LETTERS TO THE EDITOR

To the Editor:

Recently Papoutsakis, Ramkrishna and Lim have published two articles ([1],[2]) in which they use a complicated Hilbert space approach to solve the "Extended Graetz Problem". While I appreciate their use of modern analysis in applied mathematics, it is unfortunate that the examples they have used to illustrate their technique, not only can be, but have already been solved more concisely using a simple-minded Laplace Transform technique ([3],[4],[5],[6],[7]) as follows.

Starting with equations (5)–(8) in [2], and setting

$$\bar{\theta} = \int_{-\infty}^{\infty} e^{-t\zeta}\theta(\zeta,\eta)d\zeta,$$

we obtain the equation

$$\frac{1}{\eta}\frac{\partial}{\partial\eta}\left(\eta\frac{\partial\bar{\theta}}{\partial\eta}\right) + \left(\frac{t^2}{P_e'^2} - tD(\eta)\right)\bar{\theta} = 0 \qquad \text{(I)}$$

with boundary conditions

$$\frac{\partial\bar{\theta}}{\partial\eta}(t,0) = 0$$

$$\frac{\partial\bar{\theta}}{\partial\eta}(t,1) = \frac{1}{t}(1 - e^{-tz}).$$

Denoting by $f(t,\eta)$ the solution of I for which $f(t,0) = 1$, we obtain instantly

$$\bar{\theta} = \frac{f(t,\eta)}{\dfrac{\partial f}{\partial\eta}(t,1)}\cdot\frac{1}{t}(1 - e^{-tz}),$$

and by the inversion formula

$$\theta(\delta,\eta) = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty}\frac{e^{t\zeta}}{t} \times \frac{f(t,\eta)}{\dfrac{\partial f}{\partial\eta}(t,1)}(1 - e^{-tz})dt$$

$$= F(\zeta,\eta) - F(\zeta - z,\eta).$$

Using the Residue Theorem we obtain

$$F(\zeta,\eta) = 4\zeta + (\eta^2 - \eta^4/4) - \frac{7}{24} + \frac{8}{P\delta^2} + \sum_{1}^{\infty}\frac{e^{\alpha_n\zeta}f(\alpha_n,\eta)}{\alpha_n\dfrac{\partial^2 f}{\partial t\partial\eta}(\alpha_n,1)}, \quad \zeta > 0,$$

$$= -\sum_{0}^{\infty}\frac{e^{\beta_n\zeta}}{\beta_n\dfrac{\partial^2 f}{\partial t\partial\eta}(\beta_n,1)} \times f(\beta_n,\eta), \quad \zeta < 0; \qquad \text{(II)}$$

where $\beta_n(\alpha_n)$ are the positive (negative) roots of $\partial f/\partial\eta(t,1) = 0$ and the algebraic term in the expansion for $\zeta > 0$ is the residue at the second order pole at $t = 0$.

This solution (II) is identical to that appearing as equations (53), (54) in [2], but does not involve the summing of assorted infinite series to obtain the coefficients. Indeed the most complicated step in the solution is the expansion of f in powers of t up to t^2 in order to calculate the residue at the origin.

Furthermore this method makes no assumptions about the reality of the eigenvalues (although they are obviously real from physical considerations). Taken in conjunction with the authors' remark that "the existence of both positive and negative eigenvalues was not commonly recognized" I am left with the overwhelming conclusion that the authors have not carried out a proper review of the subject before embarking on their work.

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LITERATURE CITED

[1] Papoutsakis E., D. Ramkrishna and H. C. Lim, "The Extended Graetz Problem with Dirichlet Wall Boundary Conditions", *Appl. Sci. Res.*, **36**, 13 (1980).

[2] Papoutsakis E., D. Ramkrishna and H. C. Lim, "The Extended Graetz Problem with Prescribed Wall Flux". *AIChE Journal*, **26**, 5 (1980).

[3] Jones A. S., "Extensions to the solution of the Graetz Problem", *Int. J. Heat Mass Transfer*, **14**, (1971).

[4] Jones A. S., "Laminar forced convection at low Péclet number". *Bull. Austral. Math. Soc.*, **6**, 1 (1972).

[5] Jones A. S., "Laminar faced convection at low Péclet number, II", *Bull. Austral. Math. Soc.*, **6**, 1 (1972).

[6] Jones A. S., "Two-dimensional adiabatic forced convection at low Péclet number", *Appl. Sci. Res.*, **25**, (1972).

[7] Jones A. S., "Heat Transfer in Poiseuille Flow", Ph.D. dissertation, Queensland University, Brisbane, Aust. (1972).

Reply:

We have read the letter from Dr. A. S. Jones with considerable interest and his papers on the Graetz problem at low Peclet numbers cited in his letter. We consider the lack of references to his work in our papers a serious omission and since we weren't aware of it concur with his judgement that we have "not carried out a proper review of the subject". Indeed his solutions via the double sided Laplace transform are identical to those obtained by us in references [1] and [2] of his letter. While the omission of his work is indefensible, it seems most relevant to us to address some important issues here.

First, we are not the least surprised that such a solution was possible by a double sided Laplace transform. The self-adjoint formalism in our paper has its roots in the paper of Ramkrishna and Amundson [1], in which the authors point out at the very end of the article that" . . . the infinite domain problems could also have been solved by the method of Fourier transforms." Indeed the extended Graetz problem too can be solved by Fourier transform and the inversion would lead to a complex integration identical to that encountered by Jones.

The merits of a self-adjoint formalism lie in the fact that the entire analysis is predicated on the powerful properties of the self-adjoint operator, that are amenable to treatment separately from the boundary value problem. Inhomogeneities in the differential equation and the boundary conditions are then accounted for in a perfectly natural manner, thanks to the expansion theorem originating from the eigenvectors which form an orthonormal basis in the space. That the eigenvalues are real too is a direct consequence of self-adjointness. Indeed Jones has recognized the existence of both negative and positive eigenvalues but it is not clear to us how his method "makes no assumptions about the reality of eigenvalues." Physical considerations could of course apply to the boundary value (physical) problem but are less clearly related to the eigenvalue problem. The basis for the "physical argument" for reality of eigenvalues which Jones implies is undoubtedly that spatial oscillations cannot be expected in a uniform heating (or cooling) device. But this argument *presumes* that the solution *can* be expressed in the form in which it is finally obtained. Of course our claim is not that the foregoing results cannot be established rigorously in the inversion procedure but simply that our self-adjoint formalism, once it is recognized, does all of that in a straightforward manner. It is unfortunate that Jones regards the Hilbert space approach as "complicated" because it is no more complicated than the "simple-minded Laplace transform technique". In a deep sense, the two methods are identical because the spectral representation of an operator or functions of it is a fantastic generalization of Cauchy's (contour) integral formula for the function of a complex variable. Self-adjoint operator representations in terms of orthogonal projections are the *net* result of carrying out such a contour integration!

Having said all this, it is not our objective to claim that rigorous proofs must accompany the engineering applications of mathematics, and we are not complaining about the excellent work of Jones that outshines all others that we have read on the subject. But we dispute his conclusion that what we have accomplished is to arrive at his solution through a complicated route. We have since extended our analysis to considerably more difficult situations [1] than the Graetz problems. Here too Fourier or Laplace transforms are applicable but we submit that our method is most conveniently applied.

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LITERATURE CITED

[1] Papoutsakis, E. and D. Ramkrishna, "Conjugated Graetz Problems J. General Formalism and a Class of Solid-Fluid Problems," *Chem. Eng. Sci.* (in press).

To the Editor:

The number of manuscripts submitted to scientific journals continues to rise. In many fields, instances of fragmentation and duplication are becoming numerous. In combating this practice and in assuring quality in general, the only course open to technical journals is reliance on the judgement of peer reviewers. Every attempt must thus be made to make peer review as effective as possible. Effective in this context means expeditious recognition and improvement of the valuable contributions and rejection of the others.

The most valuable reviews are nearly always signed, or otherwise identifiable. Yet most journals permit reviewer anonymity. Why does this practice continue? Promoters point to the need for complete candor from the reviewers. This candor would be impeded if there was concern for the relationships with ones colleagues. Further, there is the danger of depleting the pool of reviewers who would be willing to participate without the veil of anonymity.

On the other hand, we recognize that many papers receive only cursory review. Would improvement result if the reviewer had some responsibility for the result? A review which requires a signature demands a more thorough job. In recognition for this effort the reviewers could be listed at the end of each paper. Credit for those articles found to be noteworthy contributions would be weighed against the shared blame for those which are not.

The final point is that reviews which are anything but careful and thoughtful are of little use to the editor. The loss of these cursory reviews may actually be beneficial. It seems time to give the ideal of reviewer recognition a try.

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