Answer to Pomraning's Rebuttal on Variational Boundary Conditions

Pomraning justifies his steps mainly on the grounds that other ones are always "strongly affected" by "completely arbitrary" quantities anyway. Why his whole variational principle is not therefore also completely worthless now becomes the real question!

The exact boundary conditions to the exact solution can indeed be extracted from Pomraning's new generalized functional. However, when one incorporates any approximations into the trial functions, he should also deliberately restrict α and β to expressions (like zero) that make the extra integrals in the functional negligible, as would the exact solution. Equivalently, fixed expressions for α and β influence the size of the second order errors that a trial function is assumed capable of rendering negligible; as always, that assumption must be based on outside information. By making the extra terms in the functional small, the trial function will end up depending only weakly on α and β, as good forms obviously must.

Pomraning's main reaction to my competing procedure is that its results are of a "completely different nature." He categorizes his method as an "approximate theory", and mine as concerned with the solution. He senses some differences in how they relate arbitrary constants with boundary conditions and when they make recourse to the variational principle, but the essential point remains that each procedure produces the same differential equations and selects a particular solution. For diffusion theory, my method requires any two linearly independent solutions, but only at the boundaries. It is not a great burden to obtain such quantities even in numerical work. The use of other adjoints is a much more important practical problem.

My comments on the six points will now emphasize the new, rather than the original, issues that they raised:

1. Here is the subtle mathematical point that is at the heart of the issue! The variations of the flux components at the surface do not need to be restricted linearly as a result of any desired linear relations between the flux components themselves. As a counterexample, it was already shown that holding $J(a)$ stationary and letting Γ vary produced the relation that $\Gamma = \frac{1}{2}$, a linear restriction on the flux components in the P_1 approximation. Even holding the highest order component stationary seems as 'natural' as the PC linear restrictions, and no extraneous roots occur.

Now say that the coefficients of the linear restrictions on the flux components have been established at each boundary. The P_N differential equations then determine the surface flux components themselves. Therefore, if slightly different boundary values satisfy the same linear restrictions, they can not be boundary values of solutions to the differential equations.

One can demand stationarity with respect to such nonsolution variations, of course, but there is no reason to have restricted the boundary terms to the linear form desired for solutions to the differential equations. In fact, these variations are of the type that I suggested be avoided, e.g. the type that led to a P_1 approximation in the P_2 equation instead of in the P_1 equation.

2. The number of multiple roots in the PC method rises rapidly with the order of the approximation. A P_3 approximation gives eight possible values to the extrapolation length. Six are negative, but two are positive[1]. It is hard to understand how "physical considerations" could be used to reject one value of about 0.5 and accept another of about 0.7.

3. Immediately after answer 6, Pomraning explains why it is more accurate to do the opposite of what he advocates here.

4. One of many possible procedures that would accomplish all the goals that Pomraning doubts are feasible would be to impose vanishing of all the usual Marshak surface integrals except the last. In its place, use the relationship that the asymptotic flux of the non-absorbing Milne problem should be exact.

5. The fact that diffusion theory can describe the spatial dependence of only the asymptotic part of the Milne problem flux was lost to the variational principle when a P_1 approximation was made. The coefficient of the asymptotic flux depends on results of a P_1 approximation at the surface, and the astounding accuracy of the PC extrapolation lengths for certain situations does more to raise suspicions than to demonstrate superiority.

6. At least we agree on this conclusion!

Pomraning rejects the failure of diffusion theory as an explanation of the infinity in the thin slab flux, but no other reason is possible. Since my suggested boundary conditions depend on diffusion theory, they should be better than the PC values when it is valid, but they may be much worse when none could provide a good description.

Harvey Amster

Department of Nuclear Engineering
University of California
Berkeley, California

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