Letters to the Editor

Comment on "Response Matrix Properties and Convergence Implications for an Interface-Current Nodal Formulation"

Yang has investigated the properties of the response matrices in a given group.[1] He makes a false statement when expressing the opinion ". . . no analytic study has in the past been performed on the properties of these response matrices derived by nodal diffusion theory." That statement would annul works by Henry at the Massachusetts Institute of Technology and his former students (among others, Smith and Shober), Bonalumi (Ontario Hydro), and many others including me. All those works have been published in leading American journals. I do not intend to deprive Yang from the stimulating search of the literature; thus, this letter is confined to a short list of bare facts.

1. Analytical response matrices are explicitly known in multigroup formalism as well. The row has been opened by Henry and Shober by presenting explicit two-group matrices for square nodes.

2. Analytical solutions to the multigroup diffusion equation appeared in 1984, including hexagonal geometry.

3. As to hexagonal geometry, the first analytical response matrix appeared in 1981 in an American journal following a local report in 1980.

4. Properties of the response matrices have been studied (eigenvectors, eigenvalues).

5. The effect of involving higher moments of the entering current has also been studied and revealed nonnegligible effects.

6. A formulation of an analytical three-dimensional response matrix for hexagonal node appeared first in Ref. 2, which mentioned test results of a production code. There is nothing essentially new in the three- compared with the two-dimensional case.

7. Y. Gotoh determined response matrices with regionwise different, i.e., space-dependent cross sections.

8. As far as I know, today there are almost a dozen production codes based on analytical solutions in a hexagonal three-dimensional node. Several of them use the response matrix method.

It should be emphasized that there are many possible choices of the vectors representing the partial currents given at the faces of the node; notwithstanding, the vectors in Table I of Ref. 1 are not lucky. The vector (1,1,1,1,1,0,0) means zero partial currents at the top and the bottom. Such entering currents can be given, but the exiting currents will follow the pattern (a,a,a,a,a,a,b,b), whereas, if the entering current follows the foregoing vector, so does the exiting current with different a and b ($a \neq 0$, $b \neq 0$) values.

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On "Neutron Fluence at the Pressure Vessel of a Pressurized Water Reactor Determined by the MCNP Code"

Reference 1 is of interest to me because it addresses one of the major factors directly affecting reactor operation and lifetime. During the past 7 yr, my students and I have been investigating transport theory methodologies for accurate estimation of neutron fluence at the reactor pressure vessel. While reading Ref. 1 very carefully and with much interest, I became concerned by its lack of quality and accuracy—especially because the results of the paper may potentially have an impact on the safe operation of commercial reactors. Therefore, I feel compelled to comment on a few major issues, including accuracy of results, Monte Carlo modeling, and inaccurate and/or superfluous statements.

ACCURACY OF RESULTS

In Ref. 1, p. 443, Laky and Tsoulfunidis's (L&T's) statement ". . . the MCNP results clearly predict a lower integral flux

for this cycle [than DOT results]" is not accurate. It is likely that this conclusion is the result of inconsistent/incorrect MCNP and/or DOT modeling. However, details about the deterministic (DOT) modeling are neither given nor referenced. The L&T results are in direct contradiction to our results, which have been published and presented at different conferences during the past 2.5 yr, and are listed in Refs. 2 through 7.

In Ref. 2 (published/presented ~1 yr before the submittal of Ref. 1), we presented a preliminary comparison of Monte Carlo and deterministic results for the pressure vessel fluence/cavity dosimetry calculation. In that paper, we observed results similar to those reported by L&T, but we stated the preliminary nature of the results and in fact our subsequent studies demonstrated that we had used an erroneous normalization factor in the Monte Carlo calculations. Using the correct factor, however, resulted in the opposite behavior; i.e., the Monte Carlo method showed higher fluxes than the deterministic method. The correct results were presented at the American Nuclear Society annual meeting (June 1993) and were later reported in Ref. 3 (1 month before Ref. 1 was submitted).

We continued this investigation trying to understand the cause(s) of the observed differences. We identified several major factors that contribute to the observed discrepancies; these include

1. multigroup cross sections (deterministic) versus continuous-energy cross sections (Monte Carlo)
2. three-dimensional synthesis/geometric approximations (deterministic) versus three-dimensional explicit modeling (Monte Carlo)
3. scattering anisotropic order (deterministic) versus continuous differential scattering (Monte Carlo).

In Ref. 3, we addressed the first issue, and indeed, we demonstrated that a significant portion of the difference can be attributed to the use of multigroup cross sections. In other words, we demonstrated that if one uses the same cross sections for both the Monte Carlo and deterministic calculations, the differences are <10%. Further, in Ref. 4 (1 month after Ref. 1 was submitted), we reported a more thorough analysis comparing the effect of using different available multigroup cross-section libraries to continuous-energy cross sections. The use of the multigroup libraries consistently resulted in lower calculated parameters.

In Ref. 3, we also investigated the effect of using different response cross sections and demonstrated the existence of large discrepancies between response cross sections from different libraries and the necessity of using consistent dosimetry cross sections when comparing Monte Carlo and deterministic results. A more thorough discussion of the foregoing analysis using different cross-section libraries, both multigroup and continuous energy, was presented in Ref. 5 (7 months after the submittal date of Ref. 1), Ref. 6 (2 months after the acceptance date of Ref. 1), and Ref. 7. In all of these papers, we demonstrated that the synthesis procedure does not introduce a notable inaccuracy in results for this problem (cavity dosimeters are placed close to the core midplane). Rather, the axial, radial, and azimuthal flux distributions from both Monte Carlo and deterministic calculations showed good agreement.

In Ref. 5, we also addressed the last issue, i.e., the effect of scattering anisotropic order, and demonstrated that the effect of anisotropy is small (<4%). Further, in April 1995, we presented a benchmark paper.[6] In this paper, we carefully analyzed aforementioned issues and clearly demonstrated the following:

1. Continuous-energy Monte Carlo predicts values that are higher (<30%) than the multigroup deterministic predictions.
2. Synthesis procedure has negligible affects.
3. The use of higher order scattering anisotropy may account for 3 to 4%.
4. The use of multigroup cross sections accounts for a difference of 10 to 20%. If both deterministic and Monte Carlo calculations use the same multigroup libraries, results (flux and reaction rates) are expected to be within 10%. Therefore, we conclude that the deterministic method is adequate for this type of simulation.

Finally, Ref. 7 is a comprehensive paper related to the foregoing investigations.

MONTE CARLO MODELING

Source

In Ref. 1, p. 435, L&T state, "For the computation of the total core neutron source, the core average value of ν, the total number of neutrons produced per fission, was taken to be 2.46, and the contribution to the neutron source from ^{235}U and ^{239}Pu was taken to be 60 and 40%, respectively, although the results are relatively insensitive to moderate variations in this ratio of fissioning nuclides." And, on p. 437, they state, ". . . its energy was selected from a ^{235}U watt fission spectrum . . . " We believe that this calculation is sensitive to variations in the fissile nuclides and that the use of the ^{235}U watt fission spectrum is not appropriate.

We have studied the source determination thoroughly in Refs. 8, 9, and 10. In Ref. 8 (~2 yr before Ref. 1 was submitted), we showed that the available spectra in the libraries may yield results for certain interactions such as $^{63}Cu(n,\alpha)$ that differ by more than 20%; further, it was demonstrated that these spectra were too hard or too soft beyond the ^{239}Pu and ^{235}U fission spectra. So, in Ref. 8, we recommended that care be taken in using a proper spectrum. In Ref. 9 (6 months before Ref. 1 was submitted) and Ref. 10 (submitted in October 1993, accepted in June 1994, and published in January 1995), we extended this work significantly by analyzing the effect of using different averaging approaches and different levels of homogenization. Three of the major conclusions of these papers that directly contradict Ref. 1 are as follows:

1. It is important to use a proper concentration of ^{235}U and ^{239}Pu as a function of fuel burnup and enrichment.
2. Using spectra from standard libraries results in ~20% differences for certain reaction rates.
3. The effect of uncertainty in the power-to-source conversion factor (ν/E_r) is minimal.

The approach used by L&T to determine the neutron source is not accurate and in certain situations can have a significant impact on the results. From their statements, it appears that L&T have used a (60% ^{235}U and 40% ^{239}Pu) combination to evaluate ν and possibly to estimate C (power-to-source conversion factor), while they have used a watt (^{235}U) fission spectrum for the MCNP calculations (Ref. 1, p. 437).

As mentioned, we have demonstrated that the effect of the spectrum on calculated reaction rates of certain interactions [e.g., $^{63}Cu(n,\alpha)$] is significant (~20%). Obviously, L&T have introduced significant errors in the source distribution, which drives the whole problem. So, their statement that their method

is more accurate and the fact they claim that their results are within 10% of the experimental results cannot be substantiated.

Tallying

In Ref. 1, p. 440, L&T state, "Finally, to speed up calculations, a tally volume larger than an actual foil volume was used . . . The effect of the size of the tally volume in the . . . was studied. . . . It was found that the flux variation was acceptable, generally <10% in integral and energy-dependent flux below 10 MeV. . . ." Obviously, because of the size of the tallying region, L&T may have introduced errors as high as 10%. But, they still claim, e.g., on p. 445, "Most foil saturation reaction rates were computed within ±10%," while besides the ~10% tallying uncertainty, they have ~10% statistical uncertainty and additional uncertainty due to the source spectrum.

Cross Section

In Ref. 1, p. 440, L&T state, ". . . [cross sections] were derived from ENDF/B-V compilations." Using these cross sections for this calculation raises immediate questions about the known underestimation of neutron transmission through steel due to the ENDF/B-V iron inelastic-scattering cross section. However, from Table IV in Ref. 1, it appears that maybe they have used the T-2 data for iron, which is appropriate and has been shown to be reasonably accurate. L&T, however, do not clearly state which iron data evaluation they are using and do not even acknowledge that it is a serious and relevant issue to this calculation. Further, if the MCNP results correspond to using the T-2 iron data and the DOT results are based on ENDF/B-IV iron data, the MCNP results should certainly be higher than the DOT results for this reason alone. This issue and its effect have been investigated by us and other researchers in Refs. 3, 5, 7, 11, and 12.

Dosimetry Cross Sections

In Ref. 1., p. 440, L&T identify the dosimetry cross-section libraries used in their calculations and make some statements to the effect that the cross sections derived from ENDF/B-V produced the best results with respect to the experimental data. The issue of dosimetry cross sections has been addressed in Refs. 3, 5, and 7 as well as by other researchers. However, the real problem here is that where L&T have calculated reaction rates with different dosimetry cross sections, they acknowledge only the best results and do not provide any discussion related to why the poor results were discarded. Further, they are not even completely consistent in their selection of results—they simply chose the calculated-to-experiment (C/E) ratios closest to unity.

These issues may account for some of the unexplained and inconsistent behavior of the results given in Table VII and VIII of Ref. 1. However, there is no explanation why in one cycle the reported C/E is 17.81, while for the same reaction, the C/E drops to 1.05 and 1.06 in cycles 10 and 11, respectively. Further, where C/E ratios were calculated with different dosimetry cross sections, L&T simply ignore the poorer result with no explanation or justification. Of the 33 cases that they do consider, 9 out of the 33 have relative errors that are >10% and thus are not statistically reliable. It is interesting that in spite of these results, L&T still claim a relative difference of 10%.

INACCURATE AND/OR SUPERFLUOUS STATEMENTS

I am surprised by the large number of inaccurate and/or superfluous statements in Ref. 1. A few are listed as follows:

1. On p. 434, L&T state, "In this work we present an *alternative* process that produces accurate results." They presented neither an alternative process nor a new application of the Monte Carlo method, and the accuracy of their results is questionable. Besides our work in this area, other researchers have applied Monte Carlo to this problem in the past. A few of these include Refs. 13 through 16.

2. On p. 433, L&T state, "Industry goals for uncertainty in quantities associated with light water reactor PV steel neutron exposure[7] are in the range of 10 to 15% and for dosimetry foil reaction rates are 5%." It is important to note that the 5% (from Ref. 7 of Ref. 1) refers to the uncertainty in measurements, not calculations, while on p. 443, L&T state, ". . . the goal of the industry is to be able to achieve a C/E within the ±5% range." This obviously is a misinterpretation of the original reference.

3. On p. 434, L&T state, "[S_N/synthesis] method shows weaknesses in the assumption of flux separability, the approximation of irregular geometry. . . ." Even if this were true, L&T have defeated the purpose of using Monte Carlo by not explicitly modeling source tubes (which house the foils) and by considering a large detector tallying region. These modeling approximations are not needed—especially since such approximations introduce uncertainties beyond the author's 5% limit.

4. On p. 435, L&T state, ". . . to compute the angular-dependent flux. . . ." Using the word "angular" is misleading because it can be mistaken with the angular flux that is direction dependent. I believe that L&T refer to azimuthal flux distribution.

5. On p. 438, L&T state, "Fission was treated as neutron capture since explicit treatment of fissioning in the MCNP model would result at best in an overestimation of the source and at worst in an infinite stack of banked progeny particles corresponding to a critical core condition." This statement is not needed, or the way it is stated is not proper. Clearly, this is a fixed-source problem, so why would anyone consider treating the fission explicitly and worry about overestimation?

6. On p. 439, L&T state, ". . . variance reduction techniques . . . were employed to accelerate the tally convergence to a statistically reliable result (relative error below ±10%)." This contradicts the results given in Tables VII and VIII, which reveal that 19 out of 48 of the calculated reaction rates have relative errors >10%, with a maximum value of 35.8%.

7. On p. 439, L&T state "Reflective geometry (one-eighth core modeled) reduced the size of the input model, allowing the code to run faster. Because of the reflective geometry, every. . . ." This statement is completely redundant and somewhat misleading. The reflective boundary condition is used because of the material symmetry in the physical model. In other words, the fact that the model becomes smaller or the cost becomes less is only the result of being able to use this boundary condition.

8. On p. 439, L&T state, "Energy cutoff was used to speed the foil reaction rate calculations by. . . ." Energy cutoff was used because the reactions of interest are not sensitive to lower energies; otherwise, it could not be used.

9. On p. 440, L&T state, "The majority of the cross sections used in this study were prepared by the Radiation Shielding Information Center in the MCNP type 1 (ASCII) format for use with MCNP4a. . . . " The Radiation Shielding Information Center has nothing to do with preparing the cross-section data for MCNP; they merely distribute the data that are supplied to them by the Transport Methods Group at Los Alamos National Laboratory.

10. On p. 446, L&T state, "These tallies failed to . . . , either as the result of the arrival of an isolated and heavily weighted particle from an undersampled region . . . or due to a folding of high-uncertainty portion . . . flux bin with moderately high. . . ." If L&T do not know/understand why their calculations give improper results, they should not publish their work because, after all, it is their, not the reader's, responsibility to explain and resolve the issues of their work—especially when they are claiming that they have developed "an *alternative* process that produces accurate results."

11. On p. 446, L&T state, "The method offers the possibility of an automated procedure for downloading core instrumentation data directly into the MCNP input for generation of computed flux spectra at multiple locations about the PV and ex-vessel cavity, within a moderate computational time." L&T do not give any new technique that facilitates downloading core instrumentation data. Further, it is ironic that they have not explicitly modeled regions such as cavity dosimetry and somewhat defeated the purpose of three-dimensional combinatorial geometry, which is offered by the MCNP code.

I realize that this is a rather long and detailed critique of Ref. 1, but I am very concerned with its inaccurate and misleading results and conclusions—especially since they can be used to project the life of commercial nuclear power plants. Also, I am concerned with the failure of the review process. Obviously, this paper does not comply with the following: (a) the work is correct and complete and (b) the authors give adequate credit to earlier work. (Note that none of the papers I referred to in my comments were referenced by L&T.)

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Reply to "On 'Neutron Fluence at the Pressure Vessel of a Pressurized Water Reactor Determined by the MCNP Code'"

We share with Dr. Haghighat a sincere and serious desire to further the application of Monte Carlo transport methods for the study of pressure vessel (PV) fluence and dosimetry with the important long-term goal of supporting reactor PV life-extension studies. We were very happy to read his detailed review of our work. We would like, however, to offer the following specific comments in response.