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).

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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).