Reply to "Tests of T and CPT in neutral-kaon decays"

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I comment on who discovered first the main result in my paper. I also correct a miswriting of mine in the paper.

The main point in my paper is that, if there is CPT violation in the neutral-kaon mixing, then the parameters η for the different kaon decay channels — for instance the CP-violating η_{+-} and η_{00}, corresponding respectively to the decay channels $\pi^+\pi^-$ and $\pi^0\pi^0$ — are not experimentally well defined. This means that measurements of them in kaon beams with different initial compositions — for instance, in one beam which at the initial time $t = 0$ was pure K^0, and in another beam which at $t = 0$ was pure $\overline{K}^0$ — should yield different results. The difference among those results constitutes a measurement of the complex CPT-violating parameter in the neutral-kaon mass matrix. This point is embodied in Eq. (13) of my paper.

After publishing my paper, I discovered that this result was not really original, and that it had been first discovered by Enz and Lewis [1], and later also mentioned by Lee and Wu [2], all of this more than twenty years ago. I assert however that all the other authors who wrote about this subject both at that time and since then, did not mention Enz and Lewis' result, appeared to ignore it, and usually wrote as if that result were false. I have reread carefully all the papers by Kabir that he cites in his Comment, and find that the tests of CPT violation presented in them are different from the ones that I suggest.

It is regrettable that, in a time where experimentalists are testing CPT by looking at a phase difference between η_{+-} and η_{00}, they appear to ignore that, if CPT is violated, those two parameters themselves, and of course their phases, are not well defined. This is a reflection of the fact that Enz and Lewis' result has been completely overlooked in the literature after 1965.

The last-but-one paragraph of Kabir's Comment concentrates on Eqs. (7) and (8) of my paper. Those equations embody a secondary point in my paper. I did not claim that the test of CPT contained in them "possesses special advantages." I also did not claim that that test was "new"; quite the opposite, I gave the reference to the paper where it was originally suggested [3]. Kabir seems to take my Eq. (7) as representing a *decay* curve, when it really represents an *oscillation* curve. His misinterpretation is justified by a miswriting of mine, for at a point I wrote "a precise fit to the decay curve by the expression of Eq. (7)...," and I should have written "oscillation" and not "decay." If this error of mine is corrected, Kabir's criticism ceases to be relevant.

[1] C. P. Enz and R. R. Lewis, Helv. Phys. Acta **38**, 860 (1965).
[2] T. D. Lee and C. S. Wu, Annu. Rev. Nucl. Part. Sci. **16**, 511 (1966).
[3] F. S. Crawford, Jr., Phys. Rev. Lett. **15**, 1045 (1965).