Reply to referee report 1

(SZABO: On the meaning of Lorentz covariance)

Taking into account of the referee reports, I have made important changes in the manuscript. However, there is no change in the main message of the paper. In this reply, I am reflecting to all of the referee's remarks and criticism, even if the corresponding passages have been rewritten or removed from the text.

1. Let me first reflect to some minor points:

- "*There is little if any discussion of similar proposals in the literature.*"

  My paper is not a review paper. Interpretation of special relativity has a huge literature, of course. I am referring only to those writings that were necessary. What does the referee mean a 'similar proposal'? I have the following claims: 1) Lorentz covariance and the principle of relativity are not equivalent. 2) The principle of relativity actually only holds for the equilibrium quantities characterising the equilibrium state of dissipative systems. 3) Lorentz covariance should not be regarded as a fundamental symmetry of the laws of physics. I do not know any publication which has the same or equivalent claims. (Note that some new references have been added in the new version of the paper.)

- "*A great deal of the paper is spent on routine calculations in special relativity. I think the author can presume that readers know how to apply the Lorentz transformation. That same space would be better spent arguing for the novel theses.*"

  I am ready to develop my arguments for the novel theses in more details—according to the referee's criticism. I do not want however to cancel the "routine calculations in special relativity". My impression is that we are often doing these formal derivations without deeper thinking about the physical content of what we are actually doing.

- "*The author's proposal reminds me of Lorentzian electrodynamics without an ether state of rest.*"

  I think the referee means Lorentz's theory (of relativity)—with the constructive explanation of the contraction of rods and the phase shift of clocks, the Lorentz principle, etc. [see Bell 1987]. My "proposal" is however different from the Lorentzian theory. Quite the contrary, as I have shown in a recent paper [arXiv:physics/0308033], Lorentz's theory and special relativity are completely identical—they are not simply equally good alternative theories, as they are usually regarded to be, but completely identical in both sense as physical theories and as theories about space and time—, sharing, consequently, the same difficulties.

- *"Example 1 describes a system of electric particles interacting solely electromagnetically and nonetheless in static equilibrium. Readers will wonder how such systems are possible. I can only think of a highly contrived infinite system."*

  The example says that "**some** of the particles are in equilibrium and they are at rest", that can be the case, approximately, for a while, even if the charges are not zeros, if we neglect the radiation of the particles. [A similar system is discussed by Bell 1987.] But, as it will be shown below (and also mentioned in the (original version of the) paper in Example 2), there is a trivial particular case, when all the particles are of charge zero and are at rest. [See also the famous "two rockets problem".] Anyhow, this is indeed a problematic example (I think it is problematic also in Bell 1987) and, since it is not needed, I have removed this example from the manuscript.

- The referee "cites" me in the following way: *'...For (38) [description of motion of boosted particles] describes the motion of the particles only for t > [boosted initial instant]. Before that time there is a deformation of the system, since the particles start their motions at different moments of time from various places...'* Then (s)he writes: *"This is obscure to me. The original system was described only for times t>0, so the boosted system is described only for times t > [boosted initial instant]. So the example gives no basis to claim a deformation or anything else happened prior to this time. Indeed it is unclear what is meant by deformations at all. Are they length contractions, time dilations, accelerations, what? And what is their significance? If we have to make some assumption about what happened prior to the initial instant, why would it not just be more of the same: the particles in the original system are at rest; so the boosted particles are in uniform motion?"*

  It seems the referee didn't understand my notations here. There is no such a thing as *the* "boosted initial instant"—common to all particles. The referee quotes the corresponding passage of the manuscript incorrectly. As I wrote in the paper, (38) holds "for large t, $t > t^\star_\alpha$ $(\forall\alpha \in I_1)$", where $t^\star_\alpha$ is the boosted initial instant—in the referee's words—**for the α-th particle**. There is nothing obscure here. In the boosted system, every particle is described from its own boosted initial instant $t^\star_\alpha$—if we prefer this language (although, giving an initial condition does not necessarily means that the solution in question describes the motion only for $t > 0$, it just fixes a particular solution by prescribing the state of the particle at a given moment of time). Let me explain it through a simple example. Assume that the system consists of two particles of charge zero, being at rest, one at the origin of K, the other at the point $(0,0,d)$. The Lorentz boosted system corresponds to two particles moving at constant velocity $(0,0,v)$, such that they satisfy the following conditions (see (35)–(36) in the manuscript):

$$t^\star_1 = 0$$

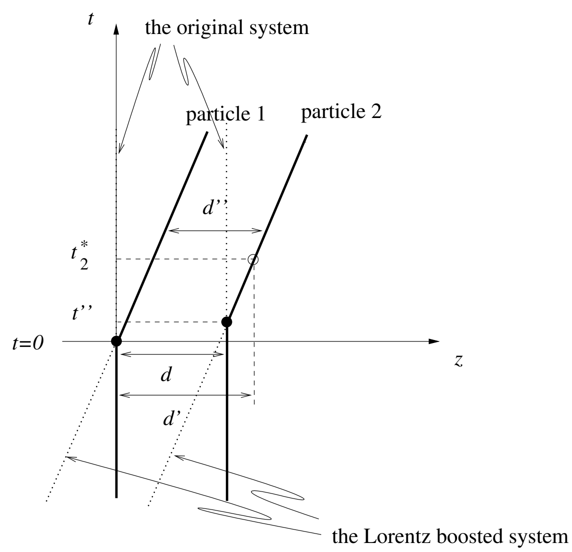

Figure 1: Both particles are at rest. Then particle 1 starts its motion at $t=0$. The motion of particle 2 is such that it goes through the point $(t_2^\star, d')$, where $d' = \frac{d}{\sqrt{1-\frac{v^2}{c^2}}}$, consequently it started from the point of coordinate d at $t'' = d\left(\frac{v}{c^2\sqrt{1-v^2/c^2}} - \frac{1-\sqrt{1-v^2/c^2}}{v\sqrt{1-v^2/c^2}}\right)$. The distance between the particles at t'' is $d'' = d\sqrt{1-v^2/c^2}$, in accordance with the Lorentz contraction.

$$\begin{aligned} t_2^\star &= \frac{\frac{v}{c^2}d}{\sqrt{1-\frac{v^2}{c^2}}} \\ \mathbf{r}_1^{new}(0) &= \begin{pmatrix} 0 \\ 0 \\ 0 \end{pmatrix} \\ \mathbf{r}_2^{new}\left(\frac{\frac{v}{c^2}d}{\sqrt{1-\frac{v^2}{c^2}}}\right) &= \begin{pmatrix} 0 \\ 0 \\ \frac{d}{\sqrt{1-\frac{v^2}{c^2}}} \end{pmatrix} \end{aligned} \tag{1}$$

The corresponding solution does not "know" about how the system was set into motion satisfying these conditions. Consider the following possible scenarios:

Example 1 The two particles are at rest; the distance between them is d (see Fig. 1). Then, particle 1 starts its motion at constant velocity v at $t=0$ from the point of coordinate 0 (the first two dimensions are omitted); particle 2 start its motion at velocity v from the point of coordinate d with a delay at time t''.

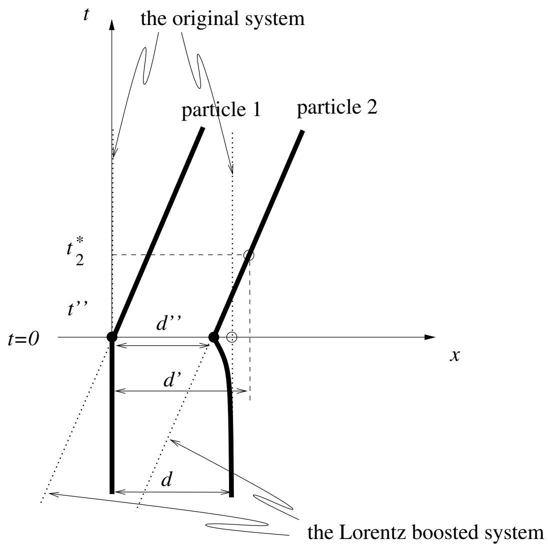

Figure 2: Both particles start at $t = 0$. Particle 2 is previously moved to the point of coordinate $d'' = d\sqrt{1 - v^2/c^2}$.

Meanwhile particle 1 moves closer to particle 2 and the distance between them is $d'' = d\sqrt{1 - v^2/c^2}$, in accordance with the Lorentz contraction. Now, one can say that the two particles are in collective motion at velocity v relative to the original system K—or, equivalently, they are collectively at rest relative to K'—for times $t > \frac{\frac{v}{c^2}d}{\sqrt{1-\frac{v^2}{c^2}}}$. In this particular case, they have actually been moving in this way since t''. Before that time, however, the particles moved relative to each other, in other words, the system underwent deformation.

Example 2 Both particles started at $t = 0$ (or, equivalently, they were uniformly accelerated from rest to velocity v), but particle 2 is previously moved to the point of coordinate $d\sqrt{1 - v^2/c^2}$ and starts from there. (Fig. 2)

Example 3 If both particles started at $t = 0$ (or, equivalently, they were uniformly accelerated from rest to velocity v) from they original places then the distance between them would remain d (Fig. 3). Still, we would say that they are in collective motion at velocity v, although this motion would not be described by the Lorentz boost.

Example 4 If, however, they are connected with a spring, then the spring (when moving at velocity v) first finds itself in a non-equilibrium state of length d, then

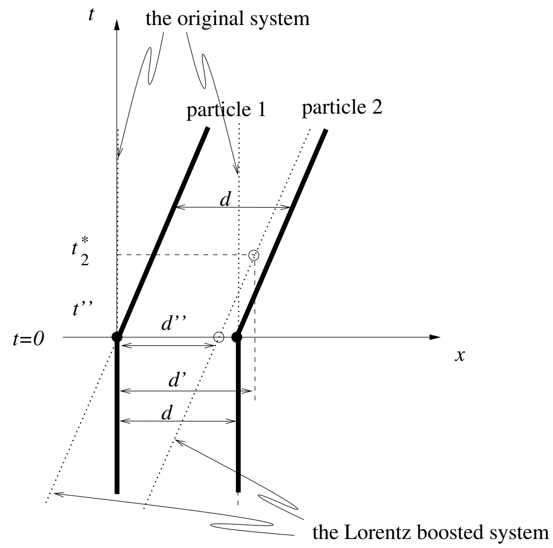

Figure 3: Both particles start at $t = 0$ from the original places. The distance between the particles does not change.

it relaxes to its equilibrium state (when moving at velocity v) and—assuming that the equilibrium properties of the spring satisfy the relativity principle, which we will argue for later on—its length (the distance of the particles) would relax to $d\sqrt{1-v^2/c^2}$, according to the Lorentz boost.

We have seen from these examples that the relationship between the Lorentz boost and the systems being in collective motion is not so trivial. In Examples 1 and 2—although, at least for large t, the system is identical with the one obtained through the Lorentz boost—it would be entirely counter intuitive to say that we simply set the system in collective motion at velocity v, because we first distorted it: in Example 1 the particles were set into motion at different moments of time; in Example 2, before we set them in motion, one of the particles was relocated relative to the other. In contrast, in Examples 3 and 4 we can rightly say that the system was set into collective motion at velocity v. But, in Example 3 the system in collective motion is different from the Lorentz boosted system (for all t), while in Example 4 the moving system is indeed identical with the Lorentz boosted one, at least for large t, after the relaxation process.

- *"...the Lorentz boosted system is not a Galilean boosted system. But surely the author does not intend that remark to establish that a Lorentz boost does not yield the original system set into uniform motion. "*

  "I could find only two paragraphs in which the author argues that the Lorentz boost is not (loosely speaking) the original system set into uniform motion."

  I do not claim in general that "the Lorentz boost is not the original system set

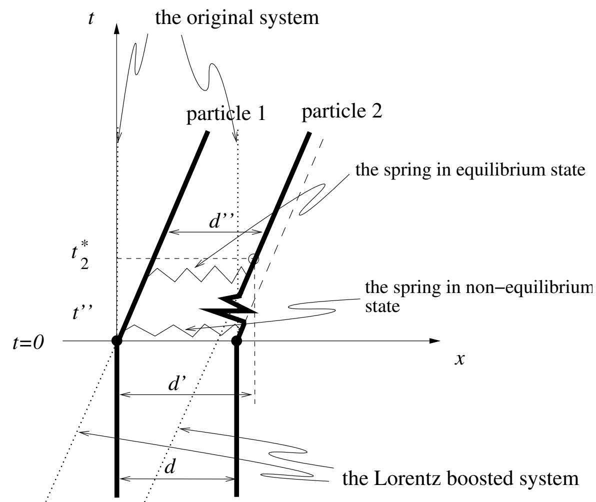

Figure 4: The particles are connected with a spring (and, say, the mass of particle 1 is much larger)

> into uniform motion". What I do claim is that the situation is more complex: sometimes it does correspond to the original system in collective motion, sometimes does not, depending on further details: the dissipation-relaxation process in question, the quantities for which the principle applies to, for the moment of time—before or after the relaxation—, etc.

2. I agree with the referee that *"At this point a lot depends on what we mean by the system set into uniform motion."* I cannot agree however with the referee's following suggestion:

"*If, as is standard, we mean a system that appears identical to co-moving observers/measuring instruments, then I have seen no argument at all for the claim. The Lorentz boost of the system is the only system I know that assures us that all co-moving measurements will have the same outcomes. Another system–say one that is not Lorentz contracted–will not have this property. Why should I believe that there are other systems without this problem? If the author does not intend the moving system to yield identical results from co-moving measurements, they why should we regard that system as being the original set into uniform motion? Co-moving observers would certainly regard them to be different.*"

Again, the referee's proposal is this:

> **Definition** A system i*s set into collective motion together with* K' if its behaviour, expressed in terms of the results of measurements obtainable by means of measuring-rods and clocks co-moving with K' is the same as the behaviour of the original system, expressed in terms of the measurements with the devices at rest in K.

I don't believe that this would be the "standard" definition. If we accepted this definition then the relativity principle—which was regarded by Einstein as one of the two fundamental theses of special relativity—would be nothing but a vacuous tautology:

> **Relativity principle** IF the behaviour of a system, expressed in terms of the results of measurements obtainable by means of measuring-rods and clocks co-moving with K' is the same as the behaviour of the original system, expressed in terms of the measurements with the devices at rest in K, THEN the behaviour of a system, expressed in terms of the results of measurements obtainable by means of measuring-rods and clocks co-moving with K' is the same as the behaviour of the original system, expressed in terms of the measurements with the devices at rest in K.

In other words, the relativity principle would be an *a priori* and trivial truth, no matter what we observe in the laboratory. I don't believe that this is the standard view. Quite the contrary, it is usually assumed that the 'collective motion of the system' has an independent meaning and the relativity principle is a contingent statement about the world, but not a tautology. (Referee#2, for instance, objected to my thesis by saying that the Lorentz boosted system *never* corresponds to the original system set in collective motion. In saying this, (s)he probably had a previous, independent meaning of 'being set into collective motion' in mind.)

I hope that this reply made my position more clear and acceptable for publication—even if there have remained several points where the referee has different views.