Remarks on a challenge to the relation between \textit{CPT} and Lorentz violation

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Abstract

The objection to my theorem that violation of \textit{CPT} symmetry implies violation of Lorentz covariance is based on a nonlocal model in which time-ordered products are not well defined. I used covariance of time-ordered products as the condition for Lorentz covariance; therefore the proposed objection is not relevant to my result.

1 Introduction

In demonstrating that violation of \textit{CPT} symmetry implies violation of Lorentz covariance, I explicitly assumed the properties of relativistic quantum field theory that are the basis of the Wightman formalism and Jost’s theorem for the necessary and sufficient conditions for \textit{CPT} symmetry. (See reference [3] of [1].) I chose covariance of of $T$ (or $r$ or $a$) products as the criterion for Lorentz covariance. I also implicitly assumed (i) that the $S$ matrix is well defined in the theory, which means that the in and out fields are related by a unitary $S$ matrix, $\phi^{out}(x) = S^{-1} \phi^{in}(x)S$, (ii) that the theory has a finite number of fields, and (iii) that the theory is formulated on ordinary (commutative) spacetime.

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A recent paper that challenges my result [2] concerns a nonlocal model in which the $T$ products are not well-defined and thus is not a counter example to my result. In addition, the paper by R. Marnelius [3] cited by the authors for “general considerations on the causality and unitarity properties of nonlocal relativistic quantum field theories.” states in the abstract “This implies that the field equations do not yield unique quantum solutions and in particular that the solutions with canonical incoming free fields are different from the solutions with canonical outgoing free fields, and none of these solutions render the total action stationary. No meaningful $S$ matrix can therefore be defined. (Boldface added by me.) It is also shown that this deficiency cannot be corrected either by restricting the form function or by adding correction terms to the perturbation expansions.”

Further, the authors of [2] cite two papers, [5] and [6], that I criticized, [1], as though I were the author of those papers.

The challenge to my theorem does not question my derivation of the theorem based on the fact that violation of one of the necessary and sufficient conditions for CPT symmetry also violates one of the conditions for Lorentz covariance.

It is well known that local commutativity (or anticommutativity) of fields implies covariance of time-ordered products. I also proved the converse of this result, that covariance of time-ordered products implies local commutativity (or anticommutativity) of fields. [4] Since I assumed covariance of the $T$ products, the fact that, as the authors state, local commutativity of fields is violated in their model is a second way in which their nonlocal model violates the conditions of my theorem and thus is not relevant to my result.

2 Discussion of other comments of [2]

Section 2 of the authors’ paper refers to the model of [5] and [6]. These papers are their reference [15]. They accept my criticisms [1] of these two papers, but nonetheless then refer to this model three times as though I had proposed it. Where they should cite their reference [15], they cite my paper which is their reference [17]. This repeated interchanging of their references [15] and [17] is highly misleading. They call the model of [5] and [6] “utmost pathological” and repeat further criticisms of the
model that I had already made [1]. They correctly state that because observables in this model do not commute at space-like separation, the proof of the spin-statistics relation is not valid. They then state “there is no concept of spin to start with altogether.” This is not correct; one can define spin by the transformation property of the field under the Lorentz group or under the rotation group with support on only one mass shell in the same way one can define spin for a field with support on both mass shells.

At the end of their section 4 the authors assert “With such a CPT-violating interaction ... the quantum corrections due to the combined interactions could lead to different properties for the particle and antiparticle, including their masses.” The authors do not provide a calculation to support this assertion, nor do they explain how their model would evade the specific detailed problems, including violations of Lorentz covariance, pointed out in my paper [1].

3 Summary

The proposed counter-example of [2] is not relevant to my theorem because I assumed Lorentz covariance of $T$ products as a condition of my theorem and the $T$ products in their model are not covariant. In addition, according to a reference [3] cited by the authors, their model does not have a properly defined S matrix at all.

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