Talking at cross-purposes: how Einstein and the logical empiricists never agreed on what they were disagreeing about

Marco Giovanelli

Abstract   By inserting the dialogue between Einstein, Schlick and Reichenbach into a wider network of debates about the epistemology of geometry, this paper shows that not only did Einstein and Logical Empiricists come to disagree about the role, principled or provisional, played by rods and clocks in General Relativity, but also that in their lifelong interchange, they never clearly identified the problem they were discussing. Einstein's reflections on geometry can be understood only in the context of his "measuring rod objection" against Weyl. On the contrary, Logical Empiricists, though carefully analyzing the Einstein–Weyl debate, tried to interpret Einstein's epistemology of geometry as a continuation of the Helmholtz–Poincaré debate by other means. The origin of the misunderstanding, it is argued, should be found in the failed appreciation of the difference between a "Helmholtzian" and a "Riemannian" tradition. The epistemological problems raised by General Relativity are extraneous to the first tradition and can only be understood in the context of the latter, the philosophical significance of which, however, still needs to be fully explored.

Keywords   Logical empiricism · Moritz Schlick · Hans Reichenbach · Albert Einstein · Hermann Weyl · Epistemology of geometry

1 Introduction

Mara Beller in her classical *Quantum dialogue* (Beller 1999) famously suggested a "dialogical" approach to the history of science. According to Beller, the scientific discourse always addresses the discourse of other scientists, who in turn may react by changing their views in response. In Beller's view, scientific discourse is not only

M. Giovanelli (✉)
Philosophisches Seminar, Universität Tübingen, Bursagasse, 1, 72070 Tübingen, Germany
e-mail: marco.giovanelli@uni-tuebingen.de

⌂ Springer

dialogical but also "polyphonic". Different dialogues overlap and nurture each other to form a communicative network, the analysis of which is indispensable in understanding the single contributions.

Beller of course provided her own celebrated dialogical analysis of the emergence of the so-called "Copenhagen interpretation" of quantum mechanics. Such a methodological approach, however, could be effectively extended to the history of philosophy of science. In particular there is little doubt that the dialogue about the epistemology of geometry between Albert Einstein and the logical empiricists, mainly Moritz Schlick and Hans Reichenbach, represents one of the decisive moments, if not the decisive moment, of twentieth century philosophy of science (Howard 2009). In this dialogue logical empiricists established the foundation of their new kind of empiricism which, for better or worse, inspired a generation of philosophers.

As we shall see, this dialogue is related to other dialogues: the debate between Einstein, Max Born, Paul Ehrenfest, Max von Laue and others around the 1910s on the notion of rigid bodies in special relativity (Sect. 2.1), and most of all to the debate between Einstein, Herman Weyl and Walter Dellänbach (Sect. 3) on the role of rigid rods and clocks in general relativity (Sect. 2.2) around the 1920s. In turn, these dialogues bear a complex relation to a series of controversies, between Bernhard Riemann, Herman von Helmholtz, Henri Poincaré and others, which burst forth in the second half on nineteenth century after the "discovery" of non-Euclidean geometries (Torretti 1978).

Einstein's dialogue with Schlick and Reichenbach must then be understood against a backdrop of this rich and variegated web of interrelated dialogical interchanges, in which every argument reveals its full meaning only when regarded as a response to other arguments. The aim of this paper is to show that this dialogue, considered in its proper historical setting, can be singled out by a sort of peculiarity: its protagonists never really agreed on what they where discussing and, surprisingly, never seemed to have noticed.

In recent literature the Einstein-logical empiricists debate has been regarded as a philosophical controversy concerning a problem that the "parties to the instrument" univocally identified: whether measuring rods and clocks do or should play an epistemologically fundamental role in general relativity. Einstein, until the end of his life, argued that the theory has empirical content, as soon as one assumes the existence of rigid infinitesimal rods that can be used to measure the interval between any two neighboring points (Stachel 1989). However, he also recognized on several occasions that this was only a sort of practical compromise (Howard 1990, 1994), or at least he was forced by Weyl to recognize it (Ryckman 1995). Both Weyl and the logical empiricists can then be considered as "Einstein's agonists", even if they were speaking up for "different Einsteins" (Ryckman 1996, 2005), or perhaps two different sides of the "same Einstein" (Howard 2005, 2009).

The "received view" can then be roughly summarized as follows: In the 1920s the logical empiricists, especially Schlick and Reichenbach, were convinced they were following Einstein's guidance in assuming that without rods and clocks the "edifice of physics" would be deprived of empirical content (Friedman 2002); however, Einstein, more in consonance with Weyl, accepted this assumption only provisionally:

the behavior of rods and clocks should be in principle derived from the theory and not used to grant its observational basis (Ryckman 2005).

Recent historical literature had thus made enormous progress in revealing the complex "dialogical" background hidden behind the apparently monolithic Einstein-logical empiricists-epistemology of geometry (see Fogel 2008, chaps. 3, 4 for an excellent overview). Einstein's position turned out to be much more sophisticated than the logical empiricists had believed, and most of all rather at odds with their own empiricism. In my opinion, however, recent literature has been strangely recalcitrant in making the successive step to unraveling a rather discomforting truth about the "emperor's clothes". Actually Einstein and the logical empiricists were simply not discussing the *same problem.* The celebrated dialogue to which modern philosophy of science owes its origin was a dialogue of the deaf.

Logical empiricists were convinced that the Helmholtz–Poincaré controversy over the empirical-conventional choice among the class of possible (Euclidean or non-Euclidean) Riemannian geometries could be extended in the new general relativistic context (Friedman 1995), by substituting a stipulation about the congruence of "finite rigid bodies" with that of "infinitesimal rigid rods" (Torretti 1983, p. 239f.). Einstein was rather exclusively concerned with the very existence of rigid infinitesimal rods, which is presupposed by Riemannian geometry, but—as Weyl had shown—is far from being necessary. Einstein's rather sporadic references to the Helmholtz–Poincaré debate served then to meet Weyl's challenge rhetorically in semi-popular writing, rather then make a case for conventionalism (Sects. 4.2, 4.5).

As we shall see in detail, the logical empiricists knew the issue raised by Weyl very well and discussed it at length—in particular Reichenbach—along with Einstein's counter-objection, thereby displaying a vast knowledge of all technical details (Sects. 4.1, 4.3, 4.4, 5); however, they surprisingly never came to realize that Einstein's complex attitude toward the relations between geometry and physics precisely addressed Weyl's criticism of the use of rods and clocks as direct metrical indicators. Even more puzzling is the fact that Einstein, in turn, apparently never felt compelled to clear up the misunderstanding, neither in private correspondence nor in published writings, nurturing the reciprocal illusion that the debate revolved around a well-identified problem to which the contenders gave different solutions.

The historical dialogue between Einstein and the logical empiricists found its crowning moment in the famous imaginary dialogue between "Reichenbach" and "Poincaré" staged by Einstein in his final "Remarks" for the Library of Living Philosopher's volume published in the late 1940s. As Reichenbach's successive reply shows, even on that occasion, Einstein and Reichenbach did not come to an agreement on what the apple of discord was (Sect. 6). For more than thirty years Einstein, Schlick and Reichenbach were engaged in a lively discussion about the epistemology of geometry, but for more than thirty years they were talking at cross-purposes.

If our reconstruction turns out to be correct, we then have to face the ugly truth that the epoch-making debate in which philosophy of science was defined as an independent discipline was based in hindsight on a quite simple misunderstanding. There are still, however, some relevant lessons that can be drawn from an accurate historical reconstruction of this debate. In contrast to what the logical empiricists thought and what it is still often argued in the literature, what we may call the "Helmholtzian"

tradition which traversed nineteenth century geometry did not play any relevant role in the emergence of general relativity, and is utterly inadequate to understand the philosophical problems that theory raised.Einstein's theory drew its conceptual resources exclusively from what we may call a "Riemannian tradition", that, although evolving in parallel during roughly the same years, philosophers have often neglected to adequately investigate (Sect. 7; see Giovanelli 2012a).

2 From finite rigid bodies to infinitesimal rigid rods: Einstein between geometry and reality

2.1 Finite rigid bodies in special relativity

Early on, Einstein insisted on the "epistemological" importance of "rigid bodies" as the mediating element to connect geometry with physical reality. Already in his 1905 paper on the electrodynamics of moving bodies, Einstein pointed out that in a coordinate system at rest the position of a body can be determined "by means of rigid measuring rods using the methods of Euclidean geometry and expressed in Cartesian coordinates"[1] (Einstein 1905, p. 892; tr. Stachel 1998, p. 125). A Cartesian system of co-ordinates can be thought of as a cubical framework formed by rigid unit rods. Translations of such a rigid cube into itself can be used to introduce number triples as coordinates and these can then be employed as marks of position throughout the entire space. To specify relations in time, we require in addition a standard clock (in general a system which runs down periodically) placed, say, at the origin of our Cartesian system of coordinates or frame of reference. In this context, the introduction of coordinates could therefore be directly defined by measurements made by rigid bodies and uniformly ticking clocks (see for instance Einstein 1907, p. 439).

As early as 1907, however, Einstein pointed out that in special relativity it is difficult to clarify the behavior of bodies and clocks in accelerated frames of reference (Einstein 1907, §18; in particular 455). Born (1909), referring to Einstein's (1907) paper, suggested a Lorentz invariant definition of a rigid body (the so called Born rigidity condition). "The method used by me," Born writes, "consists in defining rigidity by a differential law instead of an integral law" [Die Methode, die ich einschlage, besteht darin, die Starrheit statt durch ein Integralgesetz durch ein Differentialgesetz zu definieren] (Born 1909, p. 3); in particular Born defined analytically "the differential conditions of rigidity" [Die Differentialbedingungen der Starrheit] by using a quadratic form of three spatial differentials (Born 1909, §2).

In addition to the paper published in the *Annalen der Physik*, Born presented the results of his work at the meeting of the German Society of Scientists and Physicians (Salzburg, 21–25 September 1909), which was also attended by Einstein. The results were published 1 year later (Born 1910). On that occasion Born mentioned Gustav Herglotz's result that in special relativity a rigid body has only three degrees of freedom, in contrast to the six of classical mechanics (Herglotz 1910; see also Noether 1910).

---

[1] Henceforth I will provide the original German of those passages that I translated myself.

Just four day after the meeting Paul Ehrenfest published in *Physikalische Zeitschrift* a celebrated paper discussing how an ideally rigid cylinder cannot be brought from rest into a state of rotation about its axis of symmetry, without violating Lorentz Invariance (Ehrenfest 1909). The paradox became well known as the "Ehrenfest Paradox" (cf. Grøn 2004).

In private correspondence Einstein declared his interest in "[t]he latest relativity-theoretical investigations of Born and Herglotz … It really seems," he argued, "that in the theory of relativity there does not exist a 'rigid' body with 6 degrees of freedom" (to in Jakob Laub, March 1910, CPAE: 5. 232; tr. in Stachel 1989; now in Stachel 2002, p. 268). In 1911 Max von Laue, developing the works of Born, Ehrenfest, Herglotz and Noether, showed that in special relativity an accelerated body has an infinite amount of degrees of freedom, thus implying that rigid bodies cannot exist. By assuming "that a propagation with superluminal speed is excluded for all physical effects" [daß für alle physikalischen Wirkungen eine Ausbreitung mit mehr als Lichtgeschwindigkeit ausgeschlossen ist] (von Laue 1911, p. 86), Laue showed that an impulse simultaneously acting on n different points of a body will necessarily result in at least n degrees of freedom. Einstein explicitly mentioned a similar result in a discussion following the lecture version of *Die Relativitäts-Theorie* (Zurich, 16 January 1911; published as Einstein 1911): "There can be altogether no rigid body according to the Theory of Relativity" [Ein starrer Körper wird es nach der Relativitätstheorie überhaupt nicht geben können] (CPAE 3, Doc. 17, 443). If we move a part of a rigid body then also the other end should *immediately* move. However, this would be an infinitely fast signal which is not acceptable in special relativity.

Thus, although Einstein did not participate publicly in the discussion of the Ehrenfest Paradox, he was well aware of the difficulties that lurk in the definition of a "rigid body" in special relativity (cf. Sauer 2008). However, the concept of "rigid body" apparently continued to play for him the fundamental "epistemological" role of mediating between abstract geometry and physical geometry: "the propositions of Euclidean geometry," Einstein noticed in his 1912 lectures on special relativity "obtain physical content" [Die Sätze der euklidischen Geometrie bekommen dadurch einen physikalischen Inhalt], if they are interpreted as "propositions concerning arrangements [Gruppierungen] of material straight lines and tracts" in relative rest [Sätze … welche die Gruppierungen materieller Geraden und Strecken betreffen] (CPAE 4, Doc. 1, 36).

In a footnote attached to this passage, Einstein labeled this definition as "woolly (Rotation)" [unscharf (Rotation)] (CPAE 4, Doc. 1, 104; n. 56). The reference is probably to the famous "rigidly rotating disk" thought experiment (Stachel 1989), which Einstein mentioned for the first time in a published paper in February 1912 (Einstein 1912, §1). Einstein came to the conclusion—reached independently by Theodor Kaluza (Kaluza 1910)—that the propositions of Euclidean geometry cannot hold exactly on the rotating disc nor in general within a gravitational field. However, according to Einstein, "[t]he measuring rods as well as the coordinate axes are to be considered as rigid bodies", "even though the rigid body cannot really exist" (Einstein 1912, p. 131; tr. in Stachel 1989; now in Stachel 2002, p. 246)

2.2 Infinitesimal rigid rods and general relativity

As Abrahm Pais has noted, "the celebrated problem of the rigid body in the special theory of relativity stimulated Einstein's step to curved space, later in 1912" (Pais 1982, p. 202). Einstein was forced to rethink the relationship between coordinates and measurements with rods and clocks (Stachel 1989) and to abandon the restrictions imposed by Euclidean geometry; moreover Born's mathematical technique may have led him to consider quadratic differential forms with variable coefficients (Maltese and Orlando 1995). As is well known, around 1912 Einstein was introduced to the work of Erwin Bruno Christoffel (Christoffel 1869), Gregorio Ricci-Curbastro and Levi-Civita (Levi-Civita and Ricci-Curbastro 1900) by his mathematician friend Marcel Grossmann. In 1913, together with Grossmann, Einstein saw how to formulate, at least in outline, a theory of gravitation based on the expression $ds^2 = \sum g_{\mu\nu} dx_\mu dx_\nu$, whose coefficients $g_{\mu\nu}$ represent the behavior of measuring rods and clocks with reference to the coordinate system, as well as the potentials of the gravitational field (Einstein and Grossmann 1913).

In this context, co-ordinates lose their simple metrical significance and do not directly signify lengths measured by a unit measuring rod any more: The length of a measuring rod is not determined solely by the coordinate differentials dx_1, dx_2, dx_3 but also by the six functions $g_{\mu\nu} = g_{\nu\mu}$ where μ, $\nu = 1, 2, 3$. In a four dimensional manifold the measure of time is similarly influenced by the gravitational potential g_{44}, so that the distance of two neighboring events, measured with the aid of a portable clock, is different from the differential dx_4 of the time coordinate: "From this one sees that, for given dx_1, dx_2, dx_3, dx_4," the interval ds which "corresponds to these differentials can be determined only if one knows the quantities $g_{\mu\nu}$ that determine the gravitational field" (Einstein and Grossmann 1913, p. 9). Thus, one does not know the meaning of "distance" between two events, specified initially by physically meaningless co-ordinates x_ν, until one knows the coefficients $g_{\mu\nu}$. On the other hand, after a coordinate system has been introduced, the $g_{\mu\nu}$ should in principle be obtained from the direct measurement of space-like and time-like distances.

In the *Entwurf*-paper, Einstein found the solution to this conundrum by assuming that one can construct a Euclidean or Minkowskian coordinate-system with unit rods and clocks in an small enough (in astronomical proportions) region of space-time. Instead of the general coordinates x_ν, one can introduce the rectangular coordinates ξ_ν, in which $ds^2 = d\xi_1^2 + d\xi_2^2 + d\xi_3^2 + d\xi_4^2$, i.e. special relativity is valid, where the $g_{\mu\nu}$ are constant; the invariant measure of the distance between two space-time points ds, as determined by unit rods and clocks not accelerated in this frame, is referred to by Einstein as "naturally measured" (Einstein and Grossmann 1913, p. 8).

In the last sections of the successive systematic exposition of the *Entwurf*-theory, *Die formale Grundlage der allgemeinen Relativitätstheorie* (Einstein 1914)—presented to the Berlin Academy in 1914—Einstein suggested the implications that the new theory could have on the philosophy of geometry. Traditionally geometry presupposes that "two points of a *'rigid' body* must be separated by a certain distance, which is *independent of the position of the bodies*; the propositions of a geometry supplemented with this statement are (in a physical sense) *either right or wrong*" [zwei Punkte eines "starren" Körpers sollen einen bestimmten von der Lage des Körpers

unabhängigen Abstand realisieren; die Sätze der durch diese Festsetzung ergänzten Geometrie sind (im physikalischen Sinne) entweder zutreffend oder unzutreffend] (Einstein 1914, p. 1079; my emphasis). These propositions are considered as integral laws, since they deal with "finite distances between points" [Abstände zwischen endlich voneinander entfernten Punkten auftraten] (Einstein 1914, 1079). General relativity has forced us to adopt a near-geometrical approach—akin to that of Maxwell in physics—by "gradually introducing the requirement that the *finite distances between points could no longer appear in the elementary laws*" [indem sich allmählich die Forderung durchsetzte, daß in den Elementargesetzen Abstände endlich entfernten Punkte nicht mehr auftreten dürften] (Einstein 1914, p. 1080; my emphasis). Rigid finite bodies are substituted by infinitesimal rigid rods so that the propositions of geometry "are reduced to differential laws" [auf Differentialgesetze reduziert] (Einstein 1914, p. 1080; cf. D'Agostino 2000, p. 235).

After having presented the field equations for determining the $g_{\mu\nu}$ in November 1915 (Einstein 1915c,d,a,b), in §4 of his March 1916 review paper *Die Grundlage der allgemeinen Relativitätstheorie* (Einstein 1916), Einstein returned to the conditions necessary to compare the predictions of the theory with the observed facts. He insisted again on the fact that "ds^2 is a quantity ascertainable by rod-clock measurement of point-events infinitely proximate in space-time, and defined independently of any particular choice of co-ordinates" (Einstein 1916, p. 776). More precisely the $g_{\mu\nu}$ can be found stepwise in the failure to extend the local Minkowski coordinates over larger regions of space-times; they are the numbers to which we have to multiply the coordinate distances in order to assure ds^2 has the same length everywhere. Of course the *same length* can be determined only up to an arbitrary constant; so one still needs to make a global choice of the "units in which we shall express the length of lines" (Lorentz 1917, p. 1345).

2.3 Atoms and clocks

To appreciate the physical meaning of this last remark, it is useful to look at a paper that the Austrian Physicist Ludwig Flamm sent to the *Physikalische Zeitschrift* in September 1916. Illustrating the Schwarzschild solution (Schwarzschild 1916) in a geometrical form, Flamm explains with admirable clarity under which conditions it is possible to perform measurements in general relativity:

> If one chooses as an elementary clock the red line emitted by a cadmium atom and sets its period of oscillation as the time unit, then one recognizes at once that, because of the constancy of the velocity of light, the metrical unit length at every place and in every time must coincide with the same number of wave lengths of the cadmium red line. Similarly, if one set an elementary measuring-rod as the lattice spacing of the rock-salt crystal , one would reach the same conclusion in relation to the "naturally measured" velocity of light. This fundamental presupposition lies at the basis of General Theory of Relativity, that for instance *the ratio between the wave-length of the red cadmium line and the lattice constant of rock salt is an absolute constant*. Also in an arbitrary gravitational field, this constant must be independent of place and orientation and of the time point (Flamm 1916, 451; my emphasis).

[Wählt man als Elementaruhr das die rote Kadmiumlinie aussendende Molekül und setzt deren Schwingungsdauer als Zeiteinheit fest, so erkennt man ohne weiteres, daß sich wegen jener Konstanz der Lichtgeschwindigkeit die metrische Längeneinheit an allen Orten und zu allen Zeiten mit der gleichen Zahl von Wellenlängen der roten Kadmiumlinie decken muß. Legt man als Elementarmaßstab noch die Gitterdistanz des Steinsalzkristalls fest, so kommt man in Hinblick auf die Konstanz der "natürlich gemessenen" Lichtgeschwindigkeit auch hier zum gleichen Schluß. Der allgemeinen Relativitätstheorie liegt also die fundamentale Voraussetzung zugrunde, daß zum Beispiel das ältnis der Wellenlänge der roten Kadmiumlinie zur Gitterkonstante des Steinsalzkristalls eine absolute Konstante ist. Sie muß auch in beliebigem Gravitationsfelde vollkommen unabhängig sein vom Ort, von der Orientierung und dem Zeitpunkt]

General relativity is built on the assumption that ideal clocks are not affected by the presence of a gravitational field. If two cadmium atoms are brought together to the same place, they will always show the same frequency, 6436.4696 Å, for the cadmium red line (Jackson 1936). If we set this rate of ticking as the unit clock, we have then defined a time unit which can easily be reproduced all over the manifold, since all cadmium atoms' red lines will show the same frequency and same rate of ticking. Of course, the particular standard clock used does not matter because the only effect of changing from one clock to another is to change the unit of time with the ratio of the two units being constant. As Flamm points out, the ratio of the wavelength of the red cadmium spectral line over the lattice distance of rock-salt crystal is an absolute constant of nature.

Referring to Flamm's paper, among others, in his lectures on the foundations of physics held in Winter Term 1916–1917, David Hilbert seems to already be clear about the epistemological difficulties which lurk behind this assumption:

In order to read the time one needs a clock. Such a clock can be found realized in nature in the greatest conceivable perfection in an oscillating and light-emitting atom, let's say a yellow spectral line of the sodium atom … The proper time τ of an oscillating atom is independent of the gravitational field, in which the atom is located … This *axiom has* of course … only a *provisional character*. When physics will be finally fully developed, then the axiom must appear as a *consequence* of the general theory (Hilbert 1917, p. 284; last emphasis mine)

[Um die Zeit ablesen zu können, müssen wir eine Uhr haben. Eine solche finden wir in denkbarer Vollkommenheit in der Natur realisiert durch ein schwingendes und Licht emittierendes Molekül, etwa das eine helle gelbe Spektrallinie aussendende Natriummolekül …Die Eigenzeit τ einer Schwingung des Moleküls ist unabhängig von der Stelle des Gravitationsfeldes, an welcher sich das Molekül befindet … Dieses *Axiom hat* natürlich ebenso wie das weiter oben genannte, *nur provisorischen* Charakter. Wenn die neue Physik einmal vollständig ausgebaut sein wird, so muss dasselbe eine Folge der allgemeinen Theorie sein]

In order to perform measurements in general relativity one *must assume* as an axiom that, for instance, the yellow emission line of sodium vapor measured by an observer

at rest with respect to the atom will always be 5893 Å. If we set this as the unit of length, the ticking of such clocks provides a measure of the length of a time-like world-line (proper time). Two identical clocks in different positions within a gravitational field (let's say, on the earth and on the sun) measure, of course, a different elapsed time (first clock effect or twin paradox). As a consequence, the frequency of a signal emitted by one clock would appear to be increased (red shift) or decreased (blue shift) if measured at the position of the other clock (Hilbert 1917, p. 285; cf. Earman and Glymour 1980; Hentschel 1994). However, the rate of ticking of identical clocks, as measured by a nearby observer, is assumed to be independent of the world-line on which the measurements are made. Taking two world-lines of particles, whenever two identical atomic clocks have equal rates on the first line, they measures equal rates on the second world-lines. Such an "axiom" allows the comparison of lengths measured at a distance as Riemannian geometry requires.

At the end of the passage just quoted, Hilbert makes an epistemological point that cannot fade into silence. General relativity assumes this "Riemannian axiom", but it cannot prove it through its own conceptual resources. Thus this assumption has only a *provisional character.* At a later stage of development of physics it should appear, however, as the *consequence* of the theory. Even if it is hard to track down Hilbert's sources, his phrasing reveals a clear similarity with that of Einstein, who, as we shall see, had started at about the same time to attribute to this very issue a fundamental epistemological importance, which contemporary philosophers, alas, have not always been able to appreciate.

3 Schlick's early conventionalism and Einstein's discussion with Weyl and Dällenbach

3.1 Schlick's interpretation of the role of rods and clocks in general relativity

As is well known, Einstein's insistence on the importance of measurement rods and clocks was the starting point of Moritz Schlick's attempt to provide a philosophical interpretation of general relativity, exemplified in his celebrated 1917 paper, published in two installments on the semi-popular journal *Die Naturwissenschaften* (Schlick 1917, tr. in Schlick 1978, vol. I, pp. 207–269). From Schlick's point of view Einstein's approach could be considered the heir to the discussion between Helmholtz and Poincaré on the role of rigid bodies in the epistemology of geometry. In particular, Schlick introduced here the main lines of his conventionalism: the choice of the geometry of space depends on which body one considers as rigid; but it is impossible to know which bodies are rigid without knowing the geometry of space. One breaks the circle by regarding as rigid those bodies from which the simplest physics would result, that is, the physics that contain the least arbitrary elements (Schlick 1917, p. 167).

Of course Schlick was well aware that "the notion of a rigid rod, which preserved the same length at all times, no matter what its position and surroundings might be" (Schlick 1917, p. 182, tr. 1978, I, p. 238), "had already been modified to a certain extent in the Special Theory of Relativity", where "the condition was fulfilled", only for "a rod respectively at rest with regard to each system in question" (Schlick 1917, p. 182,

tr. 1978, I, p. 239). In general relativity "the length of a rod … can also depend on its place and its position", so that the very possibility of measurements was irremediably jeopardized (Schlick 1917, p. 183, tr. 1978, I, p. 244).

Schlick's strategy to find a "δός μοι ποῦ στῶ" (Schlick 1917, p. 183, tr. 1978, I, p. 244) to perform measurements within a general relativistic context was of course to assume, as Einstein did, that the behavior of rigid rods is Euclidean (or Minkowskian in case of rods and clock) in small domains—which, however, "may still be large compared with the dimensions which are used elsewhere in physics" (Schlick 1917, p. 183, tr. 1978, I, p. 245). In small portions of space-time, as Schlick points out, "[t]he 'line-element' has a direct physical meaning, and can be ascertained by means of measuring-scales and clocks" (Schlick 1917, p. 183, tr. 1978, I, p. 245), independently of the coordinate system used. It must then be assumed that the "the value which we there obtain for ds is valid generally" (Schlick 1917, p. 184, tr. 1978, I, p. 249), i.e. the line element ds has the same length under all circumstances: "The numerical value of ds is always the same, whatever orientation the chosen local co-ordinate system may have" (Schlick 1917, p. 183, tr. 1978, I, p. 245).

Schlick offers an accurate reconstruction of Einstein's own theory of measurement; its compatibility with Schlick's own conventionalism is, however, all but obvious. On Schlick's own account, the rigidity of small rods is not stipulated: rigid are those rods and ideal are those clocks that have a pseudo-Euclidean behavior; meaning those that can be disposed to forming a rectangular grid in sufficiently small regions of space-time (Torretti 1983, p. 239f.). One of these measuring devices can be set conventionally as a unit rod, but this has nothing to do with the conventional choice among different possible metrical geometries. Changing the unit of measurement does not change the metric of space but only multiplies all lengths by the same scale factor. In Riemannian geometry, this factor is in fact assumed to be constant, i.e. that ratio of the two units is assumed to be the same whenever they are compared.

Schlick, who had been in correspondence with Einstein since the late 1915 (CPAE 8, Doc. 165), sent him a copy of his paper on the 4th of February, 1917 (CPAE 8, Doc. 296). Einstein replied on the 6th of February, 1917 praising Schlick's paper unconditionally, especially for its empiricist flavor (CPAE 8, Doc. 297; cf. Howard 1984; Hentschel 1986). However, some days later (probably after the 15th of February, 1917), Einstein wrote to his former Zurich student Walter Dällenbach, stressing that, from an epistemological point of view, the idea that the ds could be found directly through a measurement done by rigid rods is not at all satisfying:

> Dear Dällenbach! Your remarks are, in my opinion, to a large extent correct.[2] Strictly speaking, the concept of ds^2 volatilizes in an empty abstraction: ds^2 *cannot be rigorously considered as the result of measurements*, not even in the absence of electromagnetic fields. You have rightly indicated the reasons why it is so. Nevertheless, in a *didactically reasonable presentation of the theory* the ds^2 must be so considered, *as if it were rigorously measurable* … A logically satisfying presentation can be achieved (*a posteriori*), so that the a single, more complex solution is related to the observed facts. A measuring-rod would then

---

[2] Einstein refers to a private conversation.

be an atomic system of a certain type that does not play any special role in the theory (CPAE 8, Doc. 565, 803; my emphasis)

[Lieber Dällenbach! Ihre Ausführungen sind meiner Ansicht nach in hohem Masse berechtigt. Streng genommen verflüchtigt sich auch der Begriff des ds^2 in eine leere Abstraktion, indem ds^2 nicht strenge als Messresultat aufgefasst werden kann, nicht einmal beim Fehlen elektromagnetischer Felder. Die Gründe dafür haben Sie ganz richtig angegeben. Trotzdem wird man bei einer didaktisch vernünftigen Darstellung der Theorie das ds^2 so behandeln, wie wenn es strenge messbar wäre … Eine logisch befriedigendere Darstellung lässt sich dadurch *(a posteriori)* erzielen, dass man die einzelnen komplexeren Lösungen der Theorie mit Beobachtungstatsachen in Beziehung setzt. Ein Massstab würde dann einem Atomsystem von gewisser Art entsprechen, welches in der Theorie keine Sonderstellung beanspruchen könnte.]

A small rigid rod in astronomical proportions, let's say a rock-salt crystal, is a composite structure atomically; its rigidity results from the fixity of the spacing between the chlorine and sodium atoms arranged in a cubic lattice, maintained by electrical forces. Thus, rigid rods cannot be considered as theoretically self-sufficient entities, but their description involves quantum theoretical principles, whose justification, however, lies *outside* the conceptual framework of general relativity. As Einstein admits, the behavior of such an atomic structure, the fact that it preserves its length wherever it is transported, should in principle be justified *within* general relativity, rather than serving to connect it with the observed facts.[3]

3.2 Weyl's theory of electromagnetism and Einstein's measuring-rod objection

As the letter to Dällenbach reveals, Einstein's repeated insistence on the indispensability of measuring rods and clocks, far from representing the chief philosophical implications of the general relativity, as Schlick thought, was then a sort of provisional and "didactic" compromise, a point on which, as we have seen, Hilbert had also insisted at nearly the same time. Dällenbach was at that time Weyl's doctoral student; his objection to Einstein probably reflects the discussions that he might have had with Weyl himself. In the Summer Term of 1917, Weyl gave his famous lectures on relativity at the ETH Zurich, which would appear as a book one year later in 1918 with the title *Raum-Zeit-Materie* (Weyl 1918c).[4]

---

[3] Einstein's 1910–1911 lecture notes on Electricity and Magnetism testifies his early commitment to this epistemological ideal: a physical theory, Einstein remarks, is "a conceptual system whose individual parts do not correspond immediately to experiential facts", but it "is true or false, i.e., corresponding or not corresponding to experience, only *as a whole*" (CPAE 3, Doc. 11, 325; cf. Howard 1994, 91f.)

[4] As Jürgen Ehlers has pointed out, "[a]part from Albert Einstein nobody has contributed more to the conceptual clarification of the general theory of relativity than Hermann Weyl" (Ehlers 1988a, 84f.). Needless to say, no attempt is made to offer an exhaustive overview of Weyl's contributions to the mathematical and philosophical foundations of spacetime theories. One may consult (Bell and Korté 2011, §§4.1, 4.3, 4.4) for a recent detailed account and further bibliographic references.

As is well known, in his presentation, Weyl explicitly tried to avoid the use of exceedingly complicated measuring instruments, such as rods and clocks, for the measurement of g_{ik} by restricting himself exclusively to the observation of the arrival of light signals—for instance, those emitted by two stars—i.e. to measuring the angle between null geodesics ($ds^2 = 0$) at a certain point. In this way, however, the metric can be determined only up to an arbitrary scale factor ($g'_{ik} = \lambda g_{ik}$), which "can be fixed only through the individual choice of a unit of measure" [… kann nur durch individuelle Wahl einer Maßeinheit festgelegt werden] (Weyl 1918c, p. 184). After determining the metric up to a constant by the use of light rays, one can use rods and clocks to determine the constant as well, by arbitrarily choosing the units for space-time distances. The constancy of λ assures the comparability at a distance not only of the angles among curves, but also of their lengths.

In 1918, however, roughly at the time that his lectures went into print, Weyl published *Reine Infinitesimalgeometrie*, a paper in which he famously argued against the necessity of this last element of "geometry at a distance" (Weyl 1918s) which still survives in Riemannian Geometry. All intervals between close pairs of points can thus be changed by the multiplication of an arbitrary factor λ, which may be different in different locations. As is well known, in a successive paper, *Gravitation und Elektrizität* (Weyl 1918b), Weyl suggested that, by removing such a blemish in Riemannian geometry, in addition to the $g_{\mu\nu}$ also the quantities ϕ_ν appear, which could be identified with the four potentials of the electromagnetic field.

As we have seen, in Einstein's theory it is presupposed that if two cadmium atom clocks are separated and brought together along different world-lines, they will still tick at equal rates, but in general will show a "first or usual clock effect"; from this they would then measure different elapsed times, if one of them had passed through a gravitational field. Weyl's theory would introduce a "second clock effect": If two cadmium atoms are separated and brought back, the final size of the atoms might differ, the spectral lines might be shifted, and thus would show different rates of ticking that would serve as an indicator of the electromagnetic field (Weyl 1918b, this topic has been extensively studied in literature; one may consult the following for further details: Vizgin 1994; O'Raifeartaigh and Straumann 2000; Scholz 2004, 2008; Goenner 2004; Ryckman 2005; Bell and Korté 2011).

Einstein was highly impressed by Weyl's theory, but he was unable to settle "the measuring-rod objection [Maßstab-Einwand]" against it, which he communicated personally to Weyl in March 1918 (CPAE 8, Doc. 509 and 512). In a brief note published in an appendices to Weyl's paper (Einstein 1918), Einstein famously argued that if Weyl's theory were true, "chemical elements with spectral-lines of definite frequency could not exist"; rather, "the relative frequency of two neighboring atoms of the same kind would be different in general" (Einstein 1918, p. 40, tr. in O'Raifeartaigh 1997, p. 35). However, this is not the case. The relative constancy of the frequency of atoms is then a plausible factual assumption.

As Weyl pointed out in his rejoinder to Einstein, however, the stability of, let's say, the grating-space of calcite or of the wave-length of cadmium light (periodicity is after all a lattice structure in time) can be learned "only through the implementation of a dynamics based on physical laws" (Weyl 1918a, p. 479; tr. O'Raifeartaigh 1997, p. 35 , slightly modified). It is quantum theory that decides in which conditions such

standards of measure are practically reproducible and in which they are not (e.g. in the case of a strong electric or magnetic field).

"Because of this problematic behavior of measuring rods and clocks", Weyl remarks, "I have relied in my book *Raum-Zeit-Materie* only on the observation of the arrival of light signals [Ankunft von Lichtsignalen] for the principled possibility of measuring [zur prinzipiellen Messung] the g_{ik}" (Weyl 1918a, p. 479; tr. O'Raifeartaigh 1997, p. 35, slightly modified). Relying on a suggestion from Erich Kretschmann (Kretschmann 1917, p. 575), Weyl believed (erroneously as it turned out) that this measuring procedure allows one to determine the absolute values of the g_{ik} at a point (after the choice of a unit of measure). The question of the existence of a second clock effect had to be left open: "in my theory as well as in Einstein's," Weyl insists, "the task remains, to *derive this fact* from a dynamics carried through explicitly" [Es bleibt in meiner wie in der Einsteinschen Theorie die Aufgabe, diese Tatsache aus einer explizite [sic] durchgeführten Dynamik abzuleiten] (Weyl 1918a, p. 479; my emphasis).

Einstein attributed great importance to his prehistory-objection. He clearly felt that Weyl's theory, "[a]part from the agreement with reality" [Abcsehn [sic] von der Übereinstimmung mit der Wirklichkeit] (Einstein to Weyl April 1918, CPAE 8, Doc. 499, 711; tr. in Goenner 2004, §4.1.2 slightly modified), had nevertheless raised a deep epistemological problem: only "if the relative size of rigid bodies does not depend on their prehistory," he wrote to Michele Besso in August 1918, "then there is a measurable distance between two (neighboring) world-points" (CPAE 8, Doc. 604, 859; tr. in Goenner 2004, §4.1.2).

Writing to Einstein (after the 15th of June, 1918), Dällenbach also acknowledged that, if Weyl's theory applied to reality, "one would lose any connection with the observed facts" [verliert man damit jede Beziehung zu beobachtbaren Tatsachen] (CPAE 8, Doc. 565, 803); however, in order to assure this connection, Einstein had assumed overly "complex things [komplexe Dinge]", like rigid bodies or clocks (CPAE 8, Doc. 565, 803), while being unable to explain their behavior. Einstein's reply is interesting throughout:

> If then two ds at distant points were measured with measuring rods or, in a certain way, were found to be equal, they would still be found equal, if they were measured in a different way. This *is a deep property of our world*, which must find expression in the foundation of physics … I know that Weyl does not admit it. He would say that *clocks and measuring-rods must appear as solutions*; they do not occur in the foundation of the theory. But I find: if the ds is measured by a clock (or a measuring-rod), is something independent of prehistory, construction and the material, then this invariant as such must also play a fundamental role in the theory. Yet, if the manner in which nature really behaves would be otherwise, then spectral lines and well-defined chemical elements would not exist. (CPAE 8, Doc. 565, 803; my emphasis; partially translated in Goenner 2004, §4.1.2)

> [Wenn also zwei an endlich distanten Punkten gelegene ds mit einem Massstab bezw. auf eine Art gemessen gleich sind, so sind sie auch anders gemessen gleich. Dies ist eine tiefliegende Eigenschaft unserer Welt … Ich weiss wohl, dass Weyl dies nicht anerkennt. Er würde sagen, Uhren und Massstäbe müssten erst als

Lösungen auftreten; im Fundament der Theorie kommen sie nicht vor. Aber ich finde: Wenn das mit einer Uhr (bezw. einern Massstab) gemessene *ds* ein von der Vorgeschichte, dem Bau und dem Material der Uhr Unabhängiges ist, so muss diese Invariante als solche auch in die Theorie eine ganz fundamentale Rolle spielen. Wenn aber die Art des wirklichen Naturgeschehens nicht so wäre, so gäbe es keine Spektrallinien und keine wohldefinierten chemischen Elemente.]

In other words, Einstein could only acknowledge a *res facti*, that we happen to live in a world in which the relative periods of clocks and the relative lengths of rods do not depend on their histories. Weyl, on the contrary, was raising a *questio iuris* by asking that the behaviors of such complicated material structures, such as rods and clocks, should be considered as a solution to the equations of the theory (in Einstein's as well as in his own theory), that is, it should be deduced from some dynamical theory of matter (cf. Weyl 1919a, p. 113).

Following this line of thought by 1920 (Weyl 1920), at the Meeting of Natural Scientists in Bad Nauheim (19–25 September 1920), Weyl had started to account for the apparent constancy of frequency of atoms by distinguishing between a "tendency of persistence" (*Beharrungstendenz*) or "adjustment" (*Einstellung*) (Weyl 1920, p. 649). Roughly, Weyl suggested that cadmium atoms do not *preserve* their size if transported, but they *adjust* it every time to the radius of the spherical curvature of every three-dimensional section of world (Weyl 1920, p. 650). Similarly, a magnetic needle always adjusts its direction to the north in the earth's magnetic field and does not preserve it.

In the discussion which followed Weyl's paper, Einstein insisted, however, that in his theory "[t]emporal-spatial intervals are physically defined with the help of measuring rods and clocks" (Einstein reply to Weyl 1920, p. 650), whose "equality is empirically independent of their prehistory". This presupposition—he argued—assures "the possibility of coordinating [zuzuordnen] a number, *ds*, to two neighboring points" (Einstein's reply to Weyl 1920, p. 650; tr. from Ryckman 2005, p. 92). Renouncing this "coordination [Zuordnung]" would rob "the theory of its most solid empirical support and possibilities of confirmation" (Einstein's reply to Weyl 1920, p. 650; tr. from Ryckman 2005, p. 92).

These and similar expressions were, of course, music to the logical empiricists' ears. However, Einstein, in the same Bad Nauheim meeting, by addressing other interlocutors, assumed a quite different stance. For instance, when discussing von Laue's paper on the gravitational redshift, Einstein explicitly regarded as "[a] *logical weakness* of the theory of relativity in its current form" the fact that "measuring-rods and clocks have to be introduced *separately*, instead of being constructed as *solutions* of differential equations" [Es ist eine logische Schwäche der Relativitätstheorie in ihrem heutigen Zustande, daß sie Maßstäbe und Uhren gesondert einführen muß, statt sie als Lösungen von Differentialgleichungen konstruieren zu können] (Einstein's reply to Laue 1920, p. 662; my emphasis).

3.3 Weyl and general relativity without rods and clocks

It must be emphasized that Weyl's rejection of the view that the behavior of rods and clocks is not apt to assure the empirical basis of general relativity was independent of

his speculative theory of electromagnetism. As we have mentioned above, in the first 1918 edition of *Raum-Zeit-Materie*, Weyl had already tried to avoid the use of rods and clocks by resorting exclusively to the observation of the arrival of light signals to measure the g_{ik} at a point.

By the third 1919 edition (Weyl 1919b), however, Weyl came to realize that "[b]y observing the arrival of light" in the neighborhood of a certain point, "we can only determine the ratio of the value of the g_{ik}" (Weyl 1919b, p. 195, tr. 1922, p. 228). Two sets of g_{ik} agreeing on light rays may still differ by an arbitrary factor λ which may be different in different locations: "It is impossible … to derive any further results from the phenomenon of the propagation of light" (Weyl 1919b, p. 195; tr. 1922, p. 228)

As Kretschmann had actually already shown (Kretschmann 1917, §11), the constancy of "the factor of proportionality of the λ which could not be derived from the phenomenon of the transmission of light", may be determined by resorting to the motion of "material points moving only under the influence of the gravitational field" (Weyl 1919b, p. 195; tr. 1922, p. 228; on this point see also Lorentz 1923, in particular 26, footnote 1): If two sets of g_{ik} determine not only the same light rays' trajectories, but also the same path of free-falling particles, then those metrics are the same up to a position-independent constant λ (on Kretschmann, also see Giovanelli 2012b).

Weyl made this claim mathematically precise in a letter to Felix Klein (published as Weyl 1921e): If g_{ik} and g'_{ik} are conformally equivalent (i.e. agree on angles between curves at the point of intersection) and projectively equivalent (i.e. agree as to which curves are geodesics) then they can differ by at most a constant proportionality factor λ (for more details cf. Ehlers 1988a; Coleman and Korté 2001, §4.9; Ryckman 2005, §4.2.3; Bell and Korté 2011, §4.3.2).

As Weyl points out, in general relativity "conformal and projective structure have an immediate intuitive meaning" [In der Relativitätstheorie haben projektive und konforme Beschaffenheit eine unmittelbar anschauliche Bedeutung] (Weyl 1921e, p. 100): the conformal structure is explored by experiments with light propagation, the projective structure by experiments with freely falling particles. Thus, according to Weyl, the g_{ik} can be determined univocally within a constant scale factor by resorting solely to the most simple physical objects one can think of, structureless particles and light rays: *"rods and clocks are not required"* [Maßstäbe und Uhren sind nicht dazu erforderlich] (Weyl 1921e, 100f.; my emphasis; for a modern version of this approach see the classical Ehlers et al. 1972, for a philosophical appreciation see Coleman and Korté 1981, 1982, 1984, 1999).

Weyl assumes then that proper time corresponds to the time registered by a clock carried by an unaccellerated or free-falling particle. The constancy of the scale factor "makes it possible to apply the unit of measure along the world-line of the motion" and thus to assign it a length (Weyl 1921d, p. 295; tr. 1921, p. 229). A clock can then be defined by employing only the conceptual resources that belong to general relativity, without any connection to the atomic constitution of matter (cf. Marzke 1959; Kundt and Hoffman 1962; Marzke and Wheeler 1964 for later attempts to rigorously construct "geodesic clocks").

In Einstein's original approach, on the contrary, one starts from the hypothesis that atoms may be used as clocks to keep the proper time along their world lines (cf. Synge 1960, Chap. III, §2), that is, that the frequency of each spectral line of any given

atom is the proper frequency. However, general relativity has nothing to say about the constancy of the number of wave crests emitted by a certain radiating atom. Thus the theory is not able to produce its own means to measure the quantities with which it deals (Marzke and Wheeler 1964; Carrier 1990; Coleman and Korté 1995).

Weyl's method therefore comes much closer to the "holistic" epistemological standard, which Einstein also regarded as methodologically desirable: the physical behavior of light rays and free-falling objects, the devices used to measure the physical geometric field encoded in the g_{ik}, is governed by general relativity itself. There is no need to lean upon the atomic constitution of rods and clocks.

In Weyl's approach, however, two sets of g_{ik} at a distant world-points, although determining the same trajectories of light rays and free falling particles, may still differ by an undetermined multiplicative constant λ and thus assign lengths to curves using different measuring units. In order to assure the "Riemannian" comparability of lengths at distance, one has to resort again to atomic clocks as trustworthy carriers of the unit of length from place to place (cf. Ehlers 1988b).

Thus the "Riemannian postulate"—that the length of lines is independent of their position—could still not be derived from general relativity. Waiting for a theory embracing both gravitational and atomic phenomena, Einstein was ready to accept the postulate pragmatically as a well-confirmed fact. Weyl, as we have seen, tried to resort to "cosmological" speculations concerning a dynamical adjustment of rods and clocks to the 'world curvature' (cf. for insance Weyl 1921b, p. 801).

What is important for the purpose of the present investigation is to emphasize that it was precisely the epistemological status of the "Riemann postulate" that was at the core of Einstein's reflections on the role of rods and clocks in his theory of gravitation. This postulate, as obvious as it may seem, might be wrong, as the very existence of Weyl geometry shows; general relativity cannot justify it by its own means, but, according to Einstein, has to rely on it to grant its empirical basis.

4 The emergence of logical empiricism as a misunderstanding of Einstein's philosophy of geometry

4.1 Reichenbach's objections to Schlick's conventionalism

Unaware of this complex dialogical background, in 1917 Schlick had, after all, very good reasons to emphasize the fundamental role that rods and clocks played in general relativity. As Einstein wrote in a letter to Ernst Cassirer in June 1920: "The destiny of general relativity as a physical theory depends entirely upon the interpretation of the ds as result of measurement, which can be obtained in a quite definite way through measuring-rods and clocks" [Mit der Deutung des ds als Massergebnis, welches mittels Massstäben und Uhren in ganz bestimmter Weise zu gewinnen ist, steht und fällt die allgemeine Relativitäts-Theorie als physikalische Theorie] (CPAE 10, Doc. 44, 293). The theory would remain a purely mathematical exercise if its abstract quantities could not be made to correspond to experimental facts through suitable probes. Schlick could then legitimately interpret Einstein's language of coordination as a confirmation of his own philosophical approach, which in the meantime had been presented in a systematic

form in the first 1918 edition of the *Allgemeine Erkenntnisslehre* (Schlick 1918), a book which, as we shall see, was to have a considerable influence on Einstein himself.

More puzzling is the fact that Schlick could believe that Einstein's insistence on the direct measurability of the *ds* was compatible with a form of conventionalism *à la* Poincaré. The impossibility of carrying out such a program is explained very neatly in Reichenbach's 1920 "Kantian" monograph on relativity, *Relativitätstheorie und Erkenntnis apriori* (Reichenbach 1920)

As is well known, according to the young Reichenbach, who had been one of the five students in Einstein's first seminar on general relativity in Berlin, conventionalism works only for spaces of constant curvature (Friedman 1995). In each of these spaces there is a unique (up to a constant positive factor—i.e. up to the choice of a "unit of length") set of congruence relations so that each such set of congruence relations is inconsistent with any other set. Conventionalism is then based precisely on the idea that one can make an arbitrary choice among one of these incompatible sets.

In Riemannian geometry of variable curvature, no unique set of congruence relations can be defined over all the space, so the very idea of a unique conventional choice among alternative congruent relations does not make sense. For this reason, Reichenbach points out, Poincaré "excludes from the beginning Riemannian geometry, because it does not permit the *displacement of a body* without change of form" (Reichenbach 1920, p. 104, n. 1; tr. 1965, p. 109, 1; translation modified). In the general case only the unit of length is globally available on a Riemannian manifold, in contrast to Weyl's non-Riemannian geometry where a separate unit of length at every point of space may be defined.

What characterizes Einstein–Riemann-geometry then is the possibility of comparing *small measuring rods* at a distance. As Reichenbach points out, "Weyl's generalization of the theory of relativity … abandons altogether the concept of a *definite length for an infinitely small measuring-rod*" (Reichenbach 1920, p. 73; tr. 1965, p. 76), that is, it drops the hypothesis of global availability of the linear unit of measure. Reichenbach correctly observes that "[i]f, for instance, Weyl's generalization should turn out to be correct … [t]hen the comparison of two *small measuring rods* at two different space points would also no longer contain the *objective relation* that it contains in Einstein's theory" (Reichenbach 1920, 87; tr. 1965, p. 91; translation modified).

Reichenbach did not hide his reservations toward Weyl's idea "that mathematics and physics are but one discipline" (Reichenbach 1920, p. 73; tr. 1965, p. 76); however, he had all the elements to understand that Schlick's attempt to save conventionalism by shifting the attention from rigid bodies to infinitesimal rigid rods simply misses the point. In Riemannian geometry, the choice of the unit rod or of the unit clock is of course arbitrary, but the ratio of the units is constant all over the manifold. This is a matter of fact and not of convention.

In the years between 1920 and 1929 Reichenbach played a fundamental role in defending and popularizing Einstein's theories (Hentschel 1982), at the same time acquiring an admirable technical knowledge of their mathematical apparatus. Surprisingly, however, probably under the influence of Schlick, he abandoned his very convincing critique of geometrical conventionalism.

After an opportunity to have a first look at Reichenbach's book, Schlick immediately wrote to Einstein: "Reichenbach does not seem to me to be fair toward

Poincaré's conventionalism" [Reichenbach scheint mir der Konventionslehre von Poincaré gegenüber nicht gerecht zu sein] (Schlick an Einstein, 9.10.1920; CPAE 10, Doc. 171). Writing to Reichenbach some weeks later, Schlick tried to debunk Reichenbach's critique of conventionalism, arguing (erroneously) that Poincaré in his later writings included geometries of variable curvature in his approach (Reichenbach to Schlick 26.11.1920; Schlick and Reichenbach 1920).

Reichenbach answered, agreeing on Schlick's "*and Einstein's point of view*" [und Einsteins Standpunkt] that in principle one could choose between keeping relativity and abandoning Euclidean geometry or *vice versa*: "physics, however, makes the *first* decision … you, and Poincaré, would say for the sake of simplicity … But I have an instinctive refusal against this interpretation" [Die Physik wählt allerdings die *erste* Entscheidung … Sie, und Poincaré, würden sagen, um der Einfachheit halber … Aber ich habe auch gegen diese Deutung eine instinktive Abneigung] (Reichenbach to Schlick 29.11.1920; Schlick and Reichenbach 1920, my emphasis). As is well known, Reichenbach very rapidly overcame his "instinctive disinclination" toward conventionalism, a fact that still puzzles historians (on this point see: Parrini 2005). In the following years Reichenbach would describe Einstein's epistemological achievement precisely as the discovery that would have made it possible in principle for physics to make the *second* decision, that is, to get rid of non-Euclidean geometry by preserving gravitation as a real force.

4.2 Einstein's *Geometrie und Erfahrung*

The publication of the expanded form of Einstein's 1921 lecture *Geometrie und Erfahrung* (Einstein 1921, tr. in 1954, pp. 232–246) probably played a major role in Reichenbach's "conversion". In the lecture, Einstein referred explicitly to Schlick's book on the theory of knowledge (Schlick 1918), which he had read during a journey to Holland in October 1919 (cf. Howard 1984, p. 620).

Following Schlick's method of implicit definitions (Schlick 1918, §7), Einstein distinguishes between "purely axiomatic geometry" and "practical geometry", which "contains affirmations as to the relations of practically-rigid bodies" (Einstein 1921, p. 6; tr. 1954, p. 235). Einstein even claims that, if "this view of geometry" as a "branch of physics", "had not served as a stepping-stone", he "should have been unable to formulate the theory of relativity" (Einstein 1921, 6f.; tr. 1954, p. 235).

Einstein, however, famously recognized that Poincaré was "*sub specie aeterni*" right (Einstein 1921, p. 8; tr. 1954, p. 236) when he denied that the physical behavior of rigid bodies can be used to establish whether the abstract geometry (G) is true or false; in principle it is always possible to make rigid bodies agree with any kind of geometry we please by changing the physical laws (P) that govern their behavior. Only the sum $G + P$, geometry plus physics, can be compared with experience (Einstein 1921, pp. 7–8; tr. 1954, p. 236).

The reference to Poincaré is at first sight quite puzzling. The concept of "rigid body" which lies at the basis of his philosophy of geometry is already quite problematic in special relativity, and becomes completely useless in a space of variable curvature, such as that of general relativity (Friedman 2002). Einstein was of course referring to

"practically infinitesimal rigid rods" [praktisch unendlich kleinen Massstabe] (CPAE 7, Doc. 31, 271).

General relativity presupposes that, if two of these rods "are found to be equal", i.e. have *the same length*, "once and anywhere, they are equal always and everywhere" (Einstein 1921, p. 9; tr. 1954, p. 237). The same assumption must be made for clocks, that "when going at *the same rate*" in some place and time, "they will always go at the same rate, no matter where and when they are again compared with each other" (Einstein 1921, 9f.; tr. 1954, p. 238; my emphasis) The name of Weyl is not explicitly mentioned by Einstein. However, Einstein's argument to support this claim is only a thinly veiled allusion to the "measuring-rod objection" against him: "The *existence of sharp spectral lines*—Einstein writes—is a convincing experimental proof of the above-mentioned principle of practical geometry" (Einstein 1921, p. 9; tr. 1954, p. 238; my emphasis)

Thus the reference to Poincaré in Einstein's *Geometrie und Erfahrung* has nothing to do with the classical problem of choosing among Euclidean and non-Euclidean sorts of Riemannian geometries. On the contrary, *the very nature of Riemannian (or pseudo-Riemannian) geometry is at stake*. The transportability of small measuring rods and atomic clocks—the fact that their relative lengths and rates are independent of their position—"is the ultimate foundation … which enables us to speak with meaning of a metric *in Riemann's sense of the word*" (Einstein 1921, p. 11; tr. 1954, p. 238; my emphasis).

As Einstein noticed in a note to an unpublished article intended for "Nature" (*Grundgedanken und Methoden der Relativitätstheorie, in ihrer Entwicklung dargestellt*, 1920), "this sort of equality" between rods and clocks, "one that is enduring and independent of the pre-history of the motion" [eine dauernde, von der Bewegungsvorgeschichte unabhängige], is "a crucial presupposition of the entire theory" [eine massgebende Voraussetzung der ganzen Theorie] (CPAE 7, Doc. 31, 280; footnote 19). The issue with which Einstein was concerned was then the status of the "Riemann postulate" within general relativity. Einstein regarded it as an *empirical fact* that small rods of equal length will remain so after arbitrary separation. Weyl could however object that such a behavior could not be directly read off from observations, since two equal measuring rods, because of the influence of temperature, external forces etc., will likely not have the same length when reunited; Einstein's alleged fact was no more than an *arbitrary stipulation* (cf. Weyl 1922, p. 52).

With a little good will one can glimpse here some vague resemblance to Poincaré's conventionalist arguments against Helmholtz's empiricism. Poincaré was ready to "save" Euclidean geometry (*G*), by changing the laws of physics (*P*); Weyl claimed that it was always possible to maintain a non-Riemannian "aether-geometry" [Äthergeometrie], by blaming the "body-geometry" [Körpergeometrie] (Weyl 1921a, p. 476)— the empirically observed Riemannian behavior of rods and clocks—on the physical mechanism of "adjustment". But the analogy cannot be pushed too far. One must bear in mind that Weyl was actually a convinced anti-conventionalist (see below on p. 25). Rods and clocks do not "define" the space-time metric, but at most reveal the status of an actually existing metric field or "ether" (as Einstein himself called it Einstein 1920).

The interpretation of *Geometrie und Erfahrung* is then a paradigmatic case where Beller's "dialogical" approach turns out to be indispensable. The text is easily misunderstood if one does not bear in mind that Einstein, in referring to Poincaré, was actually addressing Weyl's epistemological question without mentioning him (cf. Ryckman 2005, §3.5).

It is instructive to pay attention to Einstein's phrasing. According to Einstein, Poincaré was *sub specie aeterni* right for the following reason: he had shown that solid bodies and clocks are not "irreducible elements" but "composite structures", which must "not play any *independent part* in theoretical physics" (Einstein 1921, p. 8; tr. 1954, p. 236; my empahsis); in principle they should be constructed "*theoretically from elementary concepts*". However, according to Einstein, "*in the present stage of development* of theoretical physics" this is not possible, and "these concepts must still be employed as independent concepts" (Einstein 1921, p. 8; tr. 1954, p. 237; my empahsis).

These are of course exactly the alternative attitudes toward the role of rods and clocks in general relativity that Einstein, Weyl and Dällenbach had debated in public writings and in private correspondence some years earlier. Weyl still insisted on this point in a 1921 paper: Einstein's *definition* of the "measure-determination" [Maßbestimmung] "with help from measuring rods and clocks" [mit Hilfe von Maßstäben und Uhren] can be accepted only to assure "a preliminary connection to experience" [eine vorläufige Anknüpfung an die Erfahrung]; in principle, however, the transportability (or non transportability) of rods and clocks should be regarded "as *a consequence* of the developed theory" [eine Folgerung der entwickelten Theorie]. (Weyl 1921c, pp. 562–563; my emphasis). As Weyl put it in the 1921 edition of *Raum-Zeit-Materie*, geometry and physics "form an inseparable unity, something that should be kept in sight always as a whole" [eine unlösbare theoretische Einheit bilden, etwas, das man immer als Ganzes vor Augen haben muß] (Weyl 1921d, p. 60).

In a paper published in the same year—addressing Reichenbach's 1920 critique directly—Weyl seems to regard precisely this point as the major "philosophical" implication of his attempt to unify gravitation and electricity (cf. Rynasiewicz 2005):

From different sides,[5] it has been argued against my theory, that it would attempt to demonstrate in a purely speculative way something *a priori* about matters on which only experience can actually decide. This is a misunderstanding. Of course from the epistemological principle of the relativity of magnitude does not follow that the "tract" displacement through "congruent displacement" is not integrable; from it [the principle of the relativity of magnitude] no *fact* can be derived. It only teaches that the integrability *per se* need not to hold, but, if it occurs [wenn sie stattfindet], it must be understood as the *outflow* [Ausfluß] *of a law of nature* (Weyl 1921a, p. 475; last emphasis mine).

[Von verschiedenen Seiten ist gegen meine Theorie eingewendet worden, daß in ihr aus reiner Spekulation Dinge *a priori* demonstriert würden, über welche nur die Erfahrung entscheiden kann. Das ist ein Mißverständnis. Aus dem erkenntnistheoretischen Prinzip von der Relativität der Größe folgt natürlich nicht, daß

---

[5] The reference is to Reichenbach (1920) and Freundlich (1920).

die Strecken Übertragung durch kongruente Verpflanzung nicht integrabel ist; es folgt aus ihm überkaupt keine Tatsache. Es lehrt nur, daß Integrabilität an sich nicht zu bestehen braucht, sondern, wenn sie stattfindet, als Ausfluß eines Naturgesetzes verstanden werden muß.]

Weyl clears the ground of possible misunderstandings; his theory intended only to show that Einstein's assumption about the existence of rigid infinitesimal rods and ideal clocks is not *logically necessary*, but only a factual claim that general relativity should be able to justify. The mere conceivability of an alternative theory raises the question about the epistemological status of the Riemann postulate. Is it always true that every interval compared with another interval by different routes always has the same length? There is little doubt that Einstein, in his 1921 lecture, was addressing precisely this challenge of Weyl's, recognizing it as inescapable *sub specie aeternitatis*, even if impossible to overcome *sub specie temporis*.

4.3 A parting of the ways: Schlick's and Reichenbach's misunderstanding of Einstein's epistemology of geometry

Isolated from the background of the dialogue with Weyl, *Geometrie und Erfahrung* seemed to confirm Schlick's philosophy of geometry beyond every hope. Not only did Einstein explicitly mention Schlick's 1918 book, but he also referred explicitly to Poincaré, apparently interpreting his own theory in the light of the nineteenth century debate about geometry. Schlick's famous *Erläuterungen* to Helmholtz's writing on geometry (Helmholtz 1921) show how easily Einstein's formula $G + P$ could be integrated with Schlick's own conventionalism. Einstein had somehow reconciled the Helmholtz–Poincaré antithesis with a sort of Hegelian synthesis: one can determine the geometry of space empirically by assuming the conventional definition of rigidity of bodies that leads to the *simplest physics* (see e.g. Helmholtz 1921, Schlcik's note 31).

In his recension of Einstein's *Geometrie und Erfahrung*, published on June, 3th 1921 on *Die Naturwissenschaften*, Schlick seems however to be aware that there was another problem about which Einstein was concerned:

It has been often noticed that there is no simple *compelling* reason to choose some physical bodies as rigid; we could use an arbitrary system of geometry for the description of reality, if only we are ready to change at the same time the system of physics; only the totality, *geometry plus physics*, is determined forcefully by experience. Einstein recognizes the in principle irrefutable validity of this conception; however, just as Poincaré had already admitted that the economy of science forces us to choose, without hesitation, a determinate geometry as the most adequate one (only he still thought it was the Euclidean geometry), similarly Einstein concedes [the following:] in today's state of development of physics, it is inevitable to take as one's basis empirical physical bodies with determinate properties as rigid measuring rods … —namely those bodies which satisfy the condition (which *can be confirmed by the experience*) that two measuring rods are *always* and *overall* equally long, if they *once* and *somewhere* were found as equal (*a condition, which Weyl, as is well known, had tried to drop*). Under this

condition the axioms of practical geometry are pure empirical sentences that can be communicated through observation (Schlick 1921, p. 435; some emphases mine).

[Man hat oft bemerkt, daß es keinen schlechthin *zwingenden* Grund gebe, irgendwelche Naturkörper mit ganz bestimmten Eigenschaften als "starre" auszuwählen; wir könnten ein beliebiges System der Geometrie zur Beschreibung der Wirklichkeit benutzen, wenn wir nur zugleich das System der Physik dazu passend einstellen; nur die Gesamtheit: Geometrie plus Physik wird durch die Erfahrung zwangsläufig bestimmt. Einstein erkennt die prinzipiell unwiderlegliche Richtigkeit dieser Ansicht an; aber wie schon *Poincaré* zugab, daß die Ökonomie der Wissenschaft uns eine ganz bestimmte Geometrie als die geeignetste ohne Schwanken wählen laßt (nur glaubte er noch, es sei die euklidische), so ist es nach *Einstein* beim heutigen Entwicklungsstadium der Physik unabweislich, empirische Naturkörper von bestimmten Eigenschaften als praktisch starre Maßstäbe …—solche nämlich, die der durch Erfahrung prüfbaren Bedingung genügen, daß zwei Maßstäbe *stets* und *überall* gleich lang sind, wenn sie *einmal* und *irgendwo* als gleich befunden wurden (eine Bedingung, die Weyl bekanntlich aufzugeben versucht hat).]

Schlick, of course, could appreciate Einstein's refinement of Poincaré's "economical" procedure of making an in principle arbitrary decision about which rods are rigid. However, by the end of the passage, Schlick also makes a more subtle point. He remarks that such rods must satisfy a "condition" (the ratio of the lengths of two rods is the same whenever they are compared) that it is "confirmed by experience", even if Weyl has "tried to drop it". Thus Schlick was aware of Einstein's measuring rod objection. Surprisingly, however, he did not seem to realize that precisely the epistemological status of this "condition" (whether it is only "confirmed by experience", or if it should be "derived from the theory"), was Einstein's main epistemological concern.

After all, Einstein mentions Schlick's own book at the very beginning of *Geometrie und Erfahrung*, but does not mention the name of Weyl. Schlick could then have the legitimate impression that Einstein was pursuing a similar "Poincaréian" agenda, revolved on the classical problem of the choice among Euclidean and non-Euclidean geometry. This misunderstanding can be appreciated even more clearly if one considers the following passage of Reichenbach's review article on philosophical interpretations of Relativity published in the same year:

[1] We have mentioned above, that the congruence of two tracts can be defined through transportation of a natural measuring-rod; however this is of course only a *definition*. It could also be defined in another way; for instance, a measuring rod after two juxtapositions could be regarded as $\frac{1}{2}$ of its original length, after three $\frac{1}{3}$ etc. One obtains then a Riemannian geometry of different measure-determination. The "change" of the measuring rods can in this case be interpreted as an effect of force, that in this way it is introduced in the definition [of a rigid rod]. Depending on the choice of the field of force, one gets a different geometry. For this reason material objects do not define a single geometry, but a *class of geometries; this is precisely the meaning of conventionalism*. **[2]** However one

must consider, that this class of geometries is in turn not arbitrary, but their validity is based on an axiom, that denotes *an empirical fact*; it is the assumption that two natural measuring rods, which can be brought to superposition once, can be superposed again after they have been transported along different paths. In the validity of this axiom—we want to call it the axiom of the *class of Riemannian geometries*—lies an invariant characterization [Charakterisierung] of the possible geometry according to Einstein (Reichenbach 1921, 365f.; first emphasis of Reichenbach)

[Es wurde oben ausgeführt, daß die Kongruenz zweier Strecken durch Transport eines natürlichen Maßstabs definiert werden kann; aber das ist natürlich nur eine Definition. Man kann auch anders definieren, z. B. einen Maßstab nach zweimaligem Aneinanderlegen $\frac{1}{2}$ so groß nennen, nach dreimaligem Aneinanderlegen $\frac{1}{3}$ so groß usw. Man erhält dann eine Riemannsche Geometrie von anderer Maßbestimmung. Die "Veränderung" des Maßstabs kann man dabei als Wirkung einer Kraft deuten, die auf diese Weise "hinzudefiniert" wird. Je nach der Wahl des zusätzlichen Kraftfeldes erhält man eine andere Geometrie. Es ist also durch die materiellen Gebilde nicht eine einzige Geometrie definiert, sondern eine *Klasse* von Geometrien; das ist ja auch der Sinn des Konventionalismus. Jedoch ist zu beachten, daß diese ganze Klasse von Geometrien doch wieder nicht willkürlich ist, sondern auf der Geltung eines Axioms beruht, das einen empirischen Tatbestand bezeichnet; es ist die Annahme: zwei natürliche Maßstäbe, die sich einmal zur Deckung bringen lassen, lassen sich auch nach dem Transport auf verschiedenen Wegen wieder zur Deckung bringen. In der Geltung dieses Axioms—wir wollen es das Axiom der Riemannklasse nennen—liegt die invariante Charakterisierung der nach Einstein noch möglichen Geometrien.]

This passage is in my opinion extremely significant and deserves a careful reading. It shows precisely the point where Einstein and the logical empiricists came to a misunderstanding:

[1] In the first part of the quote, Reichenbach refers to the classical problem of the choice *among possible Riemannian geometries* with different measure determinations. By arguing that a non-Euclidean geometry is equivalent to a Euclidean geometry with a field of universal forces, Reichenbach shows that the choice is arbitrary. This was the original problem discussed by Helmholtz and Poincaré, and adapted by Schlick in the new general-relativistic context: "According to the theory of relativity, the choice of a geometry is arbitrary; but it is no longer arbitrary once congruence has been defined by means of rigid bodies" (Reichenbach 1921, p. 360; 1978, I, p. 38).

[2] In the second part of the passage, however, Reichenbach makes a different and, for our goals, more important consideration. Reichenbach notices that all Riemannian geometries (Euclidean or non-Euclidean) share a common property: they all presuppose that two measuring rods of the same length remain so if separated and brought together again. As Reichenbach correctly notices, this "axiom"—actually an empirical fact—defines the *entire class of Riemannian geometries*, that is of the "possible geometries according to Einstein".

It is not exaggerated to claim that here we have come to a sort of a parting of the ways: Reichenbach, in subsequent years, will base his philosophical interpretation of general relativity on the conviction that Einstein was mainly concerned with problem [1]. It is not by chance that, still in the 1978 English translation of Reichenbach's paper, the pages on Weyl's theory were simply omitted as "of no historical importance" (Reichenbach 1978, I, p. 38). However, as we have seen, it was precisely problem [2] which Einstein was addressing.

It must be emphasized that Reichenbach understood perfectly well the epistemological implications of Weyl's theory; he simply did not recognize them as the central issue of Einstein's reflections on the role of rods and clocks in general relativity. Reichenbach in fact took back his previous objection "that Weyl wants to *deduce* physics from Reason, after Weyl has cleared up this misunderstanding" [daß Weyl die Physik aus der Vernunft *deduzieren* will … nachdem Weyl dieses Mißverständnis aufgeklärt hat] (Reichenbach 1921, p. 367; the reference is to Weyl 1921a, p. 475; see above on p. 4.2). He had come to realize that Weyl did not want to impose on nature his purely infinitesimal geometry, but rather show that the transportability of rods and clocks—uncritically assumed by Einstein—is actually not logically necessary:

> Weyl took exception to the fact, that Einstein has simply accepted the univocal transportability of natural measuring-rods . He does not want to dispute the axiom of the Riemannian class for natural measuring rods; he wants only to urge that the validity of this axiom, being not *logically* necessary, "is understood as an outflow of a law of nature". I can only agree with this demand; it is the meaning of mathematics, that, by discovering more general possibilities, reveals the *contingency* of certain facts of experience and thus save Physics from simplification. Admittedly, Weyl was able to explain the univocal transportability of natural measuring-rods only in a very incomplete way. But the only fact that he had tried to follow this path, regardless of its empirical correctness, was a genial advance in the philosophical foundation of physics (Reichenbach 1921, 367f.).

> [Weyl stößt sich daran, daß bei Einstein die eindeutige Uebertragbarkeit der Maßstäbe einfach hingenommen wird. Er will nicht das Axiom der Riemannklasse für natürliche Maßstäbe bestreiten, sondern nur fordern, daß die Geltung dieses "Axioms" da es nicht *logisch* notwendig ist, als "Ausfluß eines Naturgesetzes verstanden werde". Ich kann dieser Forderung Weyls nur zustimmen; es ist die Bedeutung der Mathematik, daß sie mit dem Aufdecken allgemeinerer Möglichkeiten die speziellen Tatbestände der Erfahrung als *speziell kennzeichnet* und so die Physik vor Simplizität bewahrt. Freilich gelingt Weyl die Erklärung der eindeutigen Uebertragbarkeit natürlicher Maßstäbe nur sehr unvollkommen. Aber daß Weyl diesen Weg zu gehen versucht, bleibt unabhängig von der empirischen Richtigkeit seiner Theorie ein genialer Vorstoß zur philosophischen Grundlegung der Physik.]

Here, Reichenbach sees the point clearly: Einstein assumed the Riemannian idea of measurement as a simple *fact* (*einfach hingenommen*): the "*univocal transportability*

of natural measuring-rods" is an independent hypothesis which must be *presupposed* to assure the comparability of the predictions of the theory with reality. Weyl, by showing that another class of geometries where this assumption is dropped would be equally possible, had rightly asked for an explanation: the "*univocal transportability of natural measuring-rods*" must be *derived* from the theory—it must be a consequence of it.

Reichenbach provides an admirable presentation of the Weyl-Einstein controversy: Einstein's *de facto* Riemannian assumption vs. Weyl's requirement of its *de iure* justification in front of an equally feasible non Riemannian-alternative. What Reichenbach seems to have missed is the fact that Einstein, by referring to Poincaré in his 1921 lecture, was addressing precisely *this* issue and not supporting a form of conventionalism *à la* Schlick. The original problem discussed by Poincaré, namely the choice *within* the class of possible Riemannian geometries was not at stake at all.

On the contrary, Reichenbach reneges his previous opinion that "the metric … expresses an objective property of reality" and insists that "[t]his conception does not contradict conventionalism", which for Reichenbach is evidently implied in Einstein's formula $G + P$: "Schlick must not be interpreted as saying that a certain metric has been prescribed; a metric emerges only after the physical laws have been established (the P of Einstein's formula). One can also *change the metric, provided one changes the laws of physics correspondingly*" (Reichenbach 1921, p. 356; tr. 1878, I, 34f.; my emphasis).

4.4 Getting off track: the implementation of Reichenbach's conventionalism

Reichenbach's 1922 paper, *La signification philosophique de la théorie de la relativité* (Reichenbach 1922), the first comprehensive presentation of his new conventionalist approach to geometry and relativity, shows clearly that he, in the wake of Schlick, had come to interpret Einstein's reference to Poincaré, so to say, "literally": "The solution to the problem of space is therefore found only in this conception we call conventionalism, which goes back to Helmholtz and Poincaré" (Reichenbach 1922, p. 40; tr. 2006, p. 135). The central problem for Reichenbach is precisely how to choose among different possible Riemannian geometries.

By introducing the idea of a non-detectable force of type X, such as gravitation, which causes uniform shrinkages and expansions in all materials, Reichenbach concludes that the main philosophical lesson of general relativity is the following: "*[t]he definition of congruence is … arbitrary*, and what is congruent in one geometry is not necessarily congruent in another" (Reichenbach 1922, p. 33; tr. 2006, p. 127; my emphasis). It is worth noticing that Reichenbach makes an important remark here, although in passing: "This definition of congruence is arbitrary, *but it is uni-vocal, and it entails that two rigid rods that are congruent at a point remain congruent at all points.* This is an axiom that we can consider to be *experimentally well confirmed*" (Reichenbach 1922, p. 35; tr. 2006, p. 129; my emphasis).

Reichenbach thus addresses the issue again that the univocality of the definition of "congruence" as such is only an experimental, and not a logical, truth. A non-univocal definition of congruence would be in principle legitimate: "This is the path that Weyl

followed with perfect rigor; it cannot be said that *a rod is equal in an absolute sense to one at another place*, but that we can dispose altogether with the arbitrary process of the comparison of rods" (Reichenbach 1922, p. 41; tr. 2006, p. 136).

Reichenbach seems here to confuse different issues. There is actually no *univocal* definition of *congruence* of *bodies* in Riemannian manifolds of variable curvature, as Reichenbach himself had rightly noticed in 1920. In such spaces, in fact, the congruence of *bodies* is not independent of position: two plane figures which are congruent on the equator of an egg-shaped surface cannot be reproduced at the pole. In all Riemannian manifolds, however, there is an arbitrary, but univocal definition of the *unit of length*: two small rods of equal lengths in a place will still have the same length wherever they are compared.

The fact that the univocality of the definition of a unit of length was only *"experimentally well confirmed"* was precisely Einstein's main and only epistemological concern that emerged in the discussion of Weyl. The fact that Reichenbach did not appreciate this point is nothing but stunning. Reichenbach, in fact, explains it with usual accuracy in his more technical 1924 *Axiomatik der relativistischen Raum-Zeit-Lehre* (Reichenbach 1924, tr. 1969).

According to Reichenbach, whereas in special relativity light rays are sufficient for the construction of the metric, in general relativity, as we have seen, using light rays $ds^2 = 0$, one can determine the value of $g_{\mu\nu}$ only *up to a possibly position-dependent scale factor*: "then every metric $g'_{\mu\nu}$ which results from $g_{\mu\nu}$ through multiplication by a scalar field $\lambda(x_1, \ldots, x_4)$ will satisfy these axioms, since in it the lines $ds^2 = 0$ satisfy the same equation" (Reichenbach 1924, p. 120; tr. 1969, p. 151). As Reichenbach notices, if "the light geometry furnishes only the quotient $\frac{g_{\mu\nu}}{g'_{\mu\nu}} = q_{\mu\nu}$", then "[f]or the determination of the *absolute values* of the $g_{\mu\nu}$ in the gravitational field, however, we need material things, either natural clocks or rigid rods" (Reichenbach 1924, p. 122; tr. 1969, p. 170).

Thus Reichenbach shows very clearly that we need rods and clocks only in order to force the parameter λ to be constant:

> One can now imagine the construction of the metric $g_{\mu\nu}$. If any coordinate system K is given for the world, then point events of the distance $ds^2 = +1, ds^2 = 0, ds^2 = -1$ can be produced by means of rigid rods and clocks permanently at rest in K. The world will thus be interspersed with unit mesh points. The functions $g_{\mu\nu}$ are to be determined in such a way that with the chosen coordinates $ds = \pm 1$ or 0, respectively, for all mesh points. If merely the quotients $q_{\mu\nu}$ are to be constructed, light signals alone suffice. … *Only in order to make the unit of length (or the unit of time) of the various systems K' equal are transportable rigid rods (or clocks) needed.* The significance of material things becomes clear: they bring about *a comparison of the units at different points*. This comparison cannot be achieved by means of light signals; for all other purposes light is sufficient (Reichenbach 1924, p. 120; tr. 1969, p. 151; my emphasis)

Thus Reichenbach is completely clear about the fact that only in order to assure the reproducibility of the unit of length we need the rigid rods and clocks. If the ratios of the $g_{\mu\nu}$ is determined by using light signals, then clocks and rods serve to determine

the factor λ. The requirement *that two small rigid rods are congruent at a point and remain congruent at all points* is identical to the assumption that *scale factor* λ is constant.

Reichenbach did not appreciate Weyl's alternative strategy (he refers to Weyl 1921d, appendix I, 285f.), that in general relativity "the path of force-free mass points may take the place of material things [Gebilde]" (Reichenbach 1924, p. 122; tr. 1965, p. 154; footnote) in assuring the constancy of λ (cf. also Lorentz 1923). The definition of a free-point mass is just as circular as the definition of the rigid bodies and ideal clock; Weyl's approach, according to Reichenbach, contains "the same problems epistemologically as the definition by means of rigid bodies and material clocks" (Reichenbach 1924, p. 63; tr. 1965, p. 82, footnote; translation slightly modified; see Ryckman 2005, §4.4.4 for more details; for a recent attempt to avoid this objection see Coleman and Korté 1982).

Leaving aside the irony that Weyl's approach will lead to a much more successful example of a "constructive axiomatic" (Ehlers et al. 1972), Reichenbach seemed to have the relevant issue clearly in mind: "since light determines only the quotients $q_{\mu\nu}$", (Reichenbach 1924, p. 134; tr. 1965, p. 170) one can use rods and clocks, rather than free-particles, to assure that "$ds^2 = 1$ everywhere" (Reichenbach 1924, p. 122; tr. 1965, p. 155), i.e. not only to determine the values of the $g_{\mu\nu}$ up to a constant factor λ, but to determine the scale factor as well.

The issue at debate, then, revolves around a property which characterizes the class of Riemannian geometries as such and says nothing about the possibility of making a choice within this class. As we have seen, the ratio of the length of two small rigid rods or the ratio of the ticking of ideal clocks is an absolute constant in general relativity, and thus it is not subjected to any conventional stipulation. The relevant epistemological problem Einstein was forced to consider after his debate with Weyl was precisely that of the status of such a transportability of rods and clocks in general relativity. Again one has only to follow Reichenbach's exposition:

> The word "adjustment" used by Weyl for the first time in this context character-izes the problem very well. *It cannot be an accident* that two measuring rods are equal at every place in a neighborhood comparison; *this fact must be explained* as an adjustment to the field in which the measuring rods are embedded like test bodies. Just as the compass needle adjusts itself to the magnetic field of its immediate environment, even though merely in its direction, so measuring rods and clocks adjust their *unit lengths* to the *metrical field*. The metrical relations between material objects must be explained in this fashion. The answer can be given only by *a detailed theory of matter* of which nothing is known until now. (Reichenbach 1924, p. 64; tr. 1969, p. 91; my emphasis)

Here, once again, Reichenbach explains the vexed question eloquently: "*it cannot be an accident that two measuring rods are equal at every place in a neighborhood comparison*"; Weyl had therefore required "*this fact must be explained*". Precisely like Einstein, however, Reichenbach claims that we do not have a "*detailed theory of matter*" from which one can deduce such a behavior of our rods. It is thus almost incomprehensible that Reichenbach failed once again to realize that Einstein, in his 1921 lecture, was addressing *this* issue exactly—the justification of the "Riemannian"

no-second-clock-effect requirement and its analog for rods—and not renewing the empiricism/conventionalism debate between Helmholtz and Poincaré.

Weyl wrote a rather negative review of Reichenbach's book (Weyl 1924b), by pointing out, aside from an overall cumbersomeness in the presentation, some technical inaccuracies (cf. Rynasiewicz 2005). Reichenbach reacted indignantly, arguing that Weyl wrongly misread the book as a mathematical treatise instead of a philosophical investigation (Reichenbach 1925). However, by reading between the lines, one does not fail to see that the disagreement between Weyl and Reichenbach (Coffa 1979; Ryckman 1995, 1996, 2005) was precisely of a philosophical nature.

Reading Einstein's insistence on the indispensability of rods and clocks against the background of the nineteenth century debate on the foundation of geometry, Reichenbach reached the well known, but quite puzzling conclusion that the transition from the special to the general theory has to be interpreted as "a *renunciation of metrical characteristics*" (Reichenbach 1924, p. 155; tr. 1965, p. 195; my emphasis). The philosophical novelty of general relativity is the discovery that rods and clocks *define* the space-time metric which, so to say, does not exist before a stipulation about their rigidity has been introduced.

As is well known, for Weyl the main achievement of Einstein's theory of gravitation had to be sought instead in the transformation of the fixed metric structure of the special theory in a metric-cum-gravitational field, which is causally dependent on the state of matter. Thus the behavior of rigid bodies and clocks merely *reveals* an already determined physical-geometrical field, which is just as real as the electromagnetic field (cf. e.g. Weyl 1924a; on this topic see Coleman and Korté 1995; Bell and Korté 2011, §4.3.1).

4.5 Einstein's reflections on philosophy of geometry between 1923 and 1926

As Schlick's 1925 second edition of his *Allgemeine Erkenntnislehre* confirms, the reading of Einstein's *Geometrie und Erfharung* on the background of the Helmholtz–Poincaré controversy prevailed among logical empiricists (Schlick 1925, p. 326). Einstein's reflections on geometry published at about the same time, however, clearly suggest that Einstein's reference to nineteenth century conventionalism cannot be interpreted literally, but have to be inserted in its proper "dialogical" context. Even if his name is seldom explicitly mentioned, Einstein's phrase choice does not leave much room to doubt that Weyl was his real interlocutor. Einstein's main concern remained that of justifying his pragmatic attitude toward the "Riemannian" behavior of rods and clocks in spite of his acceptance in principle of Weyl's stricter epistemological standard.

In his delayed lecture for his 1921–1922 Nobel prize delivered to the Nordic Assembly of Naturalists at Gothenburg in July 1923 Einstein had famously insisted that, as composite atomic systems, rigid bodies cannot be used to verify the laws of nature, for these very same laws should in principle account for their rigidity (D'Agostino 2000, 241f.). According to Einstein, "it would be *logically more correct* to begin with the *whole of the laws*" (Einstein 1923, p. 3; my emphasis) and not with an "artificially isolated part" such as rods and clocks (Einstein 1923, p. 3; my emphasis).

In a 1924 recension (Einstein 1924b) of a book by a minor Neo-kantian, Adolf Elsbach (Elsbach 1924), to which attention has been drawn recently (Howard 1990, 2010), Einstein distinguishes two different "standpoints" on the question of "whether one grants reality to the practically-rigid body": according to *Standpoint A*, geometry "contains assertions about possible experiments"; on the contrary, according to *Standpoint B*, "only geometry with physical sciences taken together" can be compared with experience (Einstein 1924b, 1690f.). It is not hard to see here once again the opposition between Einstein's and Weyl's epistemological stance.

Even more important, in a paper published one year later, *Nichteuklidische Geometrie und Physik* (Einstein 1925), Einstein expressly attributed standpoint *A* to Poincaré, whereas standpoint *B* to Helmholtz:

> In order to see the matter clearly, one must consistently adopt one of *two points of view*. [*A*] In the first, one holds that the "body" of geometry is realized in principle by rigid bodies in nature, provided that certain conditions are met regarding temperature, mechanical strain, etc.; this is the point of view of the practical physicist. In this case, the "distance" of geometry agrees with a natural object and thereby all propositions of geometry gain the character of assertions about real bodies. This point of view was especially clearly advocated by *Helmholtz*, and we can add that without him the formulation of relativity theory would have been practically impossible. [*B*] In the other point of view, one denies in principle the existence of objects that agree with the fundamental concepts of geometry. Then geometry by itself would include no assertions about objects of reality, only geometry taken together with physics. This point of view, which may more complete for the systematic representation of a finished physics, was expounded particularly clearly by *Poincaré*, from this standpoint, the entire content of geometry is conventional; which geometry is preferable depends on how "simple" physics can be made by using geometry to agree with experience (Einstein 1925, p. 253, tr. in Pesic 2007, p. 161)

Again Einstein is evidently not referring to finite rigid bodies here; in a space of variable curvature "the possible contact-displacements of practically rigid bodies would be different in different cases, conditioned by the influence of the circumstances" [die möglichen Berührungslagerungen von praktisch starren Körpern [wären] in verschiedenen Fällen verschieden, von Milieu-Einflüssen bedingt] (Einstein 1924a, p. 86); there is no unique set of congruence relations, so that it would not make sense to decide once and for all which tiles of space are congruent, as Helmholtz's and Poincaré's philosophy of geometry would require. Einstein is referring, of course, to the "the possible disposition of infinitely many, infinitely small rigid bodies" (Einstein 1925, p. 253, tr. in Pesic 2007, p. 161). The local Euclidean behavior of such rods cannot be extended "over finite regions" of space (Einstein 1925, p. 253, tr. in Pesic 2007, p. 161), at least if we attribute the same length to one of such rods in all positions and in every orientation.

Einstein's reference to Helmholtz and Poincaré, then, is nothing more then a "rhetorical device" which actually serves to address Weyl's challenge. Einstein adopted the "Helmholtzian" standpoint *A*, accepting provisionally as an independent *fact* that there are rigid infinitesimal rods, whose length would be the same under all circumstances.

Weyl followed a "Poincaréan" standpoint *B*, by arguing that such a behavior of infinitesimal rods cannot be observed, but it is at most a convenient *stipulation*.

The problem with which Einstein was concerned was then completely detached from the historical dialogue between Helmholtz and Poincaré, whose names are used as mere labels to identify two abstract philosophical positions: "*consistent thinkers* — as Einstein put it in the entry "space-time" for the 13th 1926 edition of the *Encyclopedia Britannica*—considered it preferable to allow the content of experience [Erfahrungsbestände] to correspond to geometry and physics conjointly" (Einstein 1926, p. 609). Einstein, however, is still convinced that "it would not be advisable to give up the first view, from which geometry derives its origin"—i.e. the interpretation of geometry as science of the behavior of rigid bodies —, "an abstraction that is well rooted in the laws of nature" (Einstein 1926, p. 609).

Logical empiricists not only failed to appreciate that Einstein supported standpoint *A* only provisionally—as it is usually claimed in literature. They never understood the very problem to which the alternative referred: the reproducibility vs. "non-reproducibility of the gauge-unit [Nichtreproduzierbarkeit der Eicheinheit]", as Fritz London put it (London 1927, p. 187). Weyl's theory at that time was losing most of its convincing power. As is well known, however, London's suggestion to replace the scale factor in the original theory with a phase factor would make Weyl's "gauge invariance" one of the central feature of twentieth century physics (Yang 1986; O'Raifeartaigh and Straumann 2000).

5 Reichenbach's relativity of geometry

Reichenbach was of course completely aware of the problem that "non-reproducibility of the gauge-unit" was a relevant issue. He discussed it again as early as in §4 of his classical semi-popular *Philosophie der Raum-Zeit-Lehre*, which he had already finished in 1926 (but will be published as Reichenbach 1928, tr. 1958). As is well known, according to Reichenbach, whereas the definition of the concept of the unit of length is a *conceptual definition*, the choice of the interval that serves as a unit of length is a *coordinative definition*—a definition which coordinates an abstract concept with a "piece of reality", such as the meter standard in Paris (cfr. Shapiro 1994). Reichenbach, however, adds this significant consideration:

> After this solution of the problem of the unit of length, the next step leads to *the comparison of two units of lengths at different locations*. … Assume two measuring rods which are equal in length. They are transported by different paths to a distant place; there again they are laid down side by side and found equal in length … it is an *observational fact* [beobachtbare Tatsache], formulated in an *empirical statement* [Erfahrungssatz], that two measuring rods which are shown to be equal in length by local comparison made at a certain space point will be found equal in length by local comparison at every other space point, whether they have been transported along the same or different paths … *The physical fact makes the convention univocal* (eindeutig)*, i.e., independent of the path of transportation.* The statement about the *univocalness* (Eindeutigkeit) *of the convention* is therefore empirically verifiable and *not a matter of choice*. One

can say that the factual relations holding for a local comparison of rods, though they do not require the definition of congruence in terms of transported rods, make this definition admissible. Definitions that are not unique are inadmissible in a scientific system (Reichenbach 1928, 24f.; tr. 1958, p. 17; my emphasis; trasnlation slightly modified).

Reichenbach is then aware that it is only "*a matter of fact* that our world" admits a "univocal" definition of the unit of measure (Reichenbach 1928, p. 25; tr. 1958, p. 17); it is only for the "*factual relations holding for the behavior of rigid rods*" (Reichenbach 1928, p. 27; tr. 1958, p. 17). The choice of a rod as the standard unit of length is arbitrary, is a *matter of definition* or convention, but it is *a matter of fact* that two of such unit rods are equal everywhere when compared. Reichenbach, like Einstein, accepted this "fact" and somehow elevated it as a very condition for the possibility of a "coordinative definition" (Reichenbach 1928, p. 27; tr. 1958, p. 17).

However, the question of the univocality of the definition of the unit of measure is clearly not the central problem for Reichenbach. Rather, he moves quite abruptly to the question of the definition of the "congruence of bodies" (Reichenbach 1928, §5), which is at the basis of his famous "relativity of geometry" (Reichenbach 1928, §8). Reichenbach, as is well known, claims that the relativity of geometry "is essentially the result of the work of Riemann, Helmholtz, and Poincaré" (Reichenbach 1928, p. 48; tr. 1958, p. 35), who—in Reichenbach's view—first recognized that the geometry of space, whether Euclidean or non-Euclidean, rests on a conventional definition of which bodies are rigid. Einstein simply applied this approach to physics.

Of course, as Reichenbach himself noticed, even if only by the end of the book, that "there are no rigid bodies" in general relativistic space-time; the concept of rigidity "loses its definiteness in fields in which the adjustment of measuring instruments is not uniform" (Reichenbach 1928, p. 302; tr. 1958, p. 264). Reichenbach, however, believed that he still had an "ace in the hole"; in general relativity the coordinative definition does not concern finite bodies, but rather "*infinitesimal measuring instruments*" (Reichenbach 1928, p. 302; tr. 1958, p. 264; my emphasis). As it has been pointed out (Torretti 1983, 239f.), however, Reichenbach's approach is hardly compatible with his conventionalism, as it paradoxically emerged from his own account.

Reichenbach explains with the usual clarity that in order to perform measurements, we "carry around infinitesimal measuring rods" (Reichenbach 1928, p. 285; tr. 1958, p. 249), which we assume to be unit rods. Riemann's geometry assumes that the laws of disposition of such rods more closely approach those of Euclidean geometry the smaller the dimensions of the considered region of space become. Rods are progressively more rigid to the extent that they are smaller. If our rods do not show such a Euclidean behavior, as Reichenbach observes, "we would assume, therefore, that the unit rod was not chosen sufficiently small and would claim that the axiom would be satisfied if the rod were shortened" (Reichenbach 1928, p. 286; tr. 1958, p. 250). So the rigidity of rods is evidently not stipulated arbitrarily, but checked under the presupposition that space is Euclidean in small domains (Torretti 1983, 239f.).

After having determined a rigid unit rod according to this procedure, the geometry of space around a point can then be found empirically, under the presupposition "that ds^2 be equal to 1 for the *same* $g_{\mu\nu}$ and any direction of the rod rotated in P the $g_{\mu\nu}$ will

be determined uniquely [eindeutig]" (Reichenbach 1928, p. 285; tr. 1958, p. 249) (up to a coordinate transformation). If in small regions of space $g_{\mu\nu}$ are necessarily constant, over larger regions of space, we might discover that, by combining the coordinate differentials according the Euclidean formula, "the resulting ds^2 is by no means equal to 1" (Reichenbach 1928, p. 285; tr. 1958, p. 249).

We would then be forced to introduce "the corrections factors $g_{\mu\nu}$", so "that the $g_{\mu\nu}$ thus obtained will satisfy the condition $ds^2 = 1$ for all positions of the unit rod" (Reichenbach 1928, p. 286; tr. 1958, p. 250). Other observers in other regions of space-time will the adopt the same measurement procedure, and they will also find the value of the $g_{\mu\nu}$ in their neighborhood. Riemannian geometry presupposed that not only the ratio $g_{\mu\nu} = \lambda g'_{\mu\nu}$, but the value of the constant λ can be determined after all observers have come to agree on the unit of measure to use: "Through experiment we discover at every point those numbers $g_{\mu\nu}$ by which the coordinate differentials must be multiplied in order that the interval will equal 1" (Reichenbach 1928, p. 287; tr. 1958, p. 251).

This procedure can of course be easily extended to the case of a pseudo-Riemannian manifold of general relativity, where "[t]he *coordinative definition of congruence* is again given in terms of clocks, rods, and light rays" (Reichenbach 1928, p. 287; tr. 1958, p. 251). If the field equations predict a certain value of the $g_{\mu\nu}$ (up to a coordinate transformation)—let's say the Schwarzschild solution for spatial spherical symmetry—one could then verify if the predictions of the theory were "true" or "false", using light rays "as a realization of $ds^2 = 0$, and infinitesimal clocks and rods "as realizations of $ds^2 = \pm1$" (Reichenbach 1928, p. 287; tr. 1958, p. 251).

This is of course perfectly true, but it is also worth clarifying that this is not *"a coordinative definition of congruence"* in the sense of Helmholtz and Poincaré. As we have seen, the definition of "congruence of bodies" is *not univocal* in general relativity; there is no single set of congruence relations; the very idea of making a unique conventional choice among different incompatible sets does not even make sense (Friedman 1995). As Reichenbach had explicitly shown in 1924, rods and clocks only serve to determine the *choice of the linear unit of measure*, which in general relativity is regarded as arbitrary, but *univocal*.

Usually units of measure are defined with reference to an individual standard, such as the meter standard preserved in Paris; of course, it is more useful when standards are defined through class terms, for instance "the linear unit may be defined by means of the wave length of the red cadmium line."[6] The similarity of all cadmium atoms is used for this purpose, and there is no need to store a special unit at a definite location" (Reichenbach 1929a, p. 30; tr. 1978, I, p. 161). The unit of measure can easily be replicated all over the manifold.

Even though, in Reichenbach's parlance, this is a "coordinative definition", clearly it does not concern the choice among Euclidean and non-Euclidean geometry. As we have seen, by coordinating $ds^2 = -1$ with another atom, let's say an atom of sodium with a yellow emitting line, all lengths would be changed by a scale factor, but their ratios would remain unchanged. Thus the spacetime metric would remain the same, being defined only up to a constant scale factor.

---

[6] In 1927, the International Conference on Weights and Measures redefined the meter in terms of a red cadmium spectral line (1 m = 1,553,164.13 times the wavelength of the 6436.4696 Å cadmium red line).

Rather, the real philosophical problem lurks in the phrase, "there is no need to store a special unit at a definite location". The stability of atomic spectra shows that there is a class of similar objects that can be used to reproduced the unit of measure in different positions (Reichenbach 1928, p. 355). According to Reichenbach, the "that objects in question are similar is, of course, *not established by definition*, but *is a fact* that must be discovered" (Reichenbach 1929a, p. 30; tr. 1978, I, p. 161; my emphasis).

To find an explicit discussion about the nature of this factual presupposition we must wait, however, for the long appendix on Weyl's theory (Reichenbach 1928, pp. 331–373):

> The foundation of Riemannian space is [the following:] … two measuring rods which are found equal, when they are compared near to each other, will be again equally long if they are transported through different ways in another place; the same must be assumed for the unit clocks … We have noticed above that such a definition [of equality of length] is only for this reason possible, because, the measuring-bodies possesses the mentioned *special property* ; if the measuring rods would have different length every time they met, the usual *definition* of congruence would not be possible. The mentioned property is not a means to obtain a *knowledge* of the equality of tracts … but it is a necessary condition for a *definition of congruence distinguished by special properties* (on this topic see §4). (Reichenbach 1928, 331f.)

> [Die Grundlage des Riemannschen Raumes ist [die folgende:] … Zwei Maßstäbe, die benachbart verglichen gleich lang sind, erweisen sich stets wieder als gleich lang, wenn man sie auf verschiedenen Wegen an einen andern Ort transportiert; dasselbe gilt für die Einheitszeiten von Uhren … Wir hatten auch darauf hingewiesen, daß diese Definition gerade nur deshalb möglich ist, weil die Meßkörper die genannte Vorzugseigenschaft besitzen; wären die Maßstäbe beim wiederholten Zusammentreffen immer wieder verschieden lang, so wäre auch die übliche Definition der Kongruenz nicht durchführbar. Die genannte Eigenschaft ist zwar kein Mittel, um eine Erkenntnis der Streckengleichheit zu leisten … , aber sie ist doch eine notwendige Bedingung für eine durch Vorzugseigenschaften ausgezeichnete Definition der Kongruenz. (vgl. hierzu §4).]

Reichenbach refers here explicitly to the §4 of his book where he discussed precisely the "univocality" of the definition of "equality of tracts". The fundamental question is then the following. "What would happen if the measuring rods would not posses the mentioned *special property*?" [Was wäre zu tun, wenn die Meßkörper die genannte Vorzugseigenschaft nicht besäßen?] (Reichenbach 1928, p. 332). Reichenbach surprisingly consigned this question to the appendix of his book (which will not even be translated into English). However, it is not at all exaggerated to argue that this was *the core problem of Einstein's reflections on rods and clocks* as empirical indicators. Einstein had assumed the "Vorzugseigenschaft" of rods and clocks as an independent "fact"; Weyl had showed that the alleged fact is at most a "stipulation".

Reichenbach explains very minutely the technical details of Weyl's theory in the appendix (cf. Coffa 1979). A brief summary can be found, for instance, in a paper of

1929 discussing Einstein's new geometry based on distant-parallelism (on this topic cf. Sauer 2006):

> Weyl noticed that *Riemannian geometry contains a special presupposition*: two measuring rods which, if compared next to each other are equal, are always equal, if, after having been transported through different ways, are compared again. Weyl asked, whether it would be possible to conceive more general geometrical relations, in which the rods change their length in dependence from the way along which they are transported … In this way he produced *an extension of the Riemannian space-type* of undeniable mathematical meaning, which he however wanted to apply also to the physical problem just described [the unification of gravitation and electricity] … such a theory, however, had to be abandoned for physical reasons; the effect of the electrical field on transported clocks [predicted by the theory]* was absent.
> * One just has to think of an atomic clock, e.g. the rotating electron system of the atom, whose eventual change in frequency found expression in the emitted spectral lines. (Reichenbach 1929c, p. 122)

> [Weyl bemerkte, daß die Riemannsche Geometrie eine spezielle Voraussetzung enthält: nach ihr sind zwei Maßstäbe, die in Nachbarlage verglichen gleich groß sind, stets wieder gleich groß, wenn sie auf verschiedenen Wegen an einen andern Ort transportiert und dort verglichen werden. Er fragte sich, ob ein allgemeineres Verhältnis geometrisch zu erfassen sei, bei dem die Stäbe in Abhängigkeit vom Transportweg ihre Länge ändern … Damit schuf er eine Erweiterung desRiemannschen Raumtyps, deren mathematische Bedeutung außer Zweifel stand— die er nun aber zugleich für das geschilderte physikalische Problem nutzbar zu machen versuchte …Transportweg auftritt. Aber eine derartige Theorie war nun leider aus physikalischen Gründen ganz ausgeschlossen, denn ein Einfluß des elektrischen Feldes auf transportierte Uhren*) war, wie nachgewiesen werden konnte, nicht vorhanden.
> *Man denkt hier vor allem an die "Atomuhr", d.h. das rotierende Elektronensystem des Atoms, dessen etwaige Frequenzänderung in den ausgesandten Spektrallinien zum Ausdruck kommt]

Of course, Reichenbach had good reasons to consider Weyl's theory as discredited (see for instance Weyl 1929). Reichenbach, however, does not seem to realize that, when Einstein refers, rather sporadically, to Helmholtz and Poincaré, he alludes *exclusively* to this "special presupposition" of Riemannian geometry, i.e. the "Riemannian method of space-measurements" [Riemannsche Methode der Raummessung] (Reichenbach 1929c, p. 121) of which Weyl had shown the contingency by introducing "a more richer geometry then the Riemannian one" [eine Geometrie suchte, die der Riemannschen noch an Inhaltsweite überlegen war] (Reichenbach 1929c, p. 121).

6 Einstein versus Reichenbach: from the real to the imaginary dialogue

As is well know, Einstein's esteem for Reichenbach was so high that in 1926 he managed, together with Max Planck, to create a teaching position for him in "natural

philosophy" at the University of Berlin, where Reichenbach remained until the 1930s (Hecht and Hoffmann 1982). Reichenbach, who at that time started to organize discussion groups on scientific philosophy, the so-called Berlin Group, was very close to Einstein (Howard 1994). It is then even more puzzling that Reichenbach—as his famous doctrine of the "relativity of geometry" shows—was unable to appreciate that Einstein's reference to the dialogue between Helmholtz and Poincaré was little more than an homage and surely not the exposition of a philosophical program.

Reichenbach's attempt to name Einstein's theory as the heir of the Riemann–Helmholtz–Poincaré line was doomed to failure. Reichenbach completely neglected the group-theoretical implications of Helmholtz and Poincaré's work (Friedman 1995) — implications on which other philosophers such as Ernst Cassirer insisted (Cassirer 1938, 1944, 1950, I, ch. 2; written in 1930s). Most of all he did not appreciate the fact that Riemann's work evolved along a different non-geometrical tradition—mainly in the work of Christoffel, Ricci (Ricci-Curbastro 1883, 1886, 1888, 1889, 1892) and Levi-Civita (Reich 1994)—whose geometrical significance was rediscovered only *after* general relativity by Levi-Civita himself (Reich 1992). The philosophical problem raised by Weyl's theory, which Einstein was actually addressing, can be understood exclusively within this tradition (cf. Giovanelli 2012a).

However, Reichenbach could certainly not be blamed for having lost track in following Einstein's complex "dialogical" network. Einstein resorted to the logical empiricists' favorite language of "coordination" to address Weyl's holistic challenge, and at the same time presented his dispute with Weyl as a novel version of the dialogue between Helmholtz and Poincaré. Behind Einstein's dialogical strategy probably lurks the exigence of extrapolating two general epistemological attitudes toward the geometry-physics relationship—the standpoints *A* and *B* of the Elsbach-review—from their more technical counterparts.

Einstein conformed to a sort of "double truth doctrine" all of his life. On the one hand, he never abandoned the conception of geometry as an investigation of "local relations of idealized corporeal objects" (Einstein 1930c, p. 173; tr. in Pesic 2007, p. 173; see also Einstein 1930a,b); geometry is "the science of laws governing the mutual position of practically rigid bodies" (Einstein 1934, p. 227). In *Physik und Realität* (Einstein 1936), Einstein even defined as a "fatal error [verhängnisvolle Irrtum]" the fact that this conception of geometry "has fallen into oblivion" (Einstein 1936, p. 321, tr. 356).

On the other hand, he clearly had to recognize that—as Arthur Eddington eloquently put it in his 1938 Tarner lectures—"relativity theory has to go outside its own borders to obtain the definition of length, without which it cannot begin" (Eddington 1939, p. 76). Einstein candidly admits it, in a famous passage of his *Autobiographical notes* (written in 1946, but published as Einstein 1949a):

> One is struck [by the fact] that the theory introduces two kinds of physical things, i.e., (1) measuring rods and clocks, (2) all other things, e.g., the electro-magnetic field, the material point, etc. This, in a certain sense, is *inconsistent*; strictly speaking measuring rods and clocks would have to be *represented as solutions of the basic equations* (objects consisting of moving atomic configurations), not, as it were, as *theoretically self-sufficient* entities. However, the procedure

justifies itself because it was clear from the very beginning that the postulates of the theory *are not strong enough to deduce sufficiently complete equations from them* for physical events sufficiently free from arbitrariness, in order to base upon such a foundation a theory of measuring rods and clocks. If one did not wish to forego a physical interpretation of the co-ordinates in general (something which, in itself, would be possible), it was better *to permit such inconsistency — with the obligation, however, of eliminating it at a later stage of the theory*. But one must not legalize the mentioned sin so far as to imagine that intervals are physical entities of a special type, intrinsically different from other physical variables (Einstein 1949a, p. 59; my emphasis).

Einstein's epistemological preoccupations are then consistent with those that he had expressed in the past. In the final "Remarks Concerning the Essays Brought together in this Co-operative Volume" (finished in 1948) he emphasizes the following points:

1. General relativity assumes that there is "such a thing as a natural object which incorporates the 'natural-measuring-rod' independently of its position in four-dimensional space"; in particular "a spectral line" is "considered as a measure of a 'proper time' (Eigen-Zeit) ($ds^2 = g_{ik}dx_i dx_k$)". This point of view "made the invention of the General Theory of Relativity *psychologically possible*, however this supposition is *logically not necessary*" (Einstein 1949b, p. 685; may emphasis).

2. This assumption is then not definitive: Only "[if] one disregards quantum structure, one can justify the introduction of the g_{ik} 'operationally'", referring to "the existence of an *arbitrarily sharp optical signal*" (Einstein 1949b, p. 686, my emphasis). We do not have at this time "*a complete theory of physics as a totality*" in which "the objects used as tools for measurement do not lead an independent existence alongside of the objects implicated by the field-equations" (Einstein 1949b, p. 686, my emphasis).

The echo of the debate with Weyl and Dällenbach (cf. Sect. 3) in the late 1910s can still be heard in these passages written thirty years later. The constancy of atoms gives physical meaning to the mathematical invariant ds; within general relativity this remains an unverified assumption, which, in a later stage of the development of physics, should be derived from the theory.

As is well known, Einstein used exactly this form of holism *à la* Weyl against Reichenbach in the final "Remarks" (Einstein 1949b). Interestingly, Einstein again resorts to the opposition between Helmholtz and Poincaré to display the epistemological implications of the points 1. and 2.: "Is a geometry … verifiable (viz., falsifiable) or not? Reichenbach, together with *Helmholtz*, says: Yes, provided that the empirically given solid body realizes the concept of "distance" *Poincaré* says no and consequently is condemned by Reichenbach" (Einstein 1949b, 677f.).

In his contribution to the volume, Reichenbach (1949), who Einstein praises for "the precision of deductions" and "the sharpness of his assertions", namely rehearsed his neo-conventionalist doctrine: after a definition of congruence has been chosen under the criterion that it does not imply redundant physical forces, then geometry of physical space (Euclidean or non-Euclidean) can be verified empirically using rods and clocks. Einstein famously replied by imagining a dialogue between "Reichenbach" and "Poincaré" (Einstein 1949b). By the end of the dialogue an

unidentified "Non-Positivist" bursts upon the scene. The passage is famous, but it is worth repeating again:

> *Non-Positivist*: If, under the stated circumstances, you hold distance to be a legitimate concept, how then is it with your basic principle (meaning = verifiability)? Do you not have to reach the point where you must deny the meaning of geometrical concepts and theorems and to acknowledge meaning only within *the completely developed theory of relativity* (which, however, does not yet exist at all as a finished product)? Do you not have to admit that, in your sense of the word, *no "meaning" can be attributed to the individual concepts and assertions of a physical theory at all, and to the entire system only insofar as it makes what is given in experience "intelligible?"* Why do the individual concepts which occur in a theory require any specific justification anyway, if they are only indispensable within the framework of the logical structure of the theory, and the theory only in its entirety validates itself? (Einstein 1949b, p. 678; my emphasis)

By substituting a non-Positivist for Poincaré, Einstein had probably come to realize that such a holism was not really implied by Poincaré's conventionalism, being rather similar to Pierre Duhem's epistemological stance (Grünbaum 1963a, p. 133; for the historical plausibility of this hypothesis, see Howard 1990).

The holism of the non-Positivist, however, clearly resembles that of Weyl's philosophy of geometry. In his 1949 English-augmented translation (Weyl 1949) of his 1927 monograph *Philosophie der Mathemaik und der Naturwissenschaft* (Weyl 1927), Weyl explicitly argues that geometry and physics can only be "put to the test as a *whole*" (Weyl 1949, p. 134). Weyl's objections to Einstein were still vivid thirty years after their original dispute had started. It must be kept in mind that Einstein and Weyl were colleagues for nearly twenty years in Princeton. In an "Appendix" of the book Weyl still insisted on the fact that "the behavior of rods and clocks" should in principle come out "as a remote consequence of the fully developed theory" and not be used to measure "the fundamental quantity ds^2" (Weyl 1949, p. 288).

Beller's dialogical method once again becomes very useful. Einstein addressed several interlocutors at one time, interweaving different real dialogues in an imaginary one, in which, moreover, contenders from disparate epochs appear. Einstein's dialogical web it is then not easy to disentangle. Reichenbach, after all, could have gotten the impression that Einstein was attributing to him a view of the empirical character of geometry not unlike that which Einstein himself had used as a stepping-stone (and that he still defended in a 1951 letter to the Australian student Leonard Champion as reported by Stachel 1989, now in Stachel 2002, p. 251). However, it is quite clear that Einstein was addressing Weyl's objection; the apple of discord was once again "the existence of an arbitrarily sharp optical signal", that can be used to reproduce the *unit of time* at distance.

However, logical empiricists clearly still did not grasp this point. It is sufficient to consult their reviews of Schilpp's volume which came up immediately after its publication (Frank 1949; Nagel 1950). Similarly in his 1951 response to "Professor Einstein['s]" "witty defense of conventionalism" (Reichenbach 1951, p. 135), Reichenbach simply rehearsed once again his neo-conventionalist position, entirely based on the question of establishing whether the choice among Euclidean

and non-Euclidean geometries depends on an empirical or conventional definition of *congruence*. It is rather ironic that Reichenbach argued "that there can be no differences of opinion between mathematical philosophers if only their opinions were clearly stated" (Reichenbach 1951, p. 135). The lifelong dialogue that he had with Einstein, which traversed thirty years of their professional careers, seems to have been based on a complete misunderstanding.

7 Conclusion: a dialogue of the deaf

Modern philosophy of science is highly indebted to the dialogue between Einstein, Schlick and Reichenbach on the role of rods and clocks in general relativity. Through this dialogue, Logical Empiricism made its first steps as a philosophical movement, which, especially in its "American incarnation" dominated twentieth century philosophy of science, before stiffening into an over-simplified straw-man version that only recent historical literature has began to call into question.

However, this dialogue was surprisingly a dialogue of the deaf. The reason is disarmingly simple: logical empiricists were concerned with the question of making a choice *within the class* of Riemannian geometries. Einstein was rather worried by the legitimacy of the assumption that characterizes *the class* of Riemannian geometries as such, as included in the larger class of geometries put forward by Weyl. The logical empiricists knew the latter issue perfectly well, but they never realized that Weyl was the interlocutor Einstein was actually addressing. In turn, Einstein never felt compelled to set things straight, so the illusion persisted that the subject of the debate revolved around the conventional vs. empirical choice between Euclidean and non-Euclidean geometries. This turned out to be a "a stubbornly persistent illusion", which was dragged along into successive famous debates, e.g. the Grünbaum-Putnam controversy (Grünbaum 1963b; Putnam 1963; Grünbaum 1968b,a, Chap. III).

As Grünbaum's "Riemann-Poincaré principle of the conventionality of congruence" shows (cf. Giedymin 1982), the misunderstanding between Einstein and the logical empiricists is the result of what may be called a collision of mathematical traditions (Norton 1999): The "Helmholtzian" tradition, which presupposes the "existence of bodies independent of position" and the "Riemannian" tradition, which started from the much weaker presupposition that the "length of lines is independent of position" (Freudenthal 1956; Torretti 1978, pp. 82–109, Torretti 1999, p. 163).

Helmholtz's geometrical point of view was developed mathematically mainly by Sophus Lie (starting from Lie 1886) in the theory of a continuous group of transformations (Lie 1893), and was brought to its epistemological consequences by Poincaré (Friedman 1995; Heinzmann 2001 and others). Riemann's geometrical insight, however, remained dormant; it was mainly developed non-geometrically by Christoffel and Ricci in the form of a theory of differential invariants, later systematized into the so-called "absolute differential calculus", of which general relativity, by Einstein's own account, was the "triumph" (Reich 1994). The geometrical implications of Riemann's revolutionary approach were rediscovered only after the appearance of general rela-

tivity, by, among others, Levi-Civita (Reich 1992), and appeared in radicalized form in Weyl "gauge invariance" principle.

Einstein's assumption that the relative length of rods does not depend on their pre-history is comprehensible only in the context of the "Riemannian" tradition (cf. Giovanelli 2012a). The logical empiricists not only completely neglected the group-theoretical implications of Helmholtz's approach, but most of all did not accord sufficient attention to the role that Riemann and his successors had played in the history of the epistemology of geometry. Neglecting the distinction between the Riemannian and the Helmholtzian tradition in the history of the philosophy of geometry, in an attempt to furnish "a Helmholtzian approach to space and time" (Darrigol 2007) confuses, not unlike the logical empiricists, the problem of the comparability of the length lines at a distance with that of the congruence of bodies (Torretti 1983, p. 238f.).

References

Bell, J. L., & Korté, H. (2011). Hermann Weyl. In E. N. Zalta (Ed.), *The Stanford Encyclopedia of Philosophy* (Spring 2011 ed.). http://plato.standford.edu/archieves/spr2011/entries/weyl.

Beller, M. (1999). *Quantum dialogue: The making of a revolution*. Chicago: University of Chicago Press.

Born, M. (1909). Die Theorie des starren Elektrons in der Kinematik des Relativitätsprinzips. *Annalen der Physik*, *11*, 1–56.

Born, M. (1910). Über die Definition des starren Körpers in der Kinematik des Relativitätsprinzips. *Physikalische Zeitschrift*, *11*, 233–234.

Carrier, M. (1990). Constructing or completing physical geometry? On the relation between theory and evidence in accounts of space–time structure. *Philosophy of Science*, *57*(3), 369–394.

Cassirer, E. (1938). Le concept de groupe et la théorie de la perception. *Journal de Psychologie*, *35*, 368–414.

Cassirer, E. (1944). The concept of group and the theory of perception. *Philosophy and Phenomenological Research*, *5*(1), 1–36.

Cassirer, E. (1950). *The problem of knowledge: Philosophy, science, and history since Hegel*. New Haven: Yale University Press.

Christoffel, E. B. (1869). Ueber die Transformation der homogenen Differentialausdrücke zweiten Grades. *Journal für die reine und angewandte Mathematik, 70*, 46–70 now in Christoffel, 1910, Vol. I, 352–377, 378–382.

Christoffel, E. B. (1910). *Gesammelte mathematische Abhandlungen Volume L Maurer*. Leipzig: Teubner.

Coffa, A. J. (1979). Elective affinities: Weyl and Reichenbach. In W. C. Salmon (Ed.), *Hans Reichenbach: Logical empirist*. Dordrecht: Kluwer.

Coleman, R. A., & Korté, H. (1981). Spacetime G-structures and their prolongations. *Journal of Mathematical Physics*, *22*(11), 2598–2611.

Coleman, R. A., Korté, H. (1982). *The status and meaning of the laws of inertia. PSA: Proceedings of the Biennial meeting of the philosophy of science association*, Volume 1: Contributed Papers, pp. 257–274, Philadelphia.

Coleman, R. A., & Korté, H. (1984). Constraints on the nature of inertial motion arising from the universality of free fall and the conformal causal structure of space–time. *Journal of Mathematical Physics*, *25*, 3513–3527.

Coleman, R. A., & Korté, H. (1995). A new semantics for the epistemology of geometry I: Modeling spacetime structure. *Erkenntnis*, *42*, 141–160.

Coleman, R. A., & Korté, H. (1999). Geometry and forces in relativistic and pre-relativistic theories. *Foundations of Physics*, *12*(2), 147–163.

Coleman, R. A., & Korté, H. (2001). Hennann Weyl: Mathematician, Physicist, Philosopher. In E. Scholz, R. A. Coleman, & H. Korté (Eds.), *Hermann Weyls Raum - Zeit - Materie and a general introduction to his scientific work*, DMV-Seminar; 30. Basel: Birkhäuser.

CPAE: Einstein A. (1996–). *The collected papers of Albert Einstein*. Princeton: Princeton University Press.

D'Agostino, S. (2000). *A history of the ideas of theoretical physics: Essays on the nineteenth and twentieth century physics*. Boston studies in the philosophy of science, Vol. 213. Dordrecht: Kluwer.

Darrigol, O. (2007). A Helmholtzian approach to space and time. *Studies in History and Philosophy of Science Part A*, *38*(3), 528–542.

DiSalle, R. (2002). Conventionalism and modern physics. A re-assessment. *Noûs*, *36*(2), 169–200.

Earman, J., & Glymour, C. (1980). The gravitational red shift as a test of general relativity: History and analysis. *Studies in History and Philosophy of Science Part A*, *11*(3), 175–214.

Eddington, A. S. (1939). *The philosophy of physical science: Tarner Lectures, 1938*. Cambridge: Cambridge University Press.

Ehlers, J. (1988a). Einführung in die Raum-Zeit-Struktur mittels Lichtstrahlen und Teilchen. In J. Andretsch et al. (Eds.), *Philosophie und Physik der Raum-Zeit* (pp. 145–162). Mannheim: B.I.-Wissenschaftsverlag.

Ehlers, J. (1988b). Hermann Weyl's contributions to the general theory of relativity. In W. H. Deppert (Ed.), *Exact sciences and their philosophical foundations: Vorträge des Internationalen Hermann-Weyl-Kongresses, Kiel 1985 = Exakte Wissenschaften und ihre philosophische Grundlegung*. Frankfurt am Main: Lang.

Ehlers, J., Pirani, F. A. E., & Schild, A. (1972). The geometry of free fall and light propagation. In L. O'Raifeartaigh (Ed.), *General relativity. Papers in honour of J.L. Synge*. Oxford: Clarendon Press.

Ehrenfest, P. (1909). Gleichförmige Rotation starrer Körper und Relativitätstheorie. *Physikalische Zeitschrift*, *10*, 918.

Einstein, A. (1905). Zur Elektrodynamik bewegter Körper. *Annalen der Physik, 17*, 891–921 (now in CPAE 2, Doc. 23).

Einstein, A. (1907). Relativitätsprinzip und die aus demselben gezogenen Folgerungen. *Jahrbuch der Radioaktivität, 4*, 411–462 (now in CPAE 2, Doc. 47).

Einstein, A. (1911). Die Relativitäts-Theorie. *Naturforschende Gesellschaft, Zürich, Vierteljahresschrift, 56*, 1–14 (now in CPAE 3, Doc. 17).

Einstein, A. (1912). Lichtgeschwindigkeit und Statik des Gravitationsfeldes. *Annalen der Physik, 38*, 355–369 (now in CPAE 4, Doc. 3).

Einstein, A. (1914). Die formale Grundlage der allgemeinen Relativitätstheorie. *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (pp. 1030–1085). (now in CPAE 6, Doc. 9).

Einstein, A. (1915a). Erklärung der Perihelbewegung des Merkur aus der allgemeinen Relativitätstheorie. *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (pp. 831–839). (now in CPAE 6, Doc. 24).

Einstein, A. (1915b). Feldgleichungen der Gravitation. *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (pp. 844–847). (now in CPAE 6, Doc. 25).

Einstein, A. (1915c). Grundgedanken der allgemeinen Relativitätstheorie und Anwendung dieser Theorie in der Astronomie. *Sitzungsberichte der Preussischen Akademie der Wissenschaften*, 315.

Einstein, A. (1915d). Zur allgemeinen Relativitätstheorie. In *Preussische Akademie der Wissenschaften, Sitzungsberichte* (pp. 778–786, 799–801). Berlin: (now in CPAE 6, Doc. 21 and 22).

Einstein, A. (1916). Die Grundlage der allgemeinen Relativitätstheorie. *Annalen der Physik, 49, 769–822* (now in CPAE 6, Doc 30).

Einstein, A. (1918). Nachtrag zu H. Weyl, Gravitation und Elektrizität. In *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (pp. 478–480), Berlin.

Einstein, A. (1920). *Äther und Relativitätstheorie: Rede gehalten am 5. Mai 1920 an der Reichs-Universität zu Leiden*. Berlin: Springer (now in CPAE 7, Doc. 38).

Einstein, A. (1921). *Geometrie und Erfahrung. Erweiterte Fassung des Festvortrages gehalten an der Preussischen Akademie der Wissenschaften zu Berlin am 27 Januar 1921*. Berlin: Springer (now in CPAE 7, Doc. 52).

Einstein, A. (1923). Grundgedanken und Probleme der Relativitätstheorie. Nobel prize lecture, delivered before the Nordische Naturforscherversammlung in Göteborg.

Einstein, A. (1924a). Über den Äther. *Verhandlungen der Schweizerischen Naturforschenden Gesellschaft*, *105*, 85–93.

Einstein, A. (1924). Review of Elsbach, 1924. *Deutsche Literaturzeitung*, *45*, 1685–1692.

Einstein, A. (1925). Nichteuklidische Geometrie und Physik. *Die neue Rundschau*, *36*, 16–20.

Einstein, A. (1926). Space–time. In *Encyclopedia Britannica* (13th ed., pp. 608–609). Chicago: Encyclopædia Britannica, Inc.

Einstein, A. (1930a). Das Raum-, Feld- und Ather-Problem in der Physik. *Gesamtbericht, Zweite Weltkraftkonferenz*, *19*, 1–5.

Einstein, A. (1930b). Das Raum-, Feld- und Ather-Problem in der Physik. *Koralle*, *5*, 486–487.

Einstein, A. (1930c). Raum, Äther und Feld in der Physik. *Forum Philosophicum, 1*, 173–180 (tr. the same volume, pp. 180–184).

Einstein, A. (1934). Das Raum-, Äther- und Feld-Problem der Physik. In C. Seelig (Ed.), *Mein Weltbild*. Amsterdam: Querido Verlag.

Einstein, A. (1936). Physik und Realität [physics and reality]. *Franklin Institute Journal, 221*, 313–347; English translation (by J Picard), 349–382. Also reprinted from *Zeitschrift für freie deutsche Forschung, 1* (1), 5–19 and (2), 1–14 (1938).

Einstein, A. (1949a). Autobiographical notes. In P. A. Schilpp (Ed.), *Albert Einstein, Philosopher-Scientist*. Evanston, IL: Library of Living Philosophers.

Einstein, A. (1949b). Remarks concerning the essays brought together in this co-operative volume. In P. A. Schilpp (Ed.), *Albert Einstein, Philosopher-Scientist*. Evanston, IL: Library of Living Philosophers.

Einstein, A. (1954). *Ideas and opinions*. New York: Crown Publishers.

Einstein, A., & Grossmann M. (1913). Entwurf einer verallgemeinerten Relativitätstheorie und eine Theorie der Gravitation I Physikalischer Teil von A Einstein II Mathematischer Teil von M. Grossmann. *Zeitschrift für Mathematik und Physik, 62*, 225–244, 245–261 (now in CPAE 4, 13).

Elsbach, A. C. (1924). *Kant und Einstein : Untersuchungen über das Verhältnis der modernen Erkenntnistheorie zur Relativitätstheorie*. Berlin: de Gruyter.

Flamm, L. (1916). Beiträge zur Einstein'schen Gravitationstheorie. *Physikalische Zeitschrift*, *17*, 448–454.

Fogel B. (2008). *Epistemology of a theory of Everything. Weyl, Einstein, and the unification of physics*. Dissertation, University of Notre Dame, Notre Dame.

Frank, P. (1949). Einstein's philosophy of science. *Reviews of Modern Physics*, *21*(3), 349.

Freudenthal, H. (1956). Neuere Fassungen des Riemann-Helmholtz-Lieschen Raumproblems. *Mathematische Zeitschrift*, *63*, 374–405.

Freundlich, E. (1920). Zu dem Aufsatze, "Die Physik als geometrische Notwendigkeit" von Arthur Haas (Naturwissenschaften 1920, Heft 3). *Naturwissenschaften*, *8*, 234–235.

Friedman, M. (1995). Poincaré's conventionalism and the logical positivists. *Foundations of Science, 2*, 299–314 (now in [Friedman 1999]).

Friedman M. (2002). Geometry as a branch of physics. Background and context for Eisnstein's 'Geometry and experience'. In D. Malament (Ed.), *Reading natural philosophy*. Chicago-La Salle (IL): Open Court.

Giedymin, J. (1982). On the origin and significance of Poincaré's conventionalism. In J. Giedymin (Ed.), *Science and convention. Essay on Henri Poincaré's philosophy of science and the conventionalist tradition*. Oxford: Pergamon Press.

Giovanelli, M. (2012a). The forgotten tradition: How the logical empiricists missed the philosophical significance of the work of Riemann, Christoffel and Ricci. *Erkenntnis*. doi:10.1007/s10670-012-9407-2

Giovanelli, M. (2012b). Erich Kretschmann as a protological-empiricist: Adventures and misadventures of the point-coincidence argument. *Studies in the History and Philosophy of Modern Physics*. doi:10.1016/j.shpsb.2012.11.004

Goenner, H. F. (2004). On the history of unified field theories. *Living Reviews in Relativity, 7*, 2.

Grøn, Ø. (2004). Space geometry in a rotating reference frame: A historical appraisal. In G. Rizzi & M. Ruggiero (Eds.), *Relativity in rotating frames*. Dordrecht: Kluwer.

Grünbaum, A. (1963a). Carnap's views on the foundations of geometry. In P. A. H. Schilpp (Ed.), *The philosophy of Rudolf Carnap* (Vol. 11). Lasalle, IL/Evanston, IL: Open Court/The Library of Living Philosophers.

Grünbaum, A. (1963b). *Philosophical problems of space and time*. Dordrecht: Reidel.

Grünbaum, A. (1968a). *Geometry and chronometry: In philosophical perspective*. Minneapolis: University of Minnesota Press.

Grünbaum, A. (1968b). Reply to Hilary Putnam's 'An examination of Grnbaum's philosophy of geometry'. In R. Cohen & M. Wartofsky (Eds.), *Boston studies in the philosophy of science* (Vol. 5, pp. 1–150). Dordrecht: Reidel.

Hecht, H., & Hoffmann, D. (1982). Die Berufung Hans Reichenbachs an die Berliner Universität. *Deutsche Zeitschrift für Philosophie*, *30*, 651–662.

Heinzmann, G. (2001). The foundations of geometry and the concept of motion: Helmholtz and Poincaré. *Science in context*, *14*(3), 457–470.

Helmholtz, H. (1921). *Schriften zur Erkenntnistheorie*. Berlin: Springer.

Hentschel, K. (1982). Zur Rolle Hans Reichenbachs in den Debatten um die Relativitätstheorie (mit der vollständigen Korrespondenz Reichenbach-Friedrich Adler im Anhang). *Nachrichtenblatt der Deutschen Gesellschaft für Geschichte der Medizin, Naturwissenschaft & Technik, 3*, 101–102.

Hentschel, K. (1986). Die Korrespondenz Einstein-Schlick: Zum Verhältnis der Physik zur Philosophie. *Annals of Science, 43*, 475–488.

Hentschel, K. (1994). Erwin Finlay Freundlich and testing Einstein's theory of relativity. *Archive for the History of the Exact Sciences, 47*, 143–201.

Herglotz, G. (1910). Über den vom Standpunkt des Relativitätsprinzips aus als "starr" zu bezeichnenden Körper. *Annalen der Physik, 31*, 393–415.

Hilbert, D. (1916/1917, Wintersemester). *Die Grundlagen der Physik II*. Lesesaal: Georg-August-Universitat Göttingen, Mathematisches Institut (now in Hilbert 2009, 162–307).

Hilbert, D. (2009). *David Hilbert's lectures on the foundations of physics 1915–1927*. Berlin: Springer.

Howard, D. (1984). Realism and conventionalism in Einstein's philosophy of science: The Einstein-Schlick correspondence. *Philosophia Naturalis, 21*, 618–629.

Howard, D. (1990). Einstein and Duhem. *Synthese, 83*, 363–384.

Howard D. (1994). Einstein, Kant and the origins of logical empiricism. In W. C. Salmon, & G. Wolters (Eds.), *Logic, language, and the structure of scientific theories: Proceedings of the Carnap-Reichenbach Centennial*, University of Konstanz, 21–24 May 1991. Pittsburgh, PA/Konstanz: University of Pittsburgh Press/Universitätsverlag Konstanz.

Howard, D. (2005). Einstein's philosophy of science. In E. N. Zalta (Ed.), *The Stanford Encyclopedia of philosophy* (Summer 2010 ed.). http://plato.stanford.edu/archives/sum2010/entries/einsteinphilscience/.

Howard, D. (2009). Einstein and the development of twentieth-century philosophy of science. In M. Janssen & C. Lehner (Eds.), *The Cambridge companion to Einstein*. Cambridge: Cambridge University Press.

Howard, D. (2010). "Let me briefly indicate why I do not find this standpoint natural": Einstein, general relativity, and the contingent a priori. In *Discourse on a new method: Reinvigorating the marriage of history and philosophy of science*. La Salle, IL: Open Court.

Jackson, C. V. (1936). The red line of cadmium as a standard of wave-length. *Proceedings of the Royal Society of London. Series A, Mathematical and Physical Sciences, 155*(885), 407–419.

Kaluza, T. (1910). Zur Relativitätstheorie. *Physikalische Zeitschrift, 11*, 977–978.

Kretschmann, E. (1917). Über den physikalischen Sinn der Relativitätspostulate. A. Einsteins neue und seine ursprüngliche Relativitätstheorie. *Annalen der Physik, 53*, 575–614.

Kundt, W., & Hoffman, B. (1962). Determination of gravitational standard time. In *Recent developments in general relativity*. New York: Pergamon.

Laue, M. V. (1920). Theoretisches über neuere optische Beobachtungen zur Relativitätstheorie. *Physikalische Zeitschrift, 21*, 659–662.

Levi-Civita, T., & Ricci-Curbastro, G. (1900). Méthodes de calcul différentiel absolu et leurs applications. *Mathematische Annalen, 54*, 125–201 (now in Ricci-Curbarstro 1956–57, I).

Lie, S. (1886). Bemerkungen zu v. Helmholtz' Arbeit über die Tatsachen, welche der Geometrie zugrunde liegen. Berichte über die Verhandlungen der Kgl. *Sächsischen Gesellschaft der Wissenschaften zu Leipzig, 38*, 337–342.

Lie, S. (1893). *Theorie der Transformationsgruppen* (Vol. 3). Leipzig: Teubner.

London, F. (1927). Die Theorie von Weyl und die Quantenmechanik. *Naturwissenschaften, 15*, 187–187.

Lorentz, H. A. (1917). On Einstein's theory of gravitation I. In *Proceedings* (Vol. 19(II), pp. 1341–1354), Amsterdam Society.

Lorentz, H. A. (1923). The determination of the potentials in the general theory of relativity, with some remarks about the measurement of lengths and intervals of time and about the theories of Weyl and Eddington. In *Proceedings of Academy* (Vol. 29, 1–1), . Amsterdam.

Maltese, G., & Orlando, L. (1995). The definition of rigidity in the special theory of relativity and the genesis of the general theory of relativity. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics, 26*(3), 263–306.

Marzke, R., & Wheeler, J. (1964). Gravitation as geometry I: The geometry of spacetime and the geometrodynamical standard meter. In H. Chiu & W. Hoffman (Eds.), *Gravitation and Relativity*. New York: Benjamin.

Marzke, R. F. (1959). *The theory of measurement in general relativity*. Princeton, NJ: Department of Physics Senior Thesis.

Nagel, E. (1950). Einstein's philosophy of science. *The Kenyon Review, 12*(3), 520–531.

Noether, F. (1910). Zur Kinematik des starren Körpers in der Relativitätstheorie. *Annalen der Physik*, *31*, 919–944.

Norton, J. D. (1999). Geometries in collision: Einstein, Klein and Riemann. In J. Gray (Ed.), *The symbolic universe* (pp. 128–144). Oxford: Oxford University Press.

O'Raifeartaigh, L. (1997). *The dawning of gauge theory*. Princeton: Princeton University Press.

O'Raifeartaigh, L., & Straumann, N. (2000). Gauge theory: Historical origins and some modern developments. *Reviews of Modern Physics*, *72*, 1–23.

Pais, A. (1982). *Subtle is the Lord: The science and the life of Albert Einstein*. New York: Oxford University Press.

Parrini, P. (2005). L'empirismo logico. *Aspetti storici e prospettive teoriche*. Roma: Carocci

Pesic, P. (2007). *Beyond geometry: Classic papers from Riemann to Einstein*. Mineola, NY: Dover Publications.

Putnam, H. (1963). An examination of Grünbaum's philosophy of space and time. In B. Baumrin (Ed.), *Philosophy of science. The Delaware Seminar vol. 2, 1962–1963* (pp. 205–255). New York: Interscience/Wiley (now in Putnam, 1975, 93–129).

Reich, K. (1992). Levi-Civitasche Parallelverschiebung, affiner Zusammenhang, Übertragungsprinzip: 1916/1917–1922/1923. *Archiv für Geschichte der Philosophie*, *44*(1), 77–105.

Reich, K. (1994). *Die Entwicklung des Tensorkalküls: vom absoluten Differentialkalkül zur Relativitätstheorie*. Berlin: Birkhäuser.

Reichenbach, H. (1920). *Relativitätstheorie und Erkenntnis apriori*. Berlin: Springer (now in Reichenbach 1975, vol. III, 191–332).

Reichenbach, H. (1921). Der gegenwärtige Stand der Relativitätsdiskussion. Eine kritische Untersuchung. *Logos*, *22*(10), 316–378 (now in Reichenbach 1977, vol. III).

Reichenbach, H. (1922). La signification philosophique de la théorie de la relativité. *Revue philosophique de la France et de l'Étranger*, *93*, 5–61.

Reichenbach, H. (1924). *Axiomatik der relativistischen Raum-Zeit-Lehre*. Vieweg: Braunschweig.

Reichenbach, H. (1925). Über die physikalischen Konsequenzen der relativistischen Axiomatik. *Zeitschrift für Physik*, *34*(1), 32–48.

Reichenbach, H. (1928). *Philosophie der Raum-Zeit-Lehre*. Berlin and Leipzig: Walter de Gruyter (now in Reichenbach 1977, Vol II).

Reichenbach, H. (1929a). Ziele und Wege der physikalischen Erkenntnis. In *Handbuch der Physik, vol. 4: Allgemeine Grundlagen der Physik* (pp. 1–80). Berlin: Springer.

Reichenbach, H. (1929b). Die neue Theorie Einsteins über die Verschmelzung von Gravitation und Elektrizität. *Zeitschrift für Angewandte Chemie*, *42*, 121–123.

Reichenbach, H. (1949). The philosophical significance of the theory of relativity. In P. A. Schilpp (Ed.), *Albert Einstein. Philosopher-Scientist* (pp. 289–311). New York: Tudor.

Reichenbach, H. (1951). *The rise of scientific philosophy*. Berkely: University of California Press.

Reichenbach, H. (1958). *The philosophy of space and time*. New York: Dover.

Reichenbach, H. (1965). *The Theory of Relativity and a priori Knowledge*. Berkeley: University of California Press.

Reichenbach, H. (1969). *Axiomatization of the theory of relativity*. Berkeley, CA: University of California Press.

Reichenbach, H. (1977). *Gesammelte Werke in 9 Bänden*. Braunschweig; Wiesbaden: Vieweg.

Reichenbach H. (1978). *Selected writings: 1909–1953*. Vienna circle collection, Vol. 4. Dordrecht: Reidel.

Ricci-Curbastro, G. (1883). Principii di una teoria delle forme differenziali quadratiche. *Annali di Matematica Pura ed Applicata, 12*, 135–167 (now in Ricci-Curbarstro 1956–57, I).

Ricci-Curbastro, G. (1886). Sui parametri e gli invarianti delle forme quadratiche differenziali. *Annali di Matematica Pura ed Applicata (1867–1897), 14*, 1–11 (now in Ricci-Curbarstro 1956–57, I).

Ricci-Curbastro, G. (1888). Delle Derivazioni covarianti e controvarianti e del loro uso nella analisi applicata. In *Studi editi dalla Università di Padova a commemorare l'ottavo centenario della Università di Bologna* (Vol. 3, pp. 3–23). Padova: Tip. del Seminario.

Ricci-Curbastro, G. (1889). Sopra certi sistemi di funzioni. *Atti Accad. Lincei, 4/6*, 112–118 (now in Ricci-Curbarstro 1956–57, I).

Ricci-Curbastro, G. (1892). Le calcul différentiel absolu. *Bulletin des sciences mathématiques*, *16*, 167–189.

Ricci-Curbastro G. (1956–1957). *Opere*. Roma: Cremonese.

Ryckman, T. (1995). Weyl, Reichenbach and the Epistemology of Geometry. *Studies in History and Philosophy of Science*, *25*(6), 831–870.

Ryckman, T. (1996). Einstein Agonists: Weyl and Reichenbach on geometrv and the general theory of relativity. In R. N. Giere (Ed.), *Origins of logical empiricism* (pp. 165–209). Minneapolis, MN: University of Minnesota Press.

Ryckman, T. (2005). *The reign of relativity. Philosophy in physics 1915–1925*. Oxford, NY: Oxford University Press.

Rynasiewicz, R. (2005). Weyl vs. Reichenbach on Lichtgeometrie. In A. J. Kox & J. Eisenstaedt (Eds.), *The universe of general relativity*. Boston: Birkhäuser.

Sauer, T. (2006). Field equations in teleparallel space–time: Einstein's Fernparallelismus approach toward unified field theory. *Historia Mathematica*, *33*(4), 399–439.

Sauer T. (2008). The Einstein-Varićak correspondence on relativistic rigid rotation. In R. J. H. Kleinert & R. Ruffini (Eds.), *The eleventh Marcel Grossmann meeting on recent developments in theoretical and experimental general relativity, gravitation and relativistic field theories*. Singapore: World Scientific Pub. Co.

Schlick, M. (1917). Raum und Zeit in der gegenwärtigen Physik. Zur Einführung in das Verständnis der allgemeinen Relativitätstheorie. *Die Naturwissenschaften, 5*, 161–167, 177–186 (now in Schlick, 2006, vol. II).

Schlick, M. (1918). *Allgemeine Erkenntnislehre*. Naturwissenschaftliche Monographien und Lehrbücher. Berlin: Springer (now in Schlick 2006, vol. I).

Schlick, M. (1921). Review of Einstein 1921. *Die Naturwissenschaften*, *22*, 435–436.

Schlick, M. (1925). *Allgemeine Erkenntnislehre* (2nd ed.). Naturwissenschaftliche Monographien und Lehrbücher. Berlin: Springer (now in Schlick 2006, vol. 1).

Schlick M. (1978). *Philosophical papers*. Vienna Circle collection, Vol. 11. Dordrecht: Reidel.

Schlick, M. (2006–). *Gesamtausgabe*. Berlin: Springer.

Schlick, M., & Reichenbach, H. (1920–1922). Correspondence 1920–1922. http://echo.mpiwg-berlin.mpg.de/content/space/space/reichenbach1920-22.

Scholz, E. (2004). Hermann Weyl's analysis of the "problem of space" and the origin of gauge structures. *Science in Context*, *17*, 165–197.

Scholz, E. (2008). Weyl geometry in late 20th century physics. In V. Bach & D. E. Rowe (Ed.), *Beyond Einstein. Proceedings Mainz conference*, September 2008 (to appear). Birkhäuser: Basel (draft at http://arxiv.org/abs/1111.3220)

Schwarzschild, K. (1916). Über das Gravitationsfeld eines Massenpunktes nach der Einsteinschen Theorie. *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (pp. 189–196).

Shapiro, L. S. (1994). Coordinative definition' and Reichenbach's semantic framework: A reassessment. *Erkenntnis*, *41*(3), 287–323.

Stachel, J. (1989). The rigidly rotating disk as the 'missing link in the history of general relativity'. In D. Howard & J. Stachel (Eds.), *Einstein and the history of general relativity (Einstein Studies, Vol. 1)* (pp. 48–62). Boston: Birkhäuser.

Stachel, J. (1998) (ed.). *Einstein's miraculous year: five papers that changed the face of physics*. Princeton: Princeton University Press.

Stachel, J. (2002). *Einstein from "B" to "Z"*. Boston: Birkhäuser.

Synge, J. (1960). *Relativity: The general theory*. Amsterdam/New York: North-Holland Pub. Co./Interscience Publishers.

Torretti, R. (1978). *Philosophy of geometry from Riemann to Poincaré*. Dordrecht: Reidel.

Torretti, R. (1983). *Relativity and geometry*. Oxford: Pergamon Press.

Torretti, R. (1999). *The philosophy of physics*. Cambridge: Cambridge University Press.

Vizgin, V. P. (1994). *Unified field theories in the first third of the 20th century*. Boston: Birkhäuser.

von Laue, M. (1911). Zur Diskussion über den starren Körper in der Relativitätstheorie. *Physikalische Zeitschrift*, *12*, 85–87.

Weyl, H. (1918a). Erwiderung auf Einsteins Nachtrag zu H. Weyl, Gravitation und Elektrizität. *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (pp. 478–480) (now in WGA II, Doc. 31).

Weyl, H. (1918b). Gravitation und Elektrizität. *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (pp. 65–480) (now in WGA II, Doc. 31).

Weyl, H. (1918c). *Raum, Zeit, Materie: Vorlesungen über allgemeine Relativitätstheorie*. Berlin: Springer.

Weyl, H. (1918s). Reine Infinitesimalgeometrie. *Mathematische Zeitschrift 2*, 384–411 (now in WGA II, Doc. 30).

Weyl, H. (1919a). Eine neue Erweiterung der Relativitätstheorie. *Annalen der Physik, 59*, 101–133 (now in WGA II, Doc. 34).

Weyl, H. (1919b). *Raum, Zeit, Materie: Vorlesungen über allgemeine Relativitätstheorie* (2nd ed). Berlin: Springer.

Weyl, H. (1920). Elektrizität und Gravitation. *Physikalische Zeitschrift, 21*(23/24), 649–650 (now in WGA II, Doc. 40).

Weyl, H. (1921a). Über die physikalischen Grundlagen der erweiterten Relativitätstheorie. *Physikalische Zeitschrift, 22*, 473–480 (now in WGA II, Doc. 46).

Weyl, H. (1921b). Electricity and gravitation. *Nature, 106*, 800–802 (now in WGA II, Doc. 48).

Weyl, H. (1921c). Feld und Materie. *Annalen der Physik, 65*, 541–563 (now in WGA II, Doc. 47).

Weyl, H. (1921d). *Raum, Zeit, Materie: Vorlesungen über allgemeine Relativitätstheorie* (4th ed.). Berlin: Springer.

Weyl, H. (1921e). Zur Infinitesimalgeometrie: Einordnung der projektiven und konformen Auffassung. Nachrichten der Königlichen Gesellschaft der Wissenschaften zu Göttingen. *Mathematisch Physikalische Klasse, 99–112* (now in WGA II, Doc. 43).

Weyl, H. (1922). Die Relativitätstheorie auf der Naturforscherversammlung. *Jahresbericht der Deutschen Mathematikervereinigung, 31*, 51–63 (now in WGA II, Doc. 52).

Weyl, H. (1922). *Space–time–matter*. London: Methuen & Co. Ltd.

Weyl, H. (1924a). Massenträgheit und Kosmos. Ein Dialog. *Naturwissenschaften, 12*, 197–204 (now in WGA II, Doc. 65).

Weyl, H. (1924b). Rezension von: H. Reichenbach: Axiomatik der relativistischen Raum- Zeit Lehre. *Deutsche Literaturzeitung, 45*, 2122–2128.

Weyl, H. (1927). Philosophie der Mathematik und Naturwissenschaft. In A. Bäumler & M. Schröter (Eds.), *Handbuch der Philosophie*. München/Berlin: Oldenbourg.

Weyl, H. (1929). Gravitation and the electron. *Proceedings of the National Academy of Sciences of the United States of America*, *15*, 323–334.

Weyl, H. (1949). *Philosophy of mathematics and natural science*. Princeton: Princeton University Press.

WGA: Weyl, H. (1968). *Gesammelte Abhandlungen*. Berlin: Springer.

Yang, C.-N. (1986). Hermann Weyl's contribution to physics. In C. N. Yang, R. Penrose, A. Borel, & K. Chandrasekharan (Eds.), *Hermann Weyl: 1885–1985; centenary lectures*. Berlin: Springer.