Reply to referee report#5(?)

[redacted]ZABO: On the meaning of Lorentz covariance)

(The report starts as "I am in favour of publication of a revised, shortened version of the paper. I think the main ...")

Referee#4 and referee#5 suggested changes. The enclosed new version of the manuscript is a result of a compromise between their divergent suggestions.

I. Reply to the referee's 'Main point'.

The referee writes: *[E]examples 1-3 do not involve rigid bodies and I think are simply irrelevant. Example 4 involves a system with a spring, which only behaves relativistically under boosts when it relaxes to equilibrium, and so when it can be treated as rigid. Why is this not covered by Bell's point?*

I don't think that the system with a spring only "behaves relativistically under boosts when it relaxes to equilibrium". The system's behavior is governed by the Lorentz covariant equations of relativistic physics throughout its motion. (Just like the behavior of the systems in the other three examples. That is why they are not "irrelevant" from the point of view of my claim. Especially example 3 is important.) What I wanted to accentuate is that in spite of the fact that these Lorentz covariant relativistic laws of physics exactly hold for the system's behavior *both before and after* its relaxation, the relativity principle only holds for the state after the relaxation process. The reason is that the system's behavior—although is governed by the relativistic Lorentz covariant laws—differs from the solution $\left[\Lambda_v^{-1}\left(\psi_0'\right)\right]$ before relaxation. Similarly, the Lorentz covariant relativistic laws of physics continuously hold for the system's behavior in example 3, but the relativity principle is never satisfied, because the system's behavior—in spite of the fact that it is governed by the relativistic laws—differs from the solution $\left[\Lambda_v^{-1}\left(\psi_0'\right)\right]$ at any time. That is to say, in relativity theory, relativity principle is not a universal principle (as Einstein calls it); it does not hold for the whole range of validity of the Lorentz covariant laws of relativistic physics. This is a significant difference from classical mechanics, where relativity principle is indeed a universal principle: It holds for the whole range of validity of classical mechanics. (We do not need to make restrictions for the system to be a "rigid body" or to be "connected", or to be in equilibrium, etc.)

I hope Bell and Jánossy, to whom Bell refers in his paper, were not surprised by this conclusion. But none of them makes an explicit claim like this.

The referee writes: *I do not understand why the main point the author is making leads to the conclusion (p. 12) that "Lorentz covariance is not a fundamental symmetry of physics". One can and should accept the authors "thermodynamic" point and still conclude that Lorentz covariance is fundamental. The claim that "nothing experimentally supports" it is surely questionable.*

I accept this criticism. Although the normative/heuristic requirement of Lorentz covariance is usually derived from the universal validity of relativity principle, it is indeed logically possible that relativity principle is not universally valid, but the principle of Lorentz covariance is. Although, I think, it is questionable whether we have enough empirical evidences confirming the fundamental feature of Lorentz covariance, independently of the relativity principle. (The Lorentz covariance in the equilibrium quantities are, of course, still confirmed by the relativity principle.) Anyhow, the corresponding statement in the manuscript should be reformulated in this way: Since relativity principle is not a universal principle, *it* does not entitle us to infer that Lorentz covariance is a fundamental symmetry of physics.

II. Reply to the 'Other issues'

I do not reflect to the points 1, 2, 3, because the corresponding passages of the paper have been canceled. In point 4 the referee makes two remarks.

The referee writes: *[I]t is unclear what "a solution of [a set of differential equations] that describes the same behavior of the system as [the initial conditions when the system is at rest] but in a superposition with a collective translation with velocity v" means. It should mean that the initial conditions of the new arrangement look to the co-moving observer just as the original initial conditions did to the stationary observer.*

This is an essential point indeed. I call this problem the "vagueness" of ψ_v and discuss it on many pages of the manuscript (see page 3, 4, 11, in the new version). If I correctly understand, the referee's suggestion is the following:

$$\psi_v \overset{def}{=} \Lambda_v^{-1}(\psi_0') \tag{1}$$

This definition would be be problematic, I think. For instance, the usual Einsteinian derivation of the Lorentz transformation starts with the declaration of relativity principle. Therefore, Lorentz transformation must be logically preceded by the concept of a physical object in a uniform motion relative to K. Another reason is that, with this definition, equation (12, in the new version) would be satisfied automatically. Consequently, in case of Lorentz covariant theories, the relativity principle would be satisfied, by definition. This contradicts to the observation—with which the referee seems to agree—that relativity principle holds only for the equilibrium properties of dissipative systems.

The referee writes: *[T]he author is interested in the set of equations and conditions "expressed in primed variables applying the Galilean and the Lorentz transformations, respectively." But the author appears to overlook the fact that* *the dynamical entities in these equations, such as the fields in Maxwell's equations in the relativistic case, generally also have to undergo non-trivial transformations over and above the coordinate transformations. Without these extra dynamical transformations his equations (11), (12) do not in general capture the relativity principle. It is not clear to me furthermore that either this point (that is evident in any text-book treatment of the Lorentz covariance of Maxwell's equations) or the first has properly been appreciated in the examples on pp. 7–10.*

I agree with everything in this remark, except the statement that I "overlook" these facts. I wonder why does the referee think that the set of equations $\mathcal{E}$ describing the whole system does not contain the relevant equations describing the interactions. As I mentioned in the paper, a typical example is a system of relativistic particles coupled to electromagnetic field. Let me "copy-paste" here this typical example for such an $\mathcal{E}$ from the first submitted version of this manuscript: The system can be described by the retarded potentials (derived from the Maxwell equations)

$$\mathbf{A}(\mathbf{r},t) = \frac{1}{c}\sum_{i=1}^{n}\frac{q_i}{d}\left.\frac{d\mathbf{r}_i(t)}{dt}\right|_{t-\frac{d}{c}} \tag{2}$$

$$\varphi(\mathbf{r},t) = \sum_{i=1}^{n}\frac{q_i}{d} \tag{3}$$

$$d = \left|\mathbf{r}-\mathbf{r}_i\left(t-\frac{d}{c}\right)\right| \tag{4}$$

and the dynamical equations of the particles

$$\frac{d}{dt}\left(\frac{m_i}{\sqrt{1-\frac{1}{c^2}\left(\frac{d\mathbf{r}_i(t)}{dt}\right)^2}}\frac{d\mathbf{r}_i(t)}{dt}\right) = -q_i\,\mathrm{grad}\varphi\left(\mathbf{r}_i(t),t\right) - \frac{q_i}{c}\frac{\partial\mathbf{A}\left(\mathbf{r}_i(t),t\right)}{\partial t} + \frac{q_i}{c}\left[\frac{d\mathbf{r}_i(t)}{dt},\mathrm{rot}\mathbf{A}\left(\mathbf{r}_i(t),t\right)\right] \tag{5}$$

This system of equations is covariant with respect to the following Lorentz transformations:

$$x_1' = x_1 \quad x_2' = x_2 \quad x_3' = \frac{x_3 - vt}{\sqrt{1-\frac{v^2}{c^2}}} \quad t' = \frac{t-\frac{vx_3}{c^2}}{\sqrt{1-\frac{v^2}{c^2}}} \tag{6}$$

$$A_1' = A_1 \quad A_2' = A_2 \quad A_3' = \frac{A_3-\frac{v}{c}\varphi}{\sqrt{1-\frac{v^2}{c^2}}} \quad \varphi' = \frac{\varphi-\frac{v}{c}A_3}{\sqrt{1-\frac{v^2}{c^2}}} \tag{7}$$

including, of course, the Lorentz transformation of the field variables (of the potentials, in this formulation). In the light of previous referee reports, to keep the formalism simple I removed, for example, the above example and itemize only those things that are necessary. But, all statements and all formulas are correct, I believe, and this "reader friendly" solution does not hurt the "spirit" of the more detailed analysis. Anyhow, in this new version of the manuscript I tried to add a few sentences to make this background more clear.

Finally I would like to thank to the referee for mentioning two papers by Brown and Pooley. These papers are important contributions to the issue in question, indeed. (Actually I knew these writings, and I was mentioning them in my "Szabó 2003" referred in the manuscript.)