Report 4 on “On the meaning of Lorentz covariance”, by L E Szabó.

I am in favour of publication of a revised, shortened version of the paper. I think the main point made by the author is sound, but it does not need the degree of elaboration the author gives it, and nor is it quite as original as he seems to think it is.

I. Main point.

JS Bell pointed out in his essay on how to teach relativity (cited by the author) that when boosting a rod, the acceleration must be gentle, so as not to disrupt the forces of cohesion in the rod. (Bell’s point was directed to the internal forces within elements—atoms—of the rod, but it is implicit that it also applies to the forces of cohesion between atoms and molecules.) Similar remarks have been made repeatedly in the literature on SR in relation to the so-called clock hypothesis—a clock’s proper time will correspond to the Minkowski length of its worldline only if it is not accelerated so rapidly as to threaten its integrity. All this is fairly well known.

The author’s main aim seems to be to strengthen Bell’s point. He claims to give examples (1-4, pp. 7, 8) showing that gentle acceleration is not enough for the system to end up being Lorentz contracted. But 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?

The author is right to stress the role of equilibrium in the definition of boosting, and hence testing the relativity principle, but I cannot help feeling it is largely implicit in Bell’s account. On p. 12 in the conclusions we see the claim that the “relativity principle is not a general principle … One has to recognize that the [principle] … is experimentally confirmed only in such a restricted sense [i.e. when rods and clocks are in internal equilibrium]” . I think this is a valid point but over-elaborated and over-dramatised in the paper. It is obviously correct, would not have surprised Bell, and does not need all the complicated examples the author gives.

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.

II. Other issues.

1. In the second line of p. 1 the author states that it is widely accepted that special relativity (SR) is a “principal [sic] theory”. Nothing that follows in the paragraph explains what is meant by this, and it is far from clear whether the author understands the distinction Einstein introduced between “principle” and “constructive” theories when he (Einstein) classified SR as a principle theory. In fact, the whole tone of the author’s analysis in this paper is in the spirit of a constructive reading of SR![1]

2. I am not sure how much hangs on it, but the empirical definitions of space-time coordinates on p. 2 are in my opinion misleading. The definitions should refer to *ideal* rods and clocks if any. It is odd that the Bureau of Weights and Measures should

play such a prominent role in these definitions. First, the instruments they contain are not ideal, and inertial coordinates cannot and should not be *defined* in terms of them. Their only role is conventional, to fix the units involved, not space and time metrics. The most accurate clocks in the world are not found in Paris, and anyway it is theory (which incorporates inertial coordinates) that tells experimentalists what is the most accurate clock, not the experimentalists telling the theorists!

3. I am not sure the transverse Doppler effect by itself fixes the time dilation (p. 2). The standard experiment-based justification of the Lorentz transformations relies on the combination of three experiments, the Michelson-Morley, Kennedy-Thorndike and the Ives-Stilwell experiments. The last—dealing with the transverse Doppler effect—only yields the familiar dilation effect in conjunction with the first two. And it is just wrong to say that the Michelson-Morley experiment yields the familiar longitudinal length contraction effect directly. It is consistent with a family of distortion effects, including expansion as well as contraction, transverse as well as longitudinal, of which the usual contraction is a special case.[2]

4. The formal treatment of the relativity principle on p. 4 is deficient on two counts. First, it is unclear what "a solution of [a set of differential equations] that describes the same behaviour 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. Secondly, the 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.

1. See for example the discussion of the principle vs constructive theory discussion in H.R.Brown and O. Pooley, 'The origin of the spacetime metric: Bell's 'Lorentzian pedagogy' and its significance in general relativity', e-print gr-qc/9908048. Published in *Physics Meets Philosophy at the Planck Length*, C. Callender and N. Huggett (eds.), Cambridge University Press, 2001; pp. 256-272.

2. This is spelled out in detail in H R Brown 'The origins of length contraction: I the FitzGerald-Lorentz deformation hypothesis', *American Journal of Physics* **69**, 1044-1054 (2001). E-prints: gr-qc/0104032; PITT-PHIL-SCI00000218.