(Unsolicited) Response to the referee reports

I am not very much surprised by the referees' rejection, on the basis of the standard views on relativity theory. I wonder if I had received such a paper several years ago, I probably would have had the same reaction, without enough patience to read it in details. Still, I would like to ask the referees to read the paper more carefully, and to challenge what I am actually claiming.

1. Referee#2 seems to challenge my claims by writing:

> Reasonable people can of course differ over the ontological status of the Minkowski space-time structure (relational, substantival, or something in between). What is much more difficult to disagree about is that the structure of space-time in a regime governed by Lorentz invariant laws is Minkowskian and not Newtonian.

But I have never claimed that "the spacetime is Newtonian and not Minkowskian". This is a misinterpretation of the basic logical schema of my claim. Here is what I wrote in the paper:

> According to [the standard] interpretation of special relativity, the story can be described by the following logical schema: Earlier we believed in $F(x)$ (where x stands for space-time and F denotes some property). Then we discovered that $\neg F(x)$ but $G(x)$ (where G denotes some other property).
>
> Contrary to this common view, the main thesis of this paper is ... [that] the only novelty in the special relativistic account of space-time is a terminological proposal to call something else "space-time". In other words: Earlier we believed in $F(x)$. Then we discovered for some $y \neq x$ that $\neg F(y)$ and $G(y)$. Consequently, it still holds that $F(x)$.

In this logical terminology, I do not claim that $\neg G(y)$ but $F(y)$! Let me explain it through the example of electrodynamics. We want to describe a physical entity, the electromagnetic field. Then we decide which features of this entity we describe. In other words, we define empirically some physical quantities. Then we usually give names to them, say 'electric field strength' and 'magnetic induction'. Then we formulate our experiences about these physical quantities in the Maxwell equations. So, first we have to clarify which physical quantities are called "space" and "time" tags. It turns out from the context of the cited passage of the referee that we are talking about—using my precise (although "cumbersome") terminology—the $\left(\widetilde{x}_1,\widetilde{x}_2,\widetilde{x}_3,\widetilde{t}\right)$-map of the world. As you can see at the end of page 6, I do not claim that the $\left(\widetilde{x}_1,\widetilde{x}_2,\widetilde{x}_3,\widetilde{t}\right)$-map of the world is Newtonian and not Minkowskian. And classical physics does not claim that the $\left(\widetilde{x}_1,\widetilde{x}_2,\widetilde{x}_3,\widetilde{t}\right)$-map of the world is Newtonian and not Minkowskian, either. Moreover, relativity theory, on the other hand, does not claim that the (x_1,x_2,x_3,t)-map of the world is Minkowskian and not Newtonian.

2. Both referees seem to be confused with the relationship between my claim and Poincare's conventionalist views.

Referee#1 writes:

> I can only interpret this [my above quoted thesis] as a kind of conventionalism: depending on our preferences we may say that the structure of space-time is F, or we may say it is G – modifying other bits of physics and kinematics accordingly so that everything works out.

Again, that would be indeed the case if we had a choice between $F(x)$ and $G(x)$ (or between $F(y)$ and $G(y)$)! More precisely, it would be indeed the case of Poincare's conventionalism if we could make a choice between $F(x)\&Physics_F$ and $G(x)\&Physics_G$. But, as my above thesis claims, we have no such a choice! Neither relativity theory nor classical physics claims $G(x)$ (as well as none of them claims $F(y)$). What both theories claim—in agreement—is that $F(x)$ and $G(y)$.

It seems to me that the referees do not make distinction between Poincare's conventionalist thesis and what Grünbaum calls "trivial semantical conventionalism". The later simply means the semantical freedom we have in the use of the uncommitted signs like "electric displacement", "spin", or "distance" and "time".

Let me give you a familiar example where Poincare's intrinsic conventionalism seems to work. Consider Wheeler's "Charge without charge" solutions of the coupled Einstein–Maxwell equations (see the figure). We can make a choice between

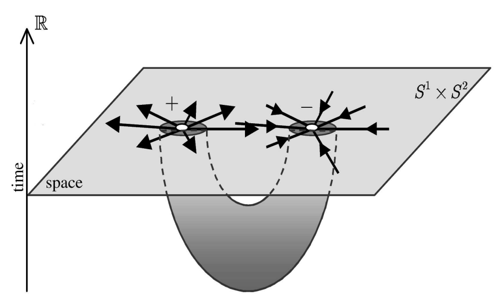


Figure 1: *"Charge without charge". There exist solutions of the coupled* vacuum *Einstein–Maxwell equations in wormhole ($S^1 \times S^2 \times \mathbb{R}$) topology. In these solutions the electric field around a mouth of the wormhole is just like a "Coulomb-field" around a point charge, although there are no sources of the electric field.*

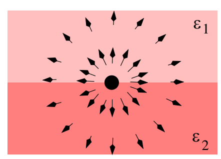
$$\left(\begin{array}{c}\text{the topology of spacetime}\\ \text{is simply connected}\end{array}\right)+\left(\begin{array}{c}\text{physical fields}\\ \text{with charges}\end{array}\right)$$

and

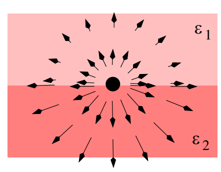
$$\left(\begin{array}{c}\text{the topology of spacetime}\\ \text{is not simply connected}\end{array}\right)+\left(\begin{array}{c}\text{physical fields}\\ \text{without charges}\end{array}\right)$$

But in this case the two theories have two different claims about the topology of the *same* "spacetime" ($F(x)$ and $G(x)$). We do not change the empirical definition of the terms "space" and "time" tags.

On the contrary, imagine that you write a textbook on electrodynamics. One can equally well describe the theory by only using quantities E (electric field strength) and B (magnetic induction), or D (electric displacement) and H (magnetic field strength), or any combinations of them (E,H or D,B)). Imagine that the standard formulation uses E and H (it is quite unusual, but still), called as usually "electric field strength" and "magnetic field strength". Now, imagine that you find it more appropriate to describe electromagnetic field trough D and H, but you don't like the notation D and don't like the impossible historic name "electric displacement". Therefore, at the beginning of your book, you introduce two quantities E and H for the description of the field, and you call them "electric field strength" and "magnetic field strength", but you give the empirical definition of D as the definition of your E. The consequence will be that many of your equations will be different from the corresponding equations of the standard textbook. For example, if you solve what the electric field strength of a charged particle is like on the border of two dielectric materials, you will show the following figure



instead of the similar figure of the standard textbook:



Does it mean that you wrote a book about a new physical discovery, according to which the behavior of electromagnetic field is different from what we believed before? Certainly not!

Does it mean that you discovered at least an alternative electrodynamical theory in the sense of Poincare's conventionalism? (or in the sense of the general underdeterminacy thesis) Certainly not!

It simply means that you use the term "electric field strength" differently, by virtue of your trivial semantic freedom.

Now, this is exactly the case with respect to special relativity theory. We just introduced new physical quantities as new variables for the same physical description of the same things, and started to use the same names "space" and "time" for them.

This is the essence of my paper. Unfortunately the referee reports do not even enter into the criticism of this claim.

3. Let me also reflect to some minor points.
Referee#1 writes:

> ...a proper Lorentzian about STR says that the contraction and dilation effects have a real, physical/causal origin (ether wind or something like that), whereas this author wants to deny any such connection.

I do not deny that the contraction and time dilatation are real physical effects and that they can be accommodated in the causal order of the physical world. As I emphasize it in Comments 2 and 3 on page 8, these are real physical effects not only in the Lorentz theory but also in relativity theory.

4. Both referees put words in my mouth. For example,

True Frame
’naively’ revealed by our clocks and rods
just arbitrarily bestows the name ’space-time’
faulty clock synchronization
’true’ Newtonian geometry
natural/unnatural character of classical/relativistic definition of space-time

I don’t use these phrases. Moreover, such a phraseology definitely contradicts to the basic logical structure of my paper. Following the above quoted logical notations, I do not make any such valuation of x and y. What I do claim is that $x \neq y$ and that both classical physics and relativity theory agree that $F(x)$ and $G(y)$. Whether you call x or y ’spacetime’ is a matter of your (metaphysical) taste. Whether you find the (x_1, x_2, x_3, t)-language or the $\left(\widetilde{x}_1, \widetilde{x}_2, \widetilde{x}_3, \widetilde{t}\right)$-language more convenient is a matter of your aims and practice. But, you cannot say that the switch from claiming $F(x)\&G(y)$ to claiming $F(x)\&G(y)$ is a scientific discovery.