Lambropolous and Luco contribution was merely to show that the volume term of the functional, which I wrote as a sum of integrals over disjoint regions R_k, could be written as an integral over the volume of the reactor if the integral is interpreted as a Riemann integral. For then the discontinuities do not contribute to the integral.

Let me now pass on to consider briefly the alternative method of removing the overdetermination problem suggested by Stacey. This involves the use of arbitrary diagonal matrices $\omega_{n'}(x,y)$ as weight functions on the $\delta\rho^*_{n+} = \delta\rho^*_{n-}$ condition (discussed on p. 456 ff of Stacey's article). In the case where the same number of flux and current expansion modes are used in each axial zone this method seems workable enough. However, it suffers from the defect that one does not have any guidelines on how to best choose these weight functions $\omega_{n'}(x,y)$. In the Lagrange multiplier method one knows that one should choose the modes in which β and α are expanded in such a way that they approximate the component normal to the interface of the current and adjoint current. If one is interested in obtaining accurate values of the functional, or in an eigenvalue problem an accurate value of the eigenvalue, this seems to be an important consideration. The remarks Kaplan[5] makes seem pertinent here.

There is, I believe, a more serious objection to Stacey's method when different number of flux and current expansion functions are used in adjacent axial regions. He then states that the same number N^* of adjoint flux and current expansion functions must be used in both regions, and that this number must equal the number of direct expansion functions in one of the regions. If, for concreteness, we assume $N^* = N_+$, then $N^* \neq N_-$. But the number of first order differential equations in each zone is, in a one group problem, just given by $4N^*$. These equations are Eqs. (11) through (14) of Stacey's article. [Actually, Eqs. (11) and (14) are differential equations; Eqs. (12) and (13) are algebraic equations.] The index n' in these equations runs over the adjoint functions, and hence it is easy to verify that there are $4N^*$ of these equations. The unknown flux and current variables in these equations are $4N$ in number. This means that one has $4N^*$ equations in the $4N$ flux and current variables in the zone on the minus side of the interface. If $N^* < N_-$, then one has more unknown functions in this zone than one has equations, and the solution of the problem is underdetermined. If $N^* > N_-$, an overdetermination problem occurs with more equations than unknown functions of z, and there is no solution possible.

A. J. Buslik

Bettis Atomic Power Laboratory
P. O. Box 79
West Mifflin, Pennsylvania 15122

May 30, 1972

[5]S. KAPLAN, *Trans. Am. Nucl. Soc.*, **6**, 3 (1963).

Reply to "Comments on Variation Flux Synthesis Methods for Multigroup Diffusion Theory"

Buslik's comments[1] reflect more a difference in viewpoint than any difference in substance, and to some extent indicate a misinterpretation of my paper.[2] My basic position is that the Lagrange multipliers can equally well be written as currents evaluated at the interface [i.e., Eqs. (5a) or (5b) and (6a) or (6b) of my paper], in which case the Lagrange multiplier functional is identical to the more transparent functional represented by F_2, providing $\gamma, \Omega = 0, 1$ in the latter, in that either functional contains some one scalar function weighting the difference in flux at the interface. Just as the Lagrange multiplier function α may be expanded in trial functions which are independent of the flux trial functions in the volumes, so may the terms $D_+\nabla\phi^*_+$ be expanded (i.e., there is no constraint that the expansion functions for interface terms be related to expansion functions for volume terms in the synthesis application). His remarks indicate that Buslik has incorrectly read such a constraint into my paper.

Using the Lagrange multipliers, one comes upon the overdetermination problem in a different guise. There is no formal overdetermination in the derivation. However, when one looks to the Euler equations for guidance in selecting expansion functions, one is confronted with both Eq. (5a) and (5b) of my paper, and one must choose either one, some combination of the two, or neither. Using the functional F_2 one comes formally upon the overdetermination problem if the functional is required to be stationary for arbitrary independent variations of, for example, $D_+\nabla\phi_+$ and $D_-\nabla\phi_-$. Setting $\gamma = 0, 1$ formally eliminates the overdetermination, but leaves one with the choice $\gamma = 0$ or $\gamma = 1$. This choice influences the guidance in selecting expansion functions. Thus, one is faced with basically the same problem in both cases, and the same range of options is open for its resolution in each instance.

While I believe that most of Buslik's comments follow from the incorrect constraint assumption mentioned above, there are three points which I should like to respond to directly.

His Lagrange multipliers are scalars in his surface integrals [Eq. (3) of his letter], and he subsequently shows in his Eq. (6) that these scalars are identifiable with the normal components of the current at the surface. He commented that I incorrectly wrote the Lagrange multipliers as vectors. I wrote the Lagrange multipliers as the scalar product of the unit vector normal to the surface $\hat{n}$ and vector quantities $\boldsymbol{\alpha}$ and $\boldsymbol{\beta}$. This scalar product is used in my Eq. (14), which is identical to his Eq. (3); i.e., his α is my $\hat{n} \cdot \boldsymbol{\alpha}$, etc. Thus, his comment, appearing just after Eq. (3) in his letter, is incorrect.

Buslik also objects to the alternative method of removing the interface overdetermination which I suggested. He raises two points. First, he states that my method suffers from the fact that the weight functions $\omega_n(x,y)$ are arbitrary, which they are. At this point he argues, "In the Lagrange multiplier method one knows that one should choose the modes in which β and α are expanded in such a way that they approximate the component normal to the interface of the current and adjoint current. If one is interested in obtaining accurate values of the functional, or in an eigenvalue problem an accurate value of the eigenvalue, this seems to be an important consideration." Earlier in his letter, in striving to make another point, he seems to argue the opposite: "Stacey states that in synthesis applications of the Lagrange multiplier principle the interface overdetermination problem is avoided by choosing one of Eqs. (6) (of his Letter) to guide the selection of trial functions for β. This is quite simply false." He cannot have it both ways. Relative to his comment on the arbitrariness of my ω_n, I would like to point out that all weighting and expansion functions are arbitrary, in all synthesis

[1]A. BUSLIK, *Nucl. Sci. Eng.*, **49**, 112 (1972).
[2]W. M. STACEY, Jr., *Nucl. Sci. Eng.*, **47**, 449 (1972).

applications, and we are guided in our selection by physical insight as much as, if not more than, by the mathematical derivation.

Buslik's final point, objecting to my interface treatment when a different number of expansion functions are used in different regions, results from his reading something into my paper that is not there. The requirement that the number of adjoint expansion functions be the same on both sides of the interface is with respect to the interface terms, as discussed after Eqs. (34) in my paper. The conclusion that one must then use more or less adjoint expansion functions than direct expansion functions in deriving my Eqs. (11) through (14), and thereby end up with more or less equations than unknowns, is Buslik's, not mine.

W. M. Stacey, Jr.

Argonne National Laboratory
9700 S. Cass Avenue
Argonne, Illinois 60439

June 12, 1972