trajectory W'' which reaches the curve C in a time T'' shorter than T, then it can go further for the duration of time $T - T''$ (> 0), reaching a curve C'' which may give a xenon maximum smaller than x_m. This contradicts the assumption of minimax trajectory W, that is to say, the minimax trajectory W is also the time-optimal trajectory between A and C. Thus the equivalence of the xenon minimax and the time-optimal problems have been proved.

The multipulse solutions[6] seem, by our opinion, to have resulted from the practical computational difficulties sometimes associated with the method of dynamic programming.

As a numerical reference, the recent study of Motoda, Togo, and Oyama[7] shows that the results obtained by time optimal and minimax criteria are identical.

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[6]M. ASH, *ibid.*, **24**, 77 (1966).

[7]H. MOTODA, Y. TOGO, and A. OYAMA, Preprints for the Joint Meeting in Reactor Physics and Reactor Engineering, Atomic Energy Society of Japan. Fuse (1966).

Optimal Shutdown Control

My comments on "Further Comments . . ." by Y. Shinohara and J. Valat, (this issue) to my earlier rejoinder *Nuclear Science and Engineering* **25**, 213 (1966) to comments by J. J. Roberts and H. P. Smith *Nuclear Science and Engineering* **25**, 212 (1966) on my original article *Nuclear Science and Engineering* **24**, 77-86 (1966) are, after catching my breath and wondering about the philosophical implications of more and more about less and less, the following:

I reiterate my earlier rejoinder that "the time optimal extremal is equivalent to the minimax extremal where the minimum time coincides with the allowable shutdown time of the minimax solution." So far so good. Continuing, "However, the converse is not true. That is, minimax extremals are not necessarily time optimal extremals." Note, in this last statement I am talking about minimax extremals in general, not necessarily those of fixed allowable shutdown time.

Hence, as also discussed further in Chapters 8 and 9 of my monograph *Optimal Shutdown Control in Nuclear Reactors*, Academic Press (1966), one can, for example, multipulse the reactor to keep within the xenon constraint while maintaining the system on a minimax xenon, *but not minimum time*, extremal. After a number of pulses, whose characteristics are determined by the particular system parameters, one will reach the xenon-iodine phase space target curve.

Whether or not the Roberts and Smith explanation of the equivalence of minimax and time-optimal extremals is adequate, as questioned in the above Y. Shinohara, J. Valat Letter, seems to me to be a matter of taste. I think it is.

Multipulse solutions will sometimes result from the computational vagaries of using the dynamic programming method, if one is not careful. This can come about from adding an artificial cost, $10(x/x_c)^{20}$ for example, to the criterion functional to definitely assure that $x = x_c$, the xenon constraint, will not be exceeded by the phase-space extremal trajectory. There are however, other more efficient numerical devices that will accomplish the same thing. This is a negligible price to pay for employing the straight forward method of dynamic programming for this class of problems. Dynamic Programming gets one out of the bind of having to solve a messy two-point boundary value problem (especially on digital machinery), an unfortunate concomitant of the corresponding Pontryagin maximum principle formulation.

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November 25, 1966

Comment on the Optimal Shutdown Control

It is a pleasure to reply to "Further Comment on the Optimal Shutdown Control," by Shinohara and Valat[1] in which they correctly note the error in Ash's statement[2] that "minimax extremals are not necessarily time optimal extremals." The proof by Shinohara and Valat, that time optimal extremals are coincident with minimax extremals in which the fixed period of operation corresponds to the minimum time, is the same as that given by Roberts and Smith[3]. The reverse proof, i.e., that minimax extremals are equivalent to time optimal extremals under the conditions noted, is correct, well presented, and nicely extends our initial approach to the problem.

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January 6, 1967

[1]Y. SHINOHARA and JEAN VALAT, *Nucl. Sci. Eng.*, **27**, 156 (1967).
[2]M. ASH, *Nucl. Sci. Eng.*, **25**, 213 (1966).
[3]JOHN J. ROBERTS and HAROLD P. SMITH, Jr., *Nucl. Sci. Eng.*, **23**, 397 (1965).

Comments on the Time Optimal Xenon Shutdown Problem

An excellent solution of this problem has recently been presented by Roberts and Smith[1]. There is, however, one small point in their analysis which needs clarification. On page 476, the trajectory ABC is considered as a possible time-optimal trajectory in the restricted state space. It is assumed that at $B = \boldsymbol{x}(\tau)$, the flux did not switch in the unrestricted space solution. B is a junction point and the jump conditions of Theorem 3 must be satisfied. That is, p_1 and H must be continuous, but p_2 may jump.

[1]J. J. ROBERTS and H. P. SMITH, Jr., *Nucl. Sci. Eng.*, **22**, 470 (1965).

Now $x_2'(\tau^-) \neq 0$ but $x_2'(\tau+) = 0$. This implies that

$$\Delta\phi = \phi(\tau+) - \phi(\tau^-) \neq 0$$

which in turn implies that $\Delta x_1' = x_1'(\tau+) - x_1'(\tau^-) \neq 0$.

The authors state that since $x_2'(\tau+) = 0$, the Hamiltonian $H = p_1 x_1' + p_2 x_2'$ is unaffected by p_2 and conclude that $\Delta H = H(\tau+) - H(\tau^-) \neq 0$ which is a contraction. This statement is not completely correct and the reasoning at this point should be as follows:

$$\Delta H = p_1(\tau)\,\Delta x_1' - p_2(\tau^-)\,x_2'(\tau^-) \quad .$$

From Eqs. (9) and (10)[1]

$$\Delta x_1' = -\gamma_1 \Delta\phi = \frac{\gamma_1 x_2'(\tau^-)}{\sigma_2 x_2(\tau^-) - \gamma_2} \quad .$$

So $$\Delta H = x_2'(\tau^-)\left[\frac{\gamma_1 p_1(\tau)}{\sigma_2 x_2(\tau^-) - \gamma_2} - p_2(\tau^-)\right]$$

and the condition for $\Delta H = 0$ is

$$p_2(\tau^-) = \frac{\gamma_1 p_1(\tau)}{\sigma_2 x_2(\tau^-) - \gamma_2} \quad .$$

Comparing this[1] with Eq. (24) we see that this is precisely the condition for B to be a switch point in the unrestricted space. This contradicts the earlier assumption and helps in the justification of the final conclusions of the paper.

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Remarks With Regard to an Article by Tassan

S. Tassan has recently published results of thermal-neutron spectrum measurements employing ^{176}Lu : ^{164}Dy and ^{176}Lu : ^{175}Lu activation ratios[1]. Concerning this article I would like to make the following remarks:

The usefulness of the article does not lie in the fact that it brings much new information on methods for measuring thermal-neutron-lattice spectra, but rather in the fact that it supports, to some extent, results and conclusions arrived at in a Technical Note published earlier in this journal[2].

The use of ^{164}Dy (combined with ^{176}Lu), and its advantage over other thermal-detector materials that have a higher resonance integral relative to their thermal cross sections, was already suggested in a 1964 Geneva Paper[3]. In the same year an experimental method for determining intracell thermal-spectrum distributions was described in detail by Smit[4]. It surpasses Tassan's method in that it also gives information on the spectrum's spatial variation inside fuel and moderator and not only averaged values for these regions, and thereby provides better material for a test of THERMOS-like codes.

It may very well be that the two latter publications were not known by Tassan at the time he wrote his article. However, in the period when he revised it (Revised March 1, 1966) the above-mentioned Technical Note appeared (January 1966) where reference was made to them. It surprises me that this has escaped both his attention and the editors', and that the article was published at all.

In the Technical Note the comparison between theory and experiment was pushed farther than in the article because, besides the Nelkin model, two other scattering models were tested. This led to accepting the improved Nelkin model, due to Koppel and Young (KY)[5], as the best of the three. This implies that if Tassan had compared his results with better THERMOS calculations using the KY model, the agreement would, on the average, have been better in the moderator but worse in the fuel.

Tassan noticed an approximately three times larger separation between calculated than between measured fuel and moderator ratios. A much weaker difference may be observed in some of the results of Smit and Stamm'ler (SS)[2] and in my opinion this large discrepancy should be mainly ascribed to uncorrected experimental errors. First, the presence of aluminum in the fuel introduces streaming effects that tend to soften the spectrum. Therefore its use should be minimized and copper catcher foils should be applied instead of aluminum. Even they will cause a disturbance and so will the remaining aluminum present in the Lu-Al and Dy-Al foils. Second, the presence of the foils in the moderator will cause a local spectrum hardening. In that respect Tassan's lutetium foils may have had more influence than the foils used by SS where the lutetium concentration was $2\frac{1}{2}$ times lower.

In connection with this kind of measurement it might be useful to draw the attention to two recent publications where improvements of Smit's method are presented as well as more experimental results and their analysis[6,7]. Although on the whole these results compare better with calculational methods than Tassan's, the agreement is not yet wholly satisfactory. A point that deserves closer investigation in this respect is the spectrum in the thermal column which has been assumed to be Maxwellian with the same temperature as that of the column.

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[1] S. TASSAN, "Thermal Spectrum Measurements in Slightly Enriched Uranium, Light-Water-Moderated Lattices by the Lutetium Activation Method," *Nucl. Sci. Eng.*, **26**, 271 (1966).

[2] J. SMIT and R. J. J. STAMM'LER, "On the Variation of the Lu: Dy Activation Ratio in Reactor Cells," *Nucl. Sci. Eng.*, **24**, 90 (1966).

[3] E. ANDERSEN et al., "Experimental and Theoretical Studies of Uranium Oxide Lattices Moderated by Mixtures of Light and Heavy Water," *Proc. 3rd Intern. Conf. Peaceful Uses At. Energy, Geneva*, III, 197 (1964).

[4] J. SMIT, "Analysis of Neutron Sepctra in UO_2 Lattices Moderated by Mixtures of Light and Heavy Water," KR-87, Kjeller, Norway (1964).

[5] J. U. KOPPEL and J. A. YOUNG, "Neutron Scattering by Water Taking into Account the Anisotropy of the Molecular Vibrations," *Nucl. Sci. Eng.*, **19**, 412 (1964).

[6] J. SMIT, "Measurement of the Spatial Variation of the Thermal Neutron Energy Spectrum in Reactor Cells," KR-103, Kjeller, Norway (1966).

[7] R. J. J. STAMM'LER, S. M. TAKAČ, and Z. J. WEISS, *"Neutron Thermalization in Reactor Lattice Cells: An NPY-Project Report,"* pp 82-84, 117, 121, Series No. 68, International Atomic Energy Agency, Vienna (1966).

Answer to Mr. Stamm'ler's Comments on Article on Thermal Spectrum Measurements

The remarks by R. J. J. Stamm'ler to my article[1] deserve some comment and explanation.

[1] S. TASSAN, *Nucl. Sci. Eng.*, **26**, 271 (1966).