NOTES AND DISCUSSIONS

Conventions and inertial reference frames

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(Received 10 May 2004; accepted 21 December 2004)

This article discusses the role of conventions in defining the concept of inertial reference frame, and it specifies key historical evidence, up to now widely ignored, connecting Poincaré, Einstein, and Reichenbach's analyses of simultaneity.
[DOI: 10.1119/1.1858446]

In special relativity, conventions have been a subject of debate among physicists and philosophers. We may better understand such disputes on synchrony, velocity, and inertial systems by setting them in relation to specific historical traditions.

Recently, Ohanian argued that dynamical considerations, applied to inertial systems, necessarily entail the standard synchronization rule employed in special relativity.[1] He defines "inertial reference frame" as a frame in which Newton's laws of motion are valid to a first approximation. He cites only one, seemingly authoritative, formulation of this definition. He claims that Einstein, in his famous 1905 paper, required "a system of co-ordinates in which the equations of Newtonian mechanics hold good, i.e., to the first approximation."[2] That expression, however, is not in the 1905 paper. It actually reads, "Consider a coordinate system in which the equations of Newtonian mechanics hold."[3] Ohanian relied on a translation that includes the clause "i.e., to the first approximation" as a footnote.[4] There is no evidence that Einstein added it. Such footnotes originated in an edition of 1913 by Otto Blumenthal, which includes notes by Sommerfeld.[5] Presumably, this specific footnote was added to harmonize the pertinent sentence with special relativity, which revises many equations of Newton's mechanics. Hence, Torretti described Einstein's original expression as "a condition blatantly at odds with the subsequent development of the paper."[6]

At the outset of his argument, Einstein assumed the validity of Newton's mechanics before showing its inadequacy. (This point has been ignored in analyses of the 1905 paper.) In particular, in discussing the lack of any absolute meaning of simultaneity, Einstein assumed that moving clocks keep the time of the "stationary" system.[7] Accordingly, the consideration of a system in which Newton's equations hold served to indicate a deficiency in Newtonian mechanics.

Newton's equations, of course, are valid as a first approximation in Einstein's theory. But, does that mean that we must necessarily define inertial systems in terms of Newton's equations?

Other definitions abound. For example, to define an inertial system in a way that is not predetermined by the adoption of particular laws of motion, we may use an approach formulated by Ludwig Lange. In 1885, he proposed an ideal construct for identifying any "inertial system" of reference.[8] It seems that he actually coined the expression. Lange argued that any three material points simultaneously projected from a single point, and moving freely in noncoplanar directions, constitute an inertial system. He assumed the points' uniform rectilinear motion as a matter of "mere convention." It seemed impossible to ascertain their motion univocally, because they were meant to function as the standard by which to measure other motions.[9] Such a system, though established by convention, would serve to verify whether other objects move rectilinearly at constant velocity.

Lange's definition has served to specify inertial systems in Newton's mechanics, as well as in Einstein's special theory. For example, in his treatise on relativity, Max Laue used Lange's definition.[10] It applies also in schemes developed upon the analysis of special relativity advanced by Hans Reichenbach.[11]

In contradistinction, another tradition defines inertial systems in terms of Newton's dynamics.[12] Arguably, one advantage of that approach is that it need not presuppose the existence of empty space and hence of any "free" particles. Still, Newton's first law of motion refers to free bodies. One of its virtues is that it continued to be exactly valid even in Einstein's special relativity.

The law of inertia by itself does not suffice to necessitate the standard synchrony used in special relativity. Hence, Ohanian suggests that this law be abandoned or construed as a corollary of Newton's second law. Thus, he opposes a tradition (cultivated by Mach, Kirchhoff, Hertz, Einstein, among many others) that begins the analysis of motion by mathematically describing varying distances between bodies rather than by postulating notions of force. Most physicists, following Newton, prefer to present the law of inertia as independent and prior to the force law.

In contrast, Ohanian considers dynamical relations as more real or fundamental than any kinematics that we may devise by various arbitrary modes of representing empirical relations. For him, $F=ma$, when applied to multiple bodies, is "a true law of nature, that is, a prediction that can be confirmed or contradicted by experiment."[13] Notwithstanding a century of apparent confirmations of relativistic deviations from Newton's force law, Ohanian requires that this very "law" is essential in defining an inertial system. He shows that different synchrony conventions would entail a change in the mathematical form of dynamical equations. The resulting equations involve what he calls "pseudo-forces." Yet, such equations do not predict any differences whatsoever in the actual material behavior of physical systems. They constitute an unusual but entirely consistent way of representing and accounting for empirical relations. Ohanian rejects such equations because they don't have the mathematical form of Newton's law. He concludes that "in an inertial reference frame, there is no freedom of choice in the synchronization, and in such a reference frame, a convention for synchronization is not needed or permitted." [14] Yet, his argument depends on adopting a *particular* definition of inertial frame.

Furthermore, he claims that Einstein was "sleepwalking" in positing the standard synchrony procedure. He construes it as "redundant," being involved (allegedly) in the definition of an "inertial reference frame," which he claimed was in the 1905 paper. But, since Einstein did not overtly formulate a definition of inertial frame consonant with his new kinematics, there is no redundancy involved. (By the way, nowhere in the 1905 paper did Einstein use the phrase "inertial reference frame," nor any of these words; he used the expression "system of coordinates.") If Ohanian is correct in connecting standard synchrony inextricably to inertial frames, then Einstein's original formulation is not redundant, precisely because he only specifies the synchrony procedure, and that would tacitly fix the definition of an inertial system. Regardless, Einstein's kinematics admits other common definitions of inertial frame.

Another point to clarify is the rise of the notion that the simultaneity of distant events and the unidirectional speed of any signal involve an element of convention. Such arguments are often traced to the works of Reichenbach in the 1920s.

However, it is well known that since the 1890s the mathematician and physicist Henri Poincaré had highlighted the importance of conventions in mathematics and physics. His concerns grew from questions about the validity of non-Euclidean geometries, as had been the case with Helmholtz. In 1898 Poincaré characterized the metric notion that light has the same speed in opposite directions as a "postulate," a presupposition "which could never be verified directly by experiment." [15] He argued that the simultaneity of distant events and the equality of two durations "can acquire meaning only by convention." [16] For Poincaré, conventions were principles chosen conveniently by all scientists because of their compatibility with empirical knowledge. In that way, the fundamental knowledge secured in mathematics and physics could be understood as not necessarily being fixed by the structure of the world (nor by transcendental categories of the mind, contrary to the philosophy of Kant).

Poincaré argued: we voluntarily ascribe certainty to such conventions because we posit their validity as a matter of definition. For example, he explained, $F=ma$ becomes a convention when we decide that "force" is just the name given to the product of mass times acceleration. The proposition cannot then be contradicted by experiment. Furthermore, he noted that if we adopt an unusual way of measuring time, then "the experiments on which Newton's second law is founded would none the less have the same meaning. Only the enunciation of the law would be different, because it would be translated into another language; it would evidently be much less simple." [17]

Poincaré argued that, to understand the principles of mechanics, it is useful to compare them with alternative formulations and hypotheses. He complained that analysis of the origins and validity of principles is often difficult because textbooks "do not clearly distinguish between what is experience, what is mathematical reasoning, what is convention, and what are hypotheses." [16]

It is also well known that Einstein studied and was influenced by Poincaré's works.[18] At a younger age, Einstein had been immersed in the philosophy of Kant. But, by 1905, owing to his readings of Mach, Poincaré, and especially Hume, Einstein abandoned the idea that notions of space and time are *a priori* knowledge. In his 1905 paper, Einstein acknowledged that different definitions of force are possible. And, he described also as a "*definition*" the requirement that light takes the same time to traverse equal paths in opposite directions. It is also well known that in his popular book on relativity he emphasized that this very requirement "is in reality neither a *supposition nor a hypothesis* about the physical nature of light, but a *stipulation* which I can make of my own free will in order to arrive at a definition of simultaneity." [19]

Among French speakers, the notion that light isotropy is conventional gained prominence because, in the French translation of Einstein's book, the German word "*festsetzung*" ("*stipulation*" in the authorized English translation) was rendered "*convention*." [20] Among English speakers, the notion that isotropy is conventional gained prominence in the writings of Eddington, who early on was reputed to be one of the few physicists who really understood relativity theory. For example, he argued that, "Strictly speaking the Michelson–Morley experiment did not prove directly that the speed of light was constant in all directions. The experiment compared the times of a journey 'there-and-back'." Eddington explained that all such experiments compare only the round trips of signals: "The measured velocity of light is the average to-and-fro velocity... there is a deadlock... which can only be removed by an arbitrary assumption or convention. The convention actually adopted is that (relative to the observer) the velocities of light in the two opposite directions are equal." [21]

Such discussions and many that followed on the conventionality of simultaneity have not mentioned whether Einstein himself actually called simultaneity a "convention." In fact, he did. In a letter of 1924 to André Metz, a philosopher of physics, Einstein noted that relativity theory involved "conventions" and "physical hypotheses." He specified one such convention: "simultaneity." [22] Moreover, in 1918 he had commented to Max Born: "The 'a priori' I must pare down to 'conventional'... ." [23] Even decades later, Einstein defended Poincaré's view that particular geometries are construed as true essentially as a matter of convention.[24]

Reichenbach, another physicist turned philosopher, maintained a friendly correspondence with Einstein. He attributed to Einstein the merit of having properly extended, to the concept of time, the role of conventions that had been elucidated by Helmholtz and Poincaré in physics.[25] In 1928 Reichenbach published his third book analyzing the concepts of space and time in Einstein's theories.[26] He elaborated the thesis that the standard procedure for synchronizing clocks involves an element of convention, in the stipulation that any light rays take equal times to travel equal paths in opposite directions relative to any inertial reference frame.[27] Promptly, Einstein wrote a book review, now virtually unknown. He praised Reichenbach's efforts to clarify how definitions connect concepts and experience: "The clear elucidation of the role of coordinative definitions, especially in the area of relativity theory, is one of the main objectives to which the author has aspired and reached." [28]

Einstein noted that Reichenbach's detailed analysis of time in special relativity was easily intelligible, and he highlighted that "value is located in clearly distinguishing what in the relativistic definition of simultaneity is a logically arbitrary stipulation, and what in it is a hypothesis, that is, a presupposition about the structure of nature." Unfortunately, nowadays, the tendency to not distinguish between conventional and factual aspects of physical theories continues to be strong.[29]

ACKNOWLEDGMENTS

I thank John Stachel and Ronald Anderson for discussions and helpful comments. I also thank Hans Ohanian and Michel Janssen for their useful comments.

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[1]H. C. Ohanian, "The role of dynamics in the synchronization problem," Am. J. Phys. **72**, 141–148 (2004).

[2]See Ref. 1, especially pages 146, 148.

[3]A. Einstein, "Zur Elektrodynamik bewegter Körper," Ann. Phys. (Leipzig) **17**, 891–921 (1905).

[4]A. Einstein, H. A. Lorentz, H. Minkowski, and H. Weyl, *The Principle of Relativity*, translated by W. Perrett and G. B. Jeffery, notes by A. Sommerfeld (1923; Dover, New York, 1952), p. 38.

[5]*Fortschritte der Mathematischen Wissenschaften in Monographien*, No. 2: H. A. Lorentz, A. Einstein, H. Minkowski, Das Relativitätsprinzip, edited by O. Blumenthal, notes by A. Sommerfeld, foreword by Blumenthal (Teubner, Stuttgart, 1913), p. 28.

[6]R. Torretti, *Creative Understanding* (University of Chicago Press, Chicago, 1990), p. 286. He characterized Einstein as having been "careless" on this matter. Still, Torretti (1983) rejected the later footnote, because it only avoids the inconsistency "at a rather high price, for the concept of a stationary frame thereby becomes irretrievably ambiguous." Torretti, *Relativity and Geometry* (Pergamon, Oxford, 1983), p. 294.

[7]Einstein, Ref. 3, p. 896.

[8]L. Lange, "Ueber das Beharrungsgesetz," Ber. der König. Sachsischen Gesellschaft der Wissenschaften zu Leipzig, Math.-Phys. Klasse **37**, 331–351 (1885).

[9]L. Lange, *Die Geschichtliche Entwickelung des Bewegungsbegriffes und Ihr Voraussichtliches Endergebniss* (W. Engelmann, Leipzig, 1886), p. 140.

[10]M. Laue, *Die Relativitätstheorie*, Vol. 1, in *Die Wissenschaft Sammlung von Einzeldarstellungen aus den Gebieten der Naturwissenschaft und Technik*, edited by E. Wiedemann, 3rd ed. (Vieweg, Braunschweig, 1919), Vol. 38. Laue's discussion of Lange's definition of inertial system did not appear in the first (1911) and second (1913) editions.

[11]For example, J. A. Winnie, "Special relativity without one-way velocity assumptions. Part I," Philos. Sci. **37**, 81–99 (1970), "Part II," **37**, 223–238 (1970).

[12]For example, J. Thomson, "On the law of inertia, the principle of chronometry and the principle of absolute clinural rest, and of absolute rotation," R. Soc. Edinburgh Proc. **12**, 568–578 (1884).

[13]Ohanian, personal communication, July 2004.

[14]Ohanian, Ref. 1, p. 143.

[15](a) H. Poincaré, "La mesure du temps," Revue de Métaphysique et de Morale **6**, 1–13 (1898); reissued in (b) Poincaré, *Foundations of Science*, authorized translation by G. B. Halsted (Science, New York, 1913), p. 232.

[16]H. Poincaré, *La Science et l'Hypothèse* (Flammarion, Paris, 1902), p. 111; translated in *Foundations of Science*, Ref. 15b, p. 92.

[17]Poincaré, Ref. 15b, p. 227.

[18]For example, Einstein intensely analyzed *La Science et l'Hypothèse*, recalled M. Solovine; A. Einstein, *Letters to Solovine* (Philosophical Library, New York, 1986), p. 9. Also, in a lecture delivered in 1921, Einstein argued: "It is essential here also to pay strict attention to the relation of experience to our concepts. It seems to me that Poincaré clearly recognized the truth in the account he gave in his book *La Science et l'Hypothèse*." A. Einstein, *The Meaning of Relativity* (Princeton U.P., Princeton, 1946), p. 3.

[19]A. Einstein, *Über die Spezielle und die Allgemeine Relativitätstheorie* (Vieweg, Braunschweig, 1917); Einstein, *Relativity*, translated by R. W. Lawson (P. Smith/H. Holt and Co., New York, 1931), p. 23.

[20]A. Einstein, *La Théorie de la Rélativité Restreinte et Généralisé*, translated by J. Rouvière (Gauthier-Villars, Paris, 1921), p. 19.

[21]A. S. Eddington, *The Mathematical Theory of Relativity* (1923; Cambridge U. P., Cambridge, 1957), p. 19; *The Nature of the Physical World* (1927; Macmillan, New York, 1929), p. 46.

[22]A. Einstein to André Metz, 27 Nov. 1924, Einstein Archive, item control number 18-255. Metz had argued against Einstein's claim that isotropy of light is a convention. Instead, he argued that isotropy is a logically necessary consequence of experiments such as that of Michelson and Morley; see A. Metz, *La Relativité*, 16th ed. (Chiron, Paris, 1923), pp. 21–22.

[23]A. Einstein to M. Born, after 28 June 1918, in *Collected Papers of Albert Einstein, Vol. 8B: Correspondence 1918* (Princeton, Princeton U.P., 1998), edited by R. Schulmann, A. J. Kox, M. Janssen, and J. Illy, p. 818.

[24]A. Einstein, "Reply to Criticisms," in *Albert Einstein: Philosopher-Scientist*, edited by P. A. Schilpp (G. Banta, Menasha, WI, 1949), pp. 676–679.

[25]H. Reichenbach, *Axiomatization of the Theory of Relativity*, translated and edited by M. Reichenbach (1924; University of California Press, Berkeley and Los Angeles, 1969), p. 13.

[26]H. Reichenbach, *Philosophie der Raum-Zeit-Lehre* (de Gruyter, Berlin, 1928).

[27]H. Reichenbach, *The Philosophy of Space and Time*, translated by M. Reichenbach and J. Freund (Dover, New York, 1958), pp. 127–129, 146, 168.

[28]A. Einstein, "Philosophie und Pädagogik," Deutsche Literaturzeitung **20**, No. 1 (1928), p. 19.

[29]Students interested in learning how physicists, especially in the U. S., have sometimes misconstrued postulates and conventions in relativity as statements of experimental facts may consult S. Goldberg, *Understanding Relativity* (Birkhäuser, Boston, 1984).

Comment on "The role of dynamics in the synchronization problem," by Hans C. Ohanian [Am. J. Phys. 72 (2), 141–148 (2004)]

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(Received 8 July 2004; accepted 21 December 2004)

[DOI: 10.1119/1.1858448]

Hans C. Ohanian[1] claims to "defeat" the conventionalist thesis of clock synchronization[2,3] by using an argument based on dynamics. My aim here is to show that his argument does not succeed.

Ohanian writes, "[The conventionalist] thesis rests on the belief that the adoption of the nonstandard synchronization leads to a self-consistent description of physical phenomena, without any demonstrably erroneous experimental conse-

