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Essay review

There's no pain in the FitzGerald contraction, is there?

Alberto A. Martínez

Department of History, University of Texas at Austin, 1 University Station B7000, Austin, TX 78712-0220, USA

Physical Relativity: Space–Time Structure from a Dynamical Perspective. Brown, Harvey R. Oxford University Press, Oxford, ISBN 0199275831, 2005, $55,00, hardback, 240 pp.

1. Maverick voices on special relativity

It is well-known that Albert Einstein distinguished between theories of principle, such as thermodynamics, and constructive theories, such as statistical mechanics. It is somewhat less known that Einstein preferred constructive theories, and hence that his special theory of relativity seemed preliminary because its elements: clocks, rigid bodies, electromagnetic signals, etc. were presupposed rather than constructed from elementary constituents. Hence, questions remained: can special relativity be reformulated as a constructive theory? Can the effects of relativistic kinematics be explained in terms of microphysical interactions?

In *Physical Relativity*, Harvey R. Brown boldly elucidates a mainly constructive account of special relativity. His project builds upon the works of earlier "trailblazers" and "unconventional voices." Foremost among them is FitzGerald, who in 1889 proposed that moving bodies deform, to account for the null results of Michelson's interferometer experiment. Contrary to most histories of relativity, Brown reasonably argues, as he has before (2001), that actually there is no evidence that FitzGerald expected *only* a contraction along a body's direction of motion. Indeed, his prominent contemporaries, such as Oliver Lodge and H. A. Lorentz, did not present the hypothesis in that way either; since they acknowledged that a transverse axis of the body could well expand slightly to contribute also to Michelson's net result. What appeals to Brown about FitzGerald's account is that it involved, from the start, the expectation that the deformation might be

E-mail address: almartinez@mail.utexas.edu.

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caused by alterations in the molecular forces within the moving body. Brown entirely rejects the original justification: that such alterations were caused by an ether. He also rejects the more recent notion that such alterations are caused as bodies conform to a space–time substratum. Instead he argues that the dynamical equilibrium and cohesion of the constituents of any body are altered by accelerations *and remain altered during inertial motion*. And Brown further expects that other so-called kinematical effects in special relativity are actually dynamical as well.

Brown revives the words of several unconventional voices that sought dynamical explanations. In 1921, the young Pauli described relativistic length contraction as a complicated process dependent on unknown laws of cohesion. Eddington, at least in 1928, also expected that inertially moving bodies should naturally contract, being a swarm of particles held in delicate electromagnetic balance. Then in 1930 and 1941, Swann stressed that the Lorentz covariance of Maxwell's equations was insufficient to account for length contraction; that quantum theory is needed to explain variations in the cohesion of matter, which cause the contraction. Later, in 1971, Lajos Jánossy argued that material systems that are carefully accelerated become contracted, since the equilibrium of any atomic arrangements is thus affected. He argued that length contraction has "nothing to do with the structure of space and time" (Jánossy, 1971, p. 13), that it is a dynamical consequence of accelerating a body. And likewise, in 1976, Bell apparently demonstrated that when a nucleus is gently accelerated the orbit of its electron becomes length-contracted by the usual relativistic factor, without even using transformation equations. Bell argued that students should approach special relativity by studying how the detailed dynamics of a system entail the FitzGerald contraction effects.

Likewise, Brown now argues that since clocks and rulers are just "moving atomic configurations," as Einstein himself described them (Einstein, 1949, p. 59), therefore, some appeal to quantum theory must be made in order to explain their relativistic behaviors. He claims that hence the special theory is incomplete unless we add "the assumption that the quantum theory of *each* of the fundamental non-gravitational interactions—and not just electrodynamics—is Lorentz covariant" (Brown, 2005, p. 5). He attributes this lesson to Swann in 1912. But this claim seems exaggerated, inasmuch as special relativity, already since 1905, was not meant to apply inertial Lorentz covariance only to electrodynamics but to any laws of physics in general, including molecular and atomic mechanics. In any case, Brown pursues the notion that physical theory should begin from the expectation that quantum dynamics are Lorentz covariant, such that the relativity principle is a consequence of that covariance. He thus calls it a dynamical principle. And, the kinematic universality of the behavior of rods and clocks is then also a consequence of quantum dynamics.

Brown further argues that his dynamical interpretation of special relativity is consistent with the spirit of Einstein's general relativity (and of alternative theories of gravity that preserve the equivalence principle). Einstein had complained that a main defect of special relativity had been the concept of the space–time continuum, which he conceived as acting on bodies without itself being acted upon. He thus criticized the inertial reference systems of special relativity as being no better than the center of the universe in Aristotle's physics (Einstein, 1954). Brown, however, rightly argues that we need not agree with Einstein that space–time geometry in special relativity "acts" on bodies, violating the action–reaction principle. He contends that even in general relativity the space–time manifold is a non-entity, not a substance. Like Rovelli (1997, p. 193), he argues that the metric tensor field

$g_{\mu\nu}$ is not identical to space–time itself but is an independent field, an interactive dynamical agent similar to matter-fields. Space–time geometry is then a manifestation of the omnipresent gravitational field. And inertial motion emerges as a consequence of the dynamics of fields, such that "It is no longer a miracle" (Brown, 2005, p. 163).

Actually, one of the main aims of *Physical Relativity* is to rebut a prevalent view in philosophy of physics: that space–time geometry causes inertial and geodesic motions. I appreciate Brown's desire to not reify space–time geometry as an independent substratum that guides the behaviors of bodies. By what mechanism would such space–time act? Brown further notes that space–time geometry by itself does not explain why all non-gravitational fields couple in the same way to a single gravitational field. Thus, the view voiced by DiSalle (1995) seems fair: that when we say that a body follows a geodesic we are giving a physical definition of the latter, we are not postulating a hidden structure that explains that motion.

2. Kinematic conspiracies and disagreements

By this point let me say that this is a provocative and challenging book, and that I certainly recommend it to philosophers of physics. It expertly synthesizes several analyses and generalizations of the Lorentz transformations. The historical sections are rich, concise and absorbing. Moreover, it includes worthwhile appendices on general covariance and on the relation between special relativity and quantum theory. The lively argumentation throughout thoroughly engages developments from decades of works in philosophy of space and time, and it deserves careful attention. Still, books on the interpretation of special relativity tend to elicit disagreements, and even this one is no exception. Consider first a few minor points.

First, for example, I disagree with Brown's insistent characterization of inertial motion as being essentially a miracle prior to the advent of general relativity. I would not say that bodies in classical mechanics and special relativity "conspire to move in straight lines at uniform speeds while being unable, by *fiat*, to communicate with each other" (Brown, 2005, pp. 14–15). Conspiracies involve two or more parties that *do* communicate to act in unison, so the blind aimless drift of inertia does not fit that bill—yes, aimless: just because some thing has a direction it does not mean that it has an aim, as people have when they go to the doctor, or as bodies do seem to have in the case of gravity. My daily motions may well be similar to Brown's, yet we share no conspiracy. Besides, inertial motions are distinct, having different speeds and directions; how much more different would they have to be to not constitute a conspiracy? It does not sway me that Brown flashes a usual rhetorical device to claim that he's right: "that anyone who is not amazed by this conspiracy has not understood it" (Brown, 2005, p. 15). What behavior could bodies well have which could not be construed so liberally as a conspiracy?

Next, Brown fairly notes that Henri Poincaré understood the conventionality of simultaneity well before Einstein, but then he also claims that Poincaré was the first to understand the relativity of simultaneity (Brown, 2005, p. 63). Yet early on, Poincaré did not claim that simultaneity is relative. Just because moving clocks synchronized by light signals give Lorentz's local time that does not necessarily mean that there can exist no other way of synchronizing clocks which would entail invariant distant simultaneity. Note, for example, that there seems to be no evidence that Poincaré ever acknowledged the existence of time dilation, as Brown rightly points out following Rindler (1970)—but therefore, it would be perfectly conceivable to Poincaré that one might attain invariant distant simultaneity, in principle, by using inertially moving clocks to synchronize distant clocks. By contrast, Einstein did expect that no synchrony procedure would yield invariant simultaneity for distant events along axes of relative motion (Einstein, 1905, p. 897).

As yet another example, Brown claims that in the dynamical approach to length contraction and time dilation "the Lorentz covariance of all the fundamental laws of physics is an unexplained brute fact" (Brown, 2005, p. 143). I object to such language; better call universal Lorentz covariance a postulate instead. But then it sounds as if we are talking again about a theory of principles.

Consider now more general reasons why I don't buy Brown's overall account. He views Einstein's principle theory approach of 1905 as a kind of scaffolding that has outlived its usefulness and which should be taken down to reveal the dynamical structure of the theory in full clarity. Brown sympathizes with Joseph Larmor for having claimed in 1929 that Einstein's 1905 reasoning was ultimately based on dynamical considerations "masquerading in the language of kinematics" (Larmor, 1929, p. 644). Yet why did Einstein's kinematic approach win wide acclaim?

John Stachel has noted that if Einstein had not contributed to electrodynamics then there might not have arisen the consensus distinction between kinematical and dynamical effects, such that length contraction would likely now be interpreted as a dynamical effect caused by motion against the ether (Stachel, 1995, p. 272). But Brown disagrees. He claims that since physicists automatically associate any dynamical account of length contraction with a purported existence of an ether, they fail to acknowledge the dynamical reality of length contraction. Hence, Brown claims that Einstein's attempt to distinguish between kinematical and dynamical effects, both in the organization and terminology of his 1905 paper, was misleading, chimerical (Brown, 2005, p. 145). Since Einstein, years later, criticized special relativity for presupposing clocks and rulers without constructing them from elementary constituents, Brown speculates that Einstein may have found the kinematic section of his relativity paper to be problematic from the beginning. In sum, Brown agues that "the distinction between kinematics and dynamics is not fundamental" (p. 4)—that there is a "true *lack* of a clear distinction between kinematic and dynamic effects (in particular in the context of length contraction and time dilation)" (Brown, 2005, p. 144).

I disagree with Brown's dynamical account of length contraction, and his blurring of the distinction between kinematics and dynamics, which he calls "a false dichotomy." He writes: "What is kinematics? In the present context it is the universal behavior of rods and clocks in motion, as determined by the inertial coordinate transformations" (Brown, 2005, p. 4). I reject this characterization because it does not capture the conceptual reorganization that was involved historically. Strictly speaking, actually, kinematics was the science that aimed to study motions of bodies in our environment as they appear to observation and regardless of the forces that might produce them. That was how Ampère originally defined kinematics (he coined the word) in his *Essai sur la Philosophie des Sciences* of 1834 (pp. 51–52), which Einstein read. Other characterizations arose, for example, as Lord Kelvin, Peter Guthrie Tait, and Heinrich Hertz construed (or misconstrued) kinematics as an *a priori* branch of pure mathematics, independent of experience. Yet the latter was clearly not the way in which Einstein employed the term in 1905, where he stressed the role of observers, measurements, ordinary bodies such as clocks, rulers, and rigid rods, and the description of their motions irrespective of forces.

The conceptual transformation that transpired throughout the 1800s, which in turn facilitated the subsequent reception of special relativity, was that increasingly many physicists realized that if physics is to be essentially based on experience, then it should begin by studying that which is actually measurable rather than from abstractions about invisible agents such as the concept of a self-standing force. Thus, they came to accept Ampère's notion that kinematics should precede statics and dynamics.

Yet for decades it has become common to misrepresent kinematics as just the study of motions where there are no forces. Thus, kinematics easily seems to be utterly fictitious once we readily acknowledge that gravitational interactions and other forces constantly affect actual material systems; leading to the impression that dynamics is fundamental. Yet the primacy of kinematics is epistemological: in order to theorize about invisible molecular or atomic mechanics we should better first study plain kinematics. So the point is not that causes or forces are absent, only that they are not taken into account in kinematics, that they are not represented by independent mathematical magnitudes. Given two moving bodies, we can describe their outlines and trajectories without knowing or referring to their masses nor to the forces that originally set them in motion or that act to move them. For example, can we discuss the motions of the gears in a clock without any use of the notion of force? Yes, we can describe their shapes, linkages, and rates of rotation without any consideration of their weights, mass, material constitution, or the forces that act to move them.

3. So what is length contraction in special relativity?

Now, consider questions about acceleration and cohesion. Suppose we observe a body *A* to rest in space relative to our reference system. Let another body impinge on it, causing it to deform slightly as the force of impact is transmitted throughout, also setting *A* in motion relative to us. Such motion and deformations involve physical causes, yet they may well be described kinematically. Next consider a body *B* resting in free space relative to us, and now let us just begin to glide sideways away from it until we achieve a constant inertial speed. In Newton's framework we say that *B* now seems to move away from us, and we call that a kinematical effect. In Einstein's framework, we say that the body moves away relative to us and that its length is shortened relative to us, and we call those effects kinematical. Einstein expected that the effect is identical to what would transpire if instead *B* were moving away from us at the same rate. For example, if a rocket accelerates near the Earth, and we are inside that rocket, then relative to us the Earth now should have a narrower length. Yet nobody will claim that thus something happened to all the molecules that constitute the Earth. The way we describe their cohesion relative to our rocket may change, but we would not say that there is any material change in the Earthly molecules partly because no such change happened relative to all other systems. By contrast, certain common changes in the configuration of molecules on a body are material changes—which might be observed from any system. Special relativity takes the effects of relative motion as fully reciprocal regardless of which system is regarded as moving. Thus observers on Earth would judge, instead, that the rocket is moving and contracted. Neither contraction is more real than the other, and neither is an optical illusion. Relativity of length means just that any two points on a given body are separated not by one universal length but by indefinitely many lengths.

Yet textbooks usually argue as if it were the *smaller* system, such as the rocket, that is really contracted. Likewise, Brown uses examples in which the body that is accelerated is itself deemed to suffer a subsequent inertial contraction. However, after any such acceleration, for any system in which the body then moves inertially faster there are other systems in which the same body instead moves *slower*, and in which, accordingly, the body exhibits an expansion. Brown writes about bodies that ''in fact'' contract (p. 120); but what about ''in fact'' expand? If we're traveling on a rocket and judge the width of the Earth to be w, and then we change our speed to move slower, we will judge that the Earth has now expanded. To choose any expansion or contraction (by relative inertial translation) as more real than another is arbitrary, it would be like saying that a particular body really does ''in fact'' move to the left. Instead, Einstein's resolve was to claim that there is no one true length of any body, that length is just a relational property.

Furthermore, as I read Brown's book, I kept getting the sense that if we do explain inertial contractions as a byproduct of accelerations then there is an implicit appeal to a notion of absolute rest. Brown even teases us with the ''sobering'' claim that the usual interpretation of quantum mechanics presupposes a kind of privileged frame (relative to which the wave function collapses instantaneously upon a measurement) (Brown, 2005, p. 67). He also claims that we may freely add, for whatever reason, an unobservable privileged reference frame to special relativity. Yet he stops short of actually stating that his account of relativity presupposes such a background frame. He seems content with the idea that such a frame may exist so long as we cannot detect it.

In the end, despite Brown's engaging and insightful arguments, I do not feel compelled to adopt his approach. I well agree that clocks and rods are not primary structures, they should be constructed from more basic elements. But I don't think that it necessarily follows that inertial length-contractions must be explained dynamically. Among the usual alternatives we can still say instead that moving lengths depend on synchrony conventions; e.g., if we synchronize our distant clocks in unusual ways, we *will* measure different lengths of moving bodies, as specified for instance by the Edwards-Winnie transformations (see e.g., Winnie, 1970). (By the way, Brown's book includes excellent discussions on the conventionality of simultaneity.) One might also continue to assume that length is just not an intrinsic property of bodies. Or finally, if special relativistic kinematics seems causally unsatisfying, someone might well formulate another theory where some contractions are real and some are apparent, but I don't think that it would be fitting to then call such a theory special relativity.

Let me close by mentioning two topics that are lacking in the book but would be deserving of future analysis in light of Brown's work. First, despite some occasional claims to the contrary, I think that there is still no experimental test of the reality of inertial length contraction. Such tests are nonetheless conceivable, as illustrated by Einstein's paper of 1911 on the twin moving rods thought-experiment, which in principle could rule against Newton's kinematics or even Einstein's. Also, it would be interesting to trace in more detail the various evolving interpretations of length contraction during the 1900s. Early writers treated it as if it were an effect that would actually be observable, yet ironically, thanks to the development of relativistic optics (as Brown does mention), increasingly more writers have come to characterize the FitzGerald contraction as having little to do with observation. It would thus seem that this contraction is indeed not a part of kinematics proper, but of what has occasionally been called phoronomy, the geometry of motion. But all the same, inertial lengths in special relativity would be no less relative than speeds.

Bodies are composed of atoms, sure, but we do not try to find a dynamical cause of the numerical value of their relative speeds.

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