertilization occurs inside one individual (Alfvén and G.I. Taylor are conspicuous examples). One hopes that sometimes the interaction takes the form of diverting the interests of excessively academic workers in the direction of more physically realistic or even commercially productive pursuits.

It is easier said than done to promote the flow of information from end to end of the

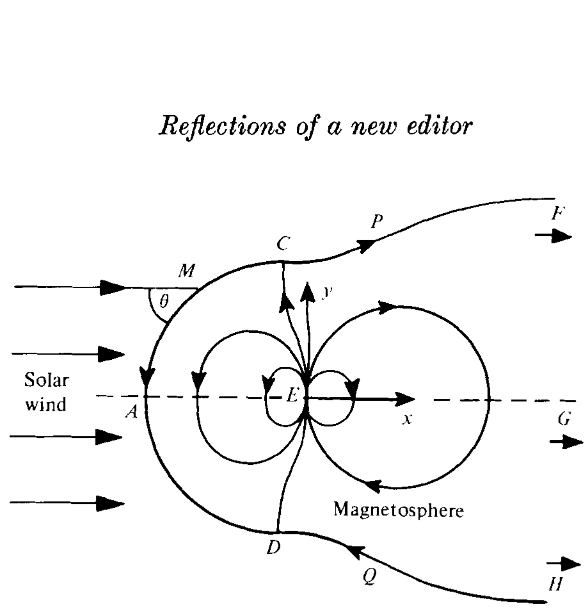

FIGURE 1. Dungey's two-dimensional model of solar wind and planetary magnetosphere.

fluid mechanics community so that advances in mathematical, experimental or design technique can be fully exploited wherever they are applicable. Thanks to John Dougherty, editor of our sister journal, *Journal of Plasma Physics*, I came across a good example of the problem a year or two ago. (Outlining it will also enable me to exercise my editor's privilege of infringing my own maxim that a *JFM* article need not contain mathematics!) It concerns two free-surface problems of apparently very different kinds, appearing in two areas of specialist endeavour between which the channels of communication are virtually non-existent, namely, astrophysical electrodynamics and groundwater engineering. The theory of two-dimensional steady groundwater flow with a free water-table surface had been fully developed on the basis of the complex-variable hodograph method by Joukowski and others in Russia, some 70 years ago. The essence of the method is to use a complex potential Ω equal to $\phi + i\psi$, where $\phi = -k(p/\rho g + y)$, in which k, p and ρ are permeability, pressure and density respectively and y is measured vertically upwards. Then if $z = x + iy$

$$w = \frac{dz}{d\Omega} = \frac{\partial x}{\partial \phi} + i\frac{\partial y}{\partial \phi}$$

along the free surface at which ψ and p are constant and therefore the imaginary part of w takes the constant value $-1/k$. Thus in the reciprocal hodograph or w-plane the *unknown* free surface maps into all or part of a *known* straight line. Each problem can then be solved in the w-plane so as to yield $w = f(\Omega)$ from which z can be recaptured by the integration

$$z = \int w \, d\Omega. \tag{1}$$

This version of the hodograph method seems to be little known even among fluid dynamicists generally, and so it is no surprise to find that it had not seeped through to astrophysicists by the time that Dungey (1961) published a solution of the two-dimensional version of the problem of the free surface between a solar wind and the magnetosphere of a planetary dipole E (see figure 1). The physical model which Dungey adopted was that the field-free solar wind undergoes essentially specular reflection at the unknown interface *FCADH* in the face of the magnetic pressure

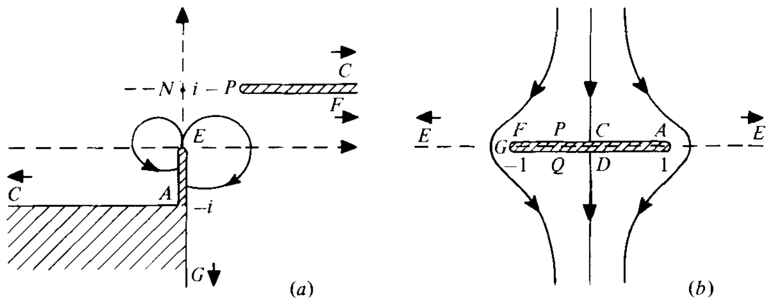

FIGURE 2. (a) w-plane, taking $2(k\mu_0)^{\frac{1}{2}}$ as unity. (b) ζ-plane. Stagnation point C bisects AF because of symmetry of Ω-field in ζ-plane. P is found to bisect CF in order that $AE = EN$ in w-plane.

$B^2/2\mu_0$ of the fluid-free magnetosphere. If the solar wind has incident momentum flux of uniform density k, then at M, where the slope is θ,

$$B^2/2\mu_0 = 2k\sin^2\theta \quad \text{or} \quad B/\sin\theta = \pm 2(k\mu_0)^{\frac{1}{2}}.$$

At the interface $B = (d\phi/dy)\sin\theta$, if $\phi + i\psi$ is now used to describe the magnetic field, and so $\partial y/\partial\phi = \pm\frac{1}{2}(k\mu_0)^{-\frac{1}{2}}$ (a constant) along the interface ψ = const., exactly as in the seepage problem, except that now there is a choice of sign. The plus sign applies along CF and DH, the minus sign along CAD. Dungey solved the problem apparently by inspiration, a real tour de force. Borrowing from seepage theory allows us to proceed by perspiration alone. Figure 2(a) shows the w-plane corresponding to the upper half of the z-plane. The unknown free surface maps into the straight ψ-const. lines AC and CPF, P being a point of maximum slope. ϕ takes a constant value, zero say, along AE and EG. A Schwarz–Christoffel mapping

$$\frac{dw}{d\zeta} = \frac{4}{\pi}\frac{(\zeta+\frac{1}{2})}{\zeta^2(1+\zeta)^{\frac{3}{2}}(1-\zeta)^{\frac{1}{2}}}$$

takes us to the ζ-plane (figure 2b) which maps the whole magnetic field and in which the Ω-solution is simple. The best formulation, avoiding multi-valued functions, relates Ω to ζ in terms of a parameter $r = p + iq$, where $\zeta = \operatorname{sech} r$, $\Omega = \tanh r$ and $0 \leqslant q \leqslant \pi$. The w–ζ mapping is

$$w = \frac{2}{\pi}\left\{\frac{\sinh r\cosh r}{1+\cosh r} + r\right\} - i,$$

from which integration according to (1) gives

$$z = \frac{2}{\pi}\left(\left(r - \frac{\pi i}{2}\right)\tanh r - \log(1+\cosh r) - 1\right),$$

which is equivalent to Dungey's solution, the z-origin being at E.

The point of the above passage in the context of an article mainly about engineering fluid mechanics is to highlight the difficulty of achieving the transfer of useful ideas across science. Supposing the groundwater engineering application had not been recognized previously, what chance would this first publication of the hodograph solution to an astrophysical problem in *JFM* (or perhaps an astrophysical periodical) have of being noticed by and influencing those in groundwater engineering? *JFM* carries so little in that field that few practitioners consult it and any that did could be forgiven for not recognizing something of potential interest to them among the unfamiliar physics and mathematics which I have just outlined. The fact that stately old analytical techniques like the complex hodograph method are less important in this computer-aided age does not detract from my general point.

A further moral is that the engineering and pure science camps should be less ready than they often are to deride the other's activities as being of no practical interest or of no intellectual content respectively.

The problem of ideas-transfer is compounded by the increasingly interdisciplinary nature of so much science and technology. Where among the plethora of journals should the engineer interested in a problem that involves not only fluid and solid mechanics but some or all of thermodynamics, chemistry, electrodynamics, electronic instrumentation, computers for calculation, control or data acquisition, and perhaps also economics or ergonomics turn to find the relevant literature or to publish his achievements? Potentially seminal analogies between problems in different disciplines (farther apart even than in the astrophysics/groundwater case just discussed) must often exist and persist unrecognized and unexploited. Perhaps leading journals should encourage the occasional appearance of articles of a highly interdisciplinary but not superficial nature, and not just every 25 years.

It is worth stopping to question the implicit assumption that engineering fluid mechanics is essentially the same subject as scientific fluid mechanics. Even if this has been true in the past, it may be becoming less true because a great deal of technology is changing very rapidly under the impact of cheap, distributed computing. Before we finally turn to this particular issue there is a prior question of longer standing to be considered. It arises because as I have already remarked true engineering is concerned with the theoretical activity of design, not the scientific investigation of events. In essence the latter consists of identifying a specific phenomenon and then following its evolution through the chain of cause and effect either analytically or empirically in a computerized or real experiment. Because of the docility of the macroscopic physical world (or the Second Law of Thermodynamics) a more or less unique outcome generally ensues. Design is by contrast an inside-out process. One starts with a desired outcome (or a choice of alternatives to be optimized according to some criterion, usually an economic rather than physical one) and one has to devise a configuration which would give rise to this outcome. Because cause-and-effect are not being pursued in forward time, there is no guarantee that a unique solution to the problem exists. No initial configuration at all may exist having the desired outcome, or instead the problem may be under-specified and a multiplicity of solutions may exist. And because the physical relationships are not being used in a cause-and-effect way, the calculation is liable to be ill-posed or even unstable. This idea is probably best conveyed through a simple but striking example which conveniently uses the notions of seepage flow that we have already encountered.

Consider the two-dimensional problem of designing a device for generating a slow rectilinear stream in a duct with non-uniform velocity, using a shaped porous bed instead of the customary gauzes (see figure 3). The upstream and downstream inviscid flows appear to the bed as regions of uniform pressure. The seepage flow through the bed is governed by the two-dimensional elliptical Laplace equation but two boundary conditions are specified at the straight, transverse downstream edge BC, where the

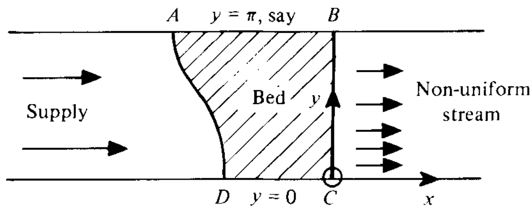

FIGURE 3. Porous bed producing non-uniform duct flow.

velocity potential ϕ is constant and its normal gradient (the desired velocity profile) is known. The problem is to find a suitable shape for the upstream edge AD, another equipotential. Here we have an improperly formulated mathematical problem, and mathematical (Hadamard) instability ensues. If we take the axes shown in figure 3, with the complex potential Ω also zero at C, and express the velocity desired along BC as a relation between ψ and y, Fourier-analysed into the form

$$y = \psi + \Sigma a_n \sin n\psi \quad \text{at} \quad \phi = 0,$$

in which the last term represents the departure from uniform velocity, then the solution satisfying the boundary conditions along AB and CD is

$$x + iy = \Omega + \Sigma a_n \sinh n(\phi + i\psi).$$

From this the form of AD emerges once a suitable constant, negative value for ϕ there, $-k$ say, is chosen. Unfortunately on AD the series for x and y in terms of ψ diverge unless $a_n \to 0$ faster than e^{-nk} as $n \to \infty$. This is not the case in general. Consider for instance the mildly non-uniform outlet stream described by

$$y = \psi + \epsilon\psi(\pi - \psi)$$

for which $a_n \to 0$ only like n^{-3}. Similar unstable design problems arise in fusion technology, where a cross-section for the confined plasma and the peripheral variation of magnetic field is chosen and the surrounding vacuum field is inferred by extrapolation away from the surface to whatever external conductors are necessary.

It offends one's intuition to learn that arbitrary velocity profiles cannot be produced by the method of figure 3, even when the departure from uniformity is weak.

The general questions as to why and when the engineering design process, especially in geometrical fields involving fluid mechanics, is sometimes unstable or ill-conditioned seems to be one worthy of serious attention and so far hardly explored. If the design process for fluid engineering systems is indeed commonly unstable one wonders why one has heard so little about the problem in the past and how, maybe instinctively or unconsciously, the engineering designer adopts procedures which sidestep the difficulty in some way.

I believe the advent of the computer provides an added reason for taking the problem seriously. I am not thinking merely of the fact that numerical methods of great sophistication are now coming into routine use by the engineer, who as a result is becoming more and more of a theoretician-of-sorts and less of an empiricist, whether he is analysing phenomena within given constraints, or designing constraints to achieve desired outcomes. (It seems self-evident that the more complicated and the less physically-based the calculation, the greater the scope for pathological behaviour).

Instead I have in mind the *active* or *interventionist* posture that the engineer is increasingly adopting in the face of cheap micro-computing. In the past engineering has largely dealt with systems composed of *passive* elements, elements with fixed characteristics or transfer functions. Control intervention to tune the system was on a limited scale, if it existed at all. But progressively, beginning in the electronics industry, system components have been made self-adaptive, adjusting their own characteristics in response to measurements which the system makes on itself according to algorithms often of considerable sophistication. The art of understanding multivariable feedback systems and of designing hardware and software for instrumentation, the processing and blending of signals and subsequent physical action has been greatly advanced by the development of cheap, small computing elements. It becomes possible to envisage much more intervention in the operation of physical systems than ever before.

This is not a new trick; biological systems have always performed it. The alimentary and cardiovascular systems are self-adaptive and full of feedback mechanisms. There are already signs of the revolution affecting man-made fluid systems. Some of the schemes for the extraction of ocean-wave power by means of articulated floating objects include self-adaptive elements, adjustable to the needs of the moment. Gust alleviation on aircraft is an older, simpler version of a similar problem. Anti-sound is already being exploited. Can one look ahead to the wider adoption of self-optimising fluid engineering systems in which large numbers of parameters defining the constraints on the flows are adjusted in response to transducer signals interpreted and combined by micro-processors following set or even self-adaptive (learning) algorithms? This certainly would give a wide new dimension to fluid engineering and allow artificial fluid mechanics to become considerably more challenging than most natural fluid mechanics outside the biological world. This is not to exclude attempts at intervention in the world of natural fluid mechanics, such as the manipulation of rainfall or hurricanes.

Active intervention in the constraints of a fluid motion most obviously means adjustment of the boundaries, their positions and motions. It is not clear whether the scope for such intervention using many degrees of freedom will be greatly limited by such considerations as cost, reliability or power consumption in practical situations. Can one imagine the old idea of the compliant surface for modifying boundary layer stability being extended from the passive case (a surface with set elastic and damping characteristics) to the active case, where the surface is more or less continuously adjustable in response to many diagnostic measurements? The control theory fraternity has been busy for some years in the study of continuous systems but the fluid mechanics world may have to take more part in the action in future.

Adjustment of the solid surfaces is surely not the only possibility, however, and it is probably time that more ingenuity was being applied to the search for alternatives. Only in certain specialist fields such as magnetohydrodynamics can we escape from the frustrating position of being able only to push the fluid at its edges and instead grab and manipulate it in midstream. Even then the sources of the long-range interventions have to be placed outside the stream and precise action focussed on particular localities is not in general possible, which fact makes the whole control problem much more difficult. In activities such as magnetic separation can we think of ways of bringing the Maxwell demon nearer to realization? Introduction of additives such as long-chain polymers or suction and injection at porous surfaces constitute other established forms of intervention which could conceivably be made self-adaptive. The manipulation of groundwater or oil reserves by suction or injection at a series of wells is probably the best existing example of interventionist fluid engineering.

It seems to me that there is great scope for research into the several questions that are raised: where in engineering fluid mechanics are the best opportunities for achieving improvements in performance (according to whatever criterion) by the self-adaptive system approach; and can it be justified in economic terms in view of the probable increased complication and dangers of unreliability; what are the best methods of making measurements on the flow upon which control action can be based; what ways of actively altering the constraints on fluid systems are practical in real engineering terms; what algorithms are best for relating the 'diagnosis' to the 'treatment' in each application and can they be made self-improving? Such questions are now beginning to be asked throughout modern technology and fluid engineering must surely be no exception.

I have left to the last the question which I find most intriguing and which links my last theme to the previous one, concerning the potential instability of the engineering design process. Active intervention by feedback control is in essence an automated, continuous design process. The controller is trying to redesign the system constraints to achieve a desired outcome. In a complicated fluid system it may not be obvious that a set of constraints which would produce the desired outcome exists at all, or is unique, and in any case the natural tendencies of fluid motions under even passive constraints to be unstable or undergo bifurcations may easily be exaggerated by having unfortunately chosen active constraints.

If we take our primitive earlier example of the porous bed intended to generate a desired non-uniform velocity profile, one can imagine using a system which progressively adds or removes porous material along AD according to preset rules in response to measurements of the actual achieved velocities out of BC. It is evident that such a strategy is doomed to failure, in general.

My final conclusion then is that in the age of the microprocessor, fluid mechanics like nearly every other activity of civilized man will never be quite the same again. Engineering, or interventionist, fluid mechanics seems to be offering endless scope for rewarding investigation, with rewards which one hopes will go beyond the intellectual satisfaction of the practitioner to the prosperity of industrial enterprises and to benefits to the community at large. I should like to look forward to seeing a fair share of the best work in this challenging field finding its way into the pages of the *Journal of Fluid Mechanics* during its second 25 years.

REFERENCE

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