COLLISION OF TRADITIONS. THE EMERGENCE OF LOGICAL EMPIRICISM BETWEEN THE RIEMANNIAN AND HELMHOLTZIAN TRADITIONS

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Abstract. This paper attempts to explain the emergence of the logical empiricist philosophy of space and time as a collision of mathematical traditions. The historical development of the "Riemannian" and "Helmholtzian" traditions in 19th century mathematics is investigated. Whereas Helmholtz's insistence on rigid bodies in geometry was developed group theoretically by Lie and philosophically by Poincaré, Riemann's *Habilitationsvotrag* triggered Christoffel's and Lipschitz's work on quadratic differential forms, paving the way to Ricci's absolute differential calculus. The transition from special to general relativity is briefly sketched as a process of escaping from the Helmholtzian tradition and entering the Riemannian one. Early logical empiricist conventionalism, it is argued, emerges as the failed attempt to interpret Einstein's reflections on rods and clocks in general relativity through the conceptual resources of the Helmholtzian tradition. Einstein's epistemology of geometry should, in spite of his rhetorical appeal to Helmholtz and Poincaré, be understood in the wake the Riemannian tradition and of its aftermath in the work of Levi-Civita, Weyl, Eddington, and others.

1. Introduction

In an influential paper, John Norton (1999) has suggested that general relativity might be regarded as the result of a "collision of geometries" or, better, of geometrical strategies. On the one hand, Riemann's "additive strategy," which makes sure that no superfluous elements enter into the initial setting and then progressively obtains the actual geometry of space as the result of a progressive enrichment of structure. On the other hand Klein's "subtractive strategy," in which, on the contrary, one starts "with all bells and whistles and then strips away all elements deemed to be descriptive fluff only" (Janssen, 2005, 61)

This paper intends to show that the emergence of the early logical empiricists' philosophy of space and time might be seen as the result of a cognate, but less successful, "collision" of two geometrical traditions that might be labeled the "Helmholtzian" and the "Riemannian" traditions. Logical empiricist neo-conventionalism is nothing but the result of the attempt to interpret the philosophical novelty of general relativity, the very "triumph" (Einstein, 1915, 778) of

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the Riemannian tradition in Einstein's own account, by resorting to the conceptual resources shaped by the Helmholtzian one. Fascinated by Helmholtz's approach to geometry based on the independence of the *congruence of bodies* from position, what the logical empiricists neglected to appreciate was that Riemann's assumption of the independence of the *length of lines* from position was the relevant issue (Torretti, 1983).

This paper is divided into three parts. In the first part (§§2; 3), the main line of Riemann's (§2.1) and Helmholtz's (§2.2) contributions to geometry are sketched and their development into different line of evolutions is investigated: if Lie and Poincaré developed the group theoretical and philosophical implications of Helmholtz's work (§3.1), Riemann's insight found its expression in the non-geometrical work of Christoffel, Lipschitz (§2.2), Ricci and Levi-Civita (§3.2) that led to the formulation of the absolute differential calculus.

In the second part of this paper, (§4, the passage form special to general relativity is presented as an escape from the Helmholtzian tradition (§4.1) and an appropriation of the conceptual tools of the Riemannian one (§4.2). It was only after general relativity that the geometrical content of this tradition was rediscovered by Levi-Civita, and implemented by Weyl in space–time physics (§4.3).

The third part of this paper, (§§5; 6; 7), will show how the standard logical empiricists' (Schlick's and most of all Reichenbach's) philosophy of space and time emerged in the attempt to interpret general relativity in the light of Helmholtz's and Poincaré's philosophy of geometry (§§5.1; 5.3; 5.4; 6.2), partly misled by some ambiguous remarks of Einstein himself (§§5.2; 6.1). Neglecting the group theoretical implications of Helmholtz's and Poincaré's work, logical empiricists considered their discussion of the role of rigid bodies in the foundations of geometry (the reproducibility of congruent figures at a distance), as the right philosophical framework to understand the role of rods and clocks in general relativity. However, rods and clocks in Einstein's theory represent the counterpart of a central feature of Riemannian geometry as such (the reproducibility of the unit of length at a distance), that Weyl and Eddington had proposed to drop (§7).

This paper hopes to show that the role of Helmholtz and Poincaré in the historical and systematical discussion of space–time theory has been overestimated among philosophers of science. Also recent historically accurate and philosophically influential works, such as those of Michael Friedman (from Friedman, 2001 to Friedman, 2010) or Robert DiSalle (DiSalle, 2006) have analyzed the developments of scientific philosophy in the 19th century, from Kant to Einstein, precisely along a line in which Helmholtz and Poincaré are the main protagonists. The historical reconstruction that we suggest in the present paper should contribute to show that the philosophical problems raised by Einstein's theory can be properly understood from the point a view of what we have labeled the Riemannian tradition, from the less accessible works of authors such as Christoffel, Lipschitz, Ricci, etc., to which historians of the philosophy of science have granted far too little attention.

2. Riemann, Helmholtz and the Birth of the 19th Century Debate on the Foundations of Geometry

2.1. Riemann's *Habilitationsvortrag* and its Analytic Development in the *Commentatio Mathematica*.

Bernhard Riemann's *Ueber die Hypothesen,* *welche der Geometrie zu Grunde liegen*—a lecture given in 1854 to fulfill the requirements of promotion to *Privatdozent*—was discovered in the late 1860s by Dedekind, to whom Riemann's wife had handed over the her husband's *Nachlass*. As Riemann's preparatory notes reveal (Scholz, 1982), his intention was to "build all geometry" without resorting to "any spatial intuition" (cited in Scholz, 1982, 228). Space might be regarded as a special case of the abstract concept of a continuous n-dimensional manifold (Scholz, 1979), in which, roughly, every single element is identified by means of n variables $x_1, x_2, x_3, \ldots, x_n$ that can assume the "the continuous series of all possible number values [Zahlenwerthe] from $-\infty$ to $+\infty$" (cited in Scholz, 1982, 228). The system of colors (Riemann, 1854/1868, 135; tr. 1873, 15), as generated as mixtures of three basic colors, can be considered as another example of a three-dimensional manifold, whereas sounds, defined by their continuously changing pitch and volume, can be seen as being the elements of a two-dimensional manifold.

Having introduced the notion of a continuous manifold of n dimensions, Riemann's next problem was to investigate the measure relations of which such a manifold is capable (Scholz, 1992), that is, the possibility of comparing "[d]efinite portions of a manifold, distinguished by a mark or by a boundary" (which Riemann calls "quanta") with one another (Riemann, 1854/1868, 135; tr. 1873, 15).

The comparison with regard to quantity in a continuous manifold happens through measurement. Measurement involves the "superposition of the quantities to be compared" [einem Aufeinanderlegen der zu vergleichenden Grössen] (Riemann, 1854/1868, 135; tr. 1873, 15). It must be possible to transport one quantity to be used "as a measuring rod [als Massstab]" (Riemann, 1854/1868, 135; tr. 1873, 15) for the others so that the ratio between the magnitude of every quantity and a given unit magnitude of the same kind can be determined univocally. Without the possibility "of transporting a certain manifold as a measuring rod on another" (cited in Scholz, 1982, 228) one could compare two manifolds "only when one is part of the other, and then only as to more or less, not as how much" (Riemann, 1854/1868, 140; tr. 1873, 15) with respect to a given standard of length. In other words, if measurement must be possible, then magnitudes must be regarded as "existing independently of position" and "expressible in terms of a unit" (Riemann, 1854/1868, 135; tr. 1873, 15).

According to Riemann, space has a feature that distinguishes it from other possible continuous manifolds. In space, the magnitude of any piece of a manifold of one dimension can be compared with any other, or, as Riemann famously put it, "the length of lines is independent of their position and consequently every line is measurable by means of every other" (Riemann, 1854/1868, 138; tr. 1873, 15), that is, it can be expressed as a multiple or a fraction of every other. In contrast, in the manifold of colors there is no relation between any two arbitrary colors that would correspond to the distance between any two points in space, nor can one compare a difference of pitch with a difference of volume in the manifold of sounds.

In space, there is instead a distance between any pair of arbitrary points, which can be expressed as a function of their coordinates. Inspired by "the celebrated memoir of Gauss, *Disqusitiones generales circa superficies curvas*" (Gauss, 1828), Riemann famously assumed the hypothesis (the simplest among the other possible alternatives) that the distance between any two arbitrarily closed points, the so called line element ds, is equal to "the square root of an always positive integral homogeneous function of the second order of the quantities dx, in which the coefficients are continuous functions of the quantities x" (Riemann, 1854/1868, 140; tr. 1873, 16; cf. Libois, 1957; Scholz, 1992).

The length of an arbitrary path connecting two points can then be calculated as the integral $\int ds$, that is, by adding together the lengths of the infinitesimal portions into which the line may be decomposed. More precisely, the equality of line-lengths corresponds to the equality of such integrals, so that the relative ratios of any two lengths are uniquely represented. Multiplying all length by a positive constant causes the values to change to a different unit of measure without upsetting relative ratios of lengths. Hence, once a unit of length has been stipulated, the numerical value of the length of an arbitrary line may be determined "in a manner wholly independent of the choice of independent variables" (Riemann, 1854/1868, 140; tr. 1873, 16).

The coefficients of the quadratic expression depend indeed on a particular choice of such variables, but, regardless of the system of variable used, the length of a line is always assigned the same value. As Riemann points out, every such expression can be in fact transformed "into another similar one if we substitute for the n independent variables functions of n new independent variables" (Riemann, 1854/1868, 140; tr. 1873, 16), leaving "unaltered the length of lines." However, as he immediately emphasizes, "we cannot transform any expression into any other" (Riemann, 1854/1868, 140; tr. 1873, 16).

In particular it is not always possible to transform every expression into one "in which the line element may be reduced to the form $\sqrt{\sum dx^2}$" (Riemann, 1854/1868, 141; tr. 1873, 16; for more details Portnoy, 1982; Zund, 1983), that is, the manifolds that Riemann calls "flat," where "the position of points" might be "expressed by rectilinear co-ordinates" (Riemann, 1854/1868, 140; tr. 1873, 16). Hence, when "the line-element ... is no longer reducible to the form of the square root of a sum of squares" (Riemann, 1854/1868, 140; tr. 1873, 16), this might be interpreted as a "deviation from flatness," just like a "sphere ... cannot be changed into a plane without stretching" (Riemann, 1854/1868, 140; tr. 1873, 16; cf. Farwell and Knee, 1992).

In the 1861 *Commentatio mathematica*—which Dedekind also found in the *Nachlass* (Riemann, 1861/1876, tr. Farwell, 1990, 240-253)—Riemann developed the formal tools for discerning the geometrical properties that do not depend on the choice of the independent variables from those that are a mere appearance introduced by the special variables one has chosen (Farwell, 1990). In particular—answering a question on heat conduction for a prize offered by the French Academy of Sciences—Riemann investigated under which conditions a positive definite quadratic differential form $\sum a_{\iota,\iota'} dx_\iota dx_{\iota'}$ with non-constant coefficients could be transformed into $\sum_\iota dx_\iota^2$ with constant coefficients by a mere change of the independent variables (Zund, 1983). Thus the four-index symbol $(\iota\iota', \iota''\iota''')$—containing the first and the second partial derivatives of the functions $a_{\iota,\iota'}$ with respect to the coordinates—turned out to furnish an objective mathematical criterion: when it vanishes, the non-constancy of the coefficients $a_{i,i'}$ (which in this context correspond to the conductivity coefficients of the body) is merely an artifact of the system of variables chosen, if not, the non-constancy expresses a real difference.

As Riemann observes, although in passing, the expression $\sqrt{\sum a_{\iota,\iota'} dx_\iota dx_{\iota'}}$ "can be regarded as a line element in a more general space of n dimensions extending beyond the bounds of our intuition (Riemann, 1861/1876, 435, tr. Farwell, 1990, 252). Quadratic differential forms that can be transformed into one another by a mere change of variables leaving the line element unchanged represent the same geometry. Thus the four-index symbol $(\iota\iota', \iota''\iota''')$ corresponds geometrically to "the measure of curvature" (Riemann, 1861/1876, 435, tr. Farwell, 1990, 252). We have thus to differentiate the $a_{\iota,\iota'}$ twice before we arrive at a geometrical property that has a significance independent of any special coordinate system.

Whereas such variation in the $a_{\iota,\iota'}$ might be neglected in small enough regions of space—a property that Riemann calls "flatness in the smallest parts" (Riemann, 1854/1868, 143; tr. 1873, 16)—, over larger regions, "the curvature at each point may have an arbitrary value in three directions" (Riemann, 1854/1868, 149; tr. 1873, 37). Spaces of constant curvature, that are "exactly the same in all directions at one point as at another" (Riemann, 1854/1868, 145; tr. 1873, 37), are merely a special case, where "not merely an existence of *lines* independent of position, but of *bodies*" is assured (Riemann, 1854/1868, 149; tr. 1873, 36).

In general, in Riemann's perspective, the properties of space are not given in advance once and for all, but can be discovered only step by step. Riemann's famous urging, in the last sections of his *Habilitationsvotrag*, to search the ground of measure relations in the "binding forces" (Riemann, 1854/1868, 149; tr. 1873, 37) that act upon space, could then be interpreted as the geometrical counterpart of Riemann's physical speculations about the propagation of all physical phenomena through infinitely near action in his *Fragmente* on *Naturphilosophie* (Riemann, 1876, 532-538; cf. Bottazzini, 1995).

2.2. **Riemann between Helmholtz and Christoffel.** The *Commentatio* remained unpublished until 1876, when Dedekind included it in the first edition of Riemann's collected works (Riemann, 1876). Dedekind, however, immediately published Riemann's *Habilitationsvortrag* in 1868 in the *Abhandlungen der Königlichen Gesellschaft der Wissenschaften* zu Göttingen (Riemann, 1854/1868). Hermann von Helmholtz had learned of Riemann's lecture in the same year from Ernst Schering, who had worked on Gauss'*Nachlass* together with Riemann and was a close friend of his: "the few indications you give of the results of the work that Riemann came to exactly the same conclusion as myself," Helmholtz wrote to Schering (Koenigsberger, 1906).

Encouraged by the agreement with Riemann's results, Helmholtz decided to present his own reflections of geometry to the public in an 1868 semi-technical talk, *Über die Tatsächlichen Grundlagen der Geometrie,* which would be published in the same year (Helmholtz, 1868a; cf. Volkert, 1993; also see Helmholtz, 1869), Helmholtz famously outlined the program of deriving Riemann's *hypothesis* that metric relations are given by a quadratic differential from Riemann's last restriction, the *fact* that their congruence does not depend on position: when two rigid point systems can be brought into congruence in a place, then they can be brought into congruence everywhere independently from the path along which they were led to each other (Helmholtz, 1868a, 198).

Helmholtz's published a lengthy mathematical proof a little later in his famous 1868 paper *Über die Thatsachen, die der Geometrie zu Grunde liegen* (Helmholtz, 1868b), the title of which clearly mimicked the title of Riemann's lecture. Roughly Helmholtz showed that translations and rotations of a rigid body—expressed analytically by $m(m-1)/2$ differential equations between m points—necessarily leave a quadratic differential form unchanged: "With this," Helmholtz concludes, "we have got to the starting point of Riemann's investigations" (Helmholtz, 1868b, 218; tr. 1977, 56). In this way, however, Helmholtz shifted the attention from points and the paths that join them to bodies and the volumes they fill Torretti, 1983, 238. Riemann's more general assumption that the *length of lines* is independent of position was substituted with the more restricted condition that *angles and sides of bodies* are independent of position, a condition that, as is we have mentioned, is satisfied by a more limited class a geometries.

The appearance of Riemann's paper, however, struck his contemporaries also for very different reasons. Dedekind mentioned Riemann's unpublished *Habilitationsvortrag* to Elwin Bruno Christoffel—his successor at the ETH in Zurich (Butzer, 1981). In 1869 in the celebrated Crelle's Journal, (Christoffel, 1869), Christoffel addressed the problem raised by Riemann, the equivalence of quadratic differential forms such as $F = \sum \omega_{ik} \vartheta x_i \vartheta x_k$, in the most general way, without focusing on the special case of the reducibility to an expression with constant coefficients (Christoffel, 1869, 46-47; cf. Ehlers, 1981). He introduced the three-index symbols $\left\{ \begin{matrix} il \\ r \end{matrix} \right\}$ later named after him (Christoffel, 1869, 49; the Christoffel symbols of the second kind)—which involve the first derivatives of the ω_{ik}—and from them derived the four-index symbol $(ghki)$ (Christoffel, 1869, 54; that corresponds to the four-index symbol introduced by Riemann and to our Riemann–Christoffel tensor).

Whereas Christoffel resorted to an abstract algebraic approach, the Bonn mathematician Rudolf Lipschitz—in the very same issue of Crelle's journal (Lipschitz, 1869), reached similar results by investigating the dynamics of a mechanical system: he found that if the "the quadrilinear form" Ψ_x vanishes (Lipschitz, 1869, 84), the differential form $ds^p = f(dx)$[1]—which enters into the definition of the inertial motion of particles—can be reduced to constant coefficients (Lipschitz, 1869, 75; cf. Lützen, 1995, 1999; Tazzioli, 1994).

Whereas Christoffel and Lipschitz remained outside the philosophical debate, in 1870 Helmholtz had already discussed the epistemological implications of his mathematical investigations in a lecture given at the University of Heidelberg, *Über den Ursprung und die Bedeutung der geometrischen Axiome* (Helmholtz, 1870/1876; a partial Eng. tr. appeared as Helmholtz, 1870). The paper includes, among others, the very famous convex mirror thought experiment, a sort of inversion of Beltrami's "interpretation" of non-Euclidean space with negative curvature in the interior of a Euclidean sphere (Beltrami, 1868a,b). Helmholtz shows that even if our rigid motions would appear distorted in the mirror (Helmholtz, 1870/1876, 44-45), the imaginary inhabitants of the mirror-world, by performing their measurements with equally distorted instruments, would not notice any difference. If they were able to converse with us, Helmholtz claims, "then neither would be able to convince the other that he had the true, and the other the distorted situation" (Helmholtz, 1870/1876, 45-46; tr. 1977, 20).

The thought experiment presupposes that in each space all parts, "if one disregards their limits, are mutually congruent" (Helmholtz, 1868b, 200-201; tr. 1977, 43). Helmholtz wanted to emphasize that a decision about which set of congruence

[1]Lipschitz considered a more general case of a differential form of the p^{th} root, whereas Riemann had only considered the case $p = 2$

relations is the "real" one cannot be made "*as long as we introduce no mechanical considerations*" (Helmholtz, 1870/1876, 45-46; tr. 1977, 20). Referring to the "investigations carried out by Prof. Lipschitz in Bonn" (Helmholtz, 1870/1876, 41-42, tr. 1977, 17) Helmholtz points out that if the inhabitants of the convex mirror were right—and we are living in a distorted non-Euclidean world—then we would be forced to introduce a non-Newtonian mechanics, where free force motions would still follow straight lines, but where the speed would depend on the position (Helmholtz, 1870/1876, 47, tr. 1977, 24).

Helmholtz's argument implies a sophisticated form of empiricism, in which, given a set of physical laws, the choice of geometry turned out to be non-arbitrary (DiSalle, 2006, 83). Its anti-Kantian implications—the possible discrepancy between the "transcendental" and physical notion of congruence Helmholtz, 1879, Beilage III—were mitigated by the fact that Helmholtz seems to consider the very existence of rigid bodies as a necessary condition of geometry (Helmholtz, 1879, Beilage II; cf. Friedman, 2002b). With isolated exceptions (Clifford, 1876), Riemann's speculations about variably curved spaces were never taken seriously in the 19th century (Hawkins, 1980, 2000). It was rather Helmholtz's approach that shaped the philosophical discussion, not without the resistance of professional philosophers, who—as Helmholtz confessed to Lipschitz—were often ready to "pronounce upon the most complex problems of the theory of space in the sure conviction of superior wisdom" (Helmholtz to Lipschitz, 2.3.1881 Lipschitz, 1986, 131).

3. The Development of the Helmholtzian and Riemannian Traditions in the 19th Century

3.1. **The Helmholtzian Tradition and the Emergence of Lie's Theory of Continuous Groups.** In 1883, Felix Klein urged his friend, the Norwegian mathematician Sophus Lie, to consider Helmholtz's geometric work (Stubhaug, 2002, 381) in the light of the theory of continuous groups, on which Lie had systematically started to work in 1870, playing a significant role in the emergence of Klein's own 1872 *Erlanger Programm* (Klein, 1872; cf. Rowe, 1989). In 1886, Lie was invited to Berlin to the Meetings of the German Natural Sciences: he gave a lecture entitled, *Bemerkungen zu v. Helmholtz Arbeit über die Tatsachen, welche der Geometrie zugrunde liegen* (Lie, 1886, 374), in which he pointed out several imprecisions in Helmholtz's demonstration, suggesting that Helmholtz's approach could be improved through group theoretical considerations (Stubhaug, 2002, 340).

In 1887, Poincaré—who had early insisted on the importance of the notion of a group (Poincaré, 1881, 1882, 1885, 1997; see also Lie's letter to Klein of October 1882, cited and translated in Rowe, 1985, 76)—referred to Lie's results in his paper on the foundations of geometry (Poincaré, 1887, 214). At the end of the paper, Poincaré also touches on the "celebrated *Memoire* of Riemann," in which every geometry is characterized "through the expression of the arc element as a function of the coordinates" (Poincaré, 1887, 214). However, he discarded it as geometrically irrelevant, because it allows for spaces which exclude "the existence of a group of motion which does not alter distances" (Poincaré, 1887, 214).

Lie did further work on Helmholtz's space problem, in some papers published in the early 1890s (see Lie, 1890a,b, 1892a,b). In a series of lectures he had given in Göttingen on non-Euclidean geometry (1889–90) (published as Klein, 1893), Klein failed to refer explicitly to Lie's result, arousing his angry reaction: "You still challenge me to destroy the Helmholtz's theory. I shall eventually do so. I also show ... that H[elmholtz]'s theory is basically false" (cited and translated in Rowe, 1988, 39).

In his 1893 lecture given in Chicago on the occasion of the World's Fair, Klein admitted that Lie's 1886 paper on Helmholtz "somehow escaped my memory" (Klein, 1894, Lecutre XL, 88). "These papers," Klein observed, "contain an application of Lie's theory of continuous groups to the problem formulated by Helmholtz" (Klein, 1894, Lecutre XL, 88). The motions of three-dimensional space with respect to a certain point can take ∞^6 of possible real values (a rigid body has six degrees of freedom) and form a group (two motions can be always replaced by a single one), that leaves invariant the distance between any two points p, p' given by $\Omega(p,\ p')$. Lie famously showed that, considering only a part of space surrounding the origin, if a $\frac{1}{2}n(n+1)$-parameter group (in an n-dimensional number manifold) can be defined, then the space is of maximal uniformity, that is, of constant or null curvature (on Lie's result see Klein, 1898).

This local result of Lie (which stands in sharp contrast with Klein's concern with the global Clifford–Klein problem; cf. Hawkins, 2000, 134) appeared in its definitive form in the last volume of his masterpiece, written with Friedrich Engel, *Theorie der Transformationsgruppen* (Lie, 1893, ch. 21). According to what Lie and Engel write in a footnote, Poincaré was not aware of Helmholtz's work in 1887 (Lie, 1893, 437, footnote). It is not clear when precisely Poincaré come to know Helmholtz's epistemological reflections (Heinzmann, 2001), but his name is mentioned in Poincaré's 1891 philosophical paper which is usually considered as the birth of geometric conventionalism (Poincaré, 1891, 774).

A year later (Poincaré, 1892), responding to the objections of George Mouret, Poincaré transformed Helmholtz's convex mirror thought experiment in his famous "parable" (Sklar, 1974, 91-93 and 113-115) of the heated sphere (cf. also Poincaré, 1895, 641–644), in which a non-Euclidean space is mimicked by a suitable temperature field, which uniformly distorts the motion of solids and the paths of light rays. The inhabitants of such a world might, however, decide to introduce a non-Maxwellian law of light propagation, in order keep the simpler Euclidean geometry (Walter, 2009). Thus the latter can always be "saved" if one, contrary to Helmholtz, is ready to change the "laws of physics."

In Poincaré's approach, also, a choice must be made between alternative, but unique sets of congruence relations (unique up to the choice of a unit of length): homogenous tessellations of space, each defined by the properties of the corresponding group of rigid motions. It is not by chance that Poincaré again explicitly excluded Riemann's approach based "on the manner in which the *length of a curve* is defined" (Poincaré, 1891, 773), being incompatible" with the "movement of *a solid body*" such as "we assume to be possible in Lie's theorem" (Poincaré, 1891, 773; 47). According to Poincaré, the geometries of Riemann, in the general case, "can be nothing but purely analytical, and would not lend themselves to proofs analogous to those of Euclid." (Poincaré, 1891, 773; 47)

3.2. **The Analytical Development of the Riemannian Tradition: Ricci and the Absolute Differential Calculus.** Roughly in the same years, Riemann's work had been in fact developed along the non-geometrical path put forward by Christoffel. In 1892, Gregorio Ricci-Curbastro had published in Darboux's *Bulletin* a first summary (Ricci-Curbastro, 1892) of nearly a decade of work on the equivalence of quadratic differential forms (Ricci-Curbastro, 1884, 1886, 1888, 1889). Ricci was able to systematize Christoffel's results into his "absolute differential calculus," (Bottazzini, 1999) as Ricci started to call it in 1893, alluding to the fact that it can be applied "independently of the choice of the independent variables" (Ricci-Curbastro, 1893, 1336, n.1).

Ricci's main interest was in the study of quadratic differential forms, such as "$\varphi = \sum_{rs} a_{rs} dx_r dx_s$," where the a_{rs} can be the conductivity coefficients in the analytical theory of heat, or the components of pressure in elasticity theory (Ricci-Curbastro, 1892, 167-168). Roughly, the problem was then to establish the laws according to which the coefficients a_{rs} change, by replacing the independent variables $x_1\ x_2,\ \ldots x_n$ with the variables $y_1\ y_2,\ \ldots y_n$ (which are smooth functions of the first ones) in such a way as to leave φ invariant. Ricci's main innovation was to interpret the three index-symbol $a_{rs,i}$ and the four index-symbol $a_{rt,su}$ (Ricci-Curbastro, 1892, 173) introduced by Christoffel as the result of a kind of differentiation more general than the usual one, that he referred to as "covariant differentiation." The case in which "$\sqrt{\varphi}$ is the expression of the line element of the space of three dimensions in curvilinear coordinates x_1, x_2, x_3" (Ricci-Curbastro, 1892, 167-168) is of course only a particular application of Ricci's mathematical apparatus.

In Padua, in 1899, Klein met Ricci's pupil Tullio Levi-Civita and asked him to publish, in the *Mathematische Annalen*, an organic and systematic account of Ricci's results (Levi-Civita to Arnold Sommerfeld 30.3.1899, Sommerfeld, 2000-2004, vol.1, 105). In 1900, Levi-Civita co-authored with Ricci the memoir "Méthodes de calcul différentiel absolu et leurs applications" in 1900 (Levi-Civita and Ricci-Curbastro, 1900), which was destined to become the manifesto of the absolute differential calculus. Ricci's results however, were initially dismissed as "useful but not indispensable" (Bianchi, 1902, 149), and remained substantially unnoticed (Reich, 1994, 77, but see Bottazzini, 1999). As is well known, it has been general relativity that showed the indispensability of Ricci's work and stimulated Levi-Civita's geometrical reinterpretation (see below §4.3). As we shall see, it is only from inside this tradition, which spread from Riemann to Levi-Civita, that the significance of general relativity for the history of the epistemology of geometry should be understood.

4. Einstein's Relativity Theories: Between the Helmholtzian and Riemannian Traditions

4.1. **Escaping from the Helmholtzian Tradition. Rigid Bodies and Special Relativity.**

Hermann Minkowski's (Minkowski, 1909) presentation of the Einstein–Lorentz electrodynamics of a moving body (Einstein, 1905) in terms of the group G_c (that leaves the quadratic differential expression $c^2t^2 - dx^2 - dx^2 - dz^2$ invariant) might be considered the first triumph of the group theoretical approach to physics (Walter, 1999)—a fact about which Minkowski's Göttingen colleague Klein was of course very pleased (Klein, 1910). It is however less appreciated in the philosophical literature that already special relativity unwittingly marked the beginning of the end of the Helmholtzian approach to the epistemology of geometry, via the discussion of the relativistic definition of rigid motion.

It was another Göttingen scientist, Max Born, who was the first to face the analytical problem of "defining rigidity by a differential law instead of an integral law" (Born, 1909, 3), by using a quadratic form of three spatial differentials $d\xi, d\eta, d\gamma$, ($ds^2 = p_{11}d\xi^2 + p_{22}d\eta^2 + p_{33}d\zeta^2 + 2p_{12}d\xi\ d\eta + 2p_{13}d\xi\ d\zeta + 2p_{23}d\eta\ d\zeta$) in which the coefficients $p_{\alpha\beta}$ are the "deformation quantities" (Born, 1909, 10). A motion of a filament in space–time will be rigid if $\frac{\partial p_{\alpha\beta}}{\tau} = 0$ (where τ is the proper time. Born then carried out the integration of these conditions for bodies for the case of a uniformly accelerated translation (Born, 1909, 15). Soon thereafter Paul Ehrenfest (Ehrenfest, 1909) pointed out that, assuming Born's relativistic definition of rigidity, it turns out that a rigid cylinder cannot rotate without violating Lorentz invariance (the Ehrenfest paradox). Gustav Herglotz (Herglotz, 1910) and Fritz Noether (Noether, 1910), Emmy Noether's brother) showed that Born's infinitesimal condition of rigidity implies that a rigid body has only three degrees of freedom.

Einstein had rapidly come to recognize that, according to the "investigations of Born and Herglotz," "in the theory of relativity there does not exist a 'rigid' body with six degrees of freedom" (an Jakob Laub, March 1910; CPAE, 5, Doc. 199, 232; cited and tr. in Stachel, 1989, 268). It was finally Max von Laue who, in 1911 (Laue, 1911), showed that actually special relativity does not allow at all for the usual concept of a rigid body: in a relativistic rigid body, "the number of kinematic degrees of freedom is infinitely great" (Laue, 1911, 86).

After all, the very notion of a rigid body also intuitively contradicted the special relativistic ban on superluminal signaling (Laue, 1911, 86): the motion of a part of a rigid body would instantaneously "signal" to all other parts of the body that they had to move, too. It was, however, not easy to avoid resorting to "rigid bodies" in a context where a coordinate system was still thought of as a rigid cubical lattice of meter sticks (and an array of clocks) that fill space–time. By discussing his celebrated "rigidly rotating disk" thought experiment (Stachel, 1989), Einstein seemed then to be ready to embrace the patent inconsistency that, "even though the rigid body cannot really exist," nevertheless "[t]he measuring rods as well as the coordinate axes are to be considered as rigid bodies" (Einstein, 1912, 131).

4.2. **Entering into the Riemannian Tradition: Rods and Clocks and General Relativity.** Einstein's rigidly rotating disc has been described as the "missing link" (Stachel, 1989) in the path that led to Einstein's "decisive idea" (Einstein, 1923b) to resort to Gauss's theory of surfaces; but also Born's work may have suggested to him that he consider quadratic differential forms with variable coefficients (Maltese and Orlando, 1995). Einstein was not "aware at that time of the work of Riemann, Ricci, and Levi-Civita" (Einstein, 1923b), to which Grossman famously introduced him in August 1912, starting the long journey that would lead to general relativity (similar statements can be found in Einstein's 1922 Kyoto address Abiko, 2000).

In a 1913 joint paper (Einstein and Grossmann, 1913), Einstein and Grossman famously outlined a theory of gravitation based the so called *Entwurf*-theory. In a paper published briefly thereafter (Grossmann, 1913), Grossman summarizes their collaboration as follows: "The mathematical core [Grundgedanke] of Einstein's theory of gravitation" consists in the idea "of characterising a gravitational field through a quadratic differential form with variable coefficients", $ds^2 = \sum g_{\mu\nu} dx_\mu dx_\nu$, whose coefficients $g_{\mu\nu}$ represent both the behavior of measuring rods and clocks with reference to the coordinate system, as well as the potentials of the gravitational field. "Of fundamental importance [Von grundlegender Bedeutung] in this respect [hierbei]," Grossman continues, "are the famous paper of Christoffel, *Über die Transformation ganzer homogener Differentialausdrücke* [Christoffel, 1869], and the paper, based on the latter, of Ricci und Levi-Civita, *Méthodes de calcul différentiel absolu et leurs applications* [Levi-Civita and Ricci-Curbastro, 1900]. In the latter work the authors developed the method for giving to differential equations of mathematical physics a form which is independent of the coordinate system" (Grossmann, 1913, 291).

Einstein's articles and research notes up to 1915 document how the search for the coordinate-independent partial differential field equations (cf. Renn, 2007, for a recent overall account) to determine the $g_{\mu\nu}$ had to overcome—besides "technical" problems related to finding a suitable two-index contraction of the Riemann-Christoffel tensor—also some "philosophical" difficulties that concerned primarily the physical significance of the coordinate system.

Einstein had to explain how to verify the values $g_{\mu\nu}$ predicted by the field equations through measurements with rods and clocks in a context in which the length of a measuring rod and the rate of a clock is not determined solely by the coordinate differentials but also by the quantities $g_{\mu\nu}$ (Einstein and Grossmann, 1913), that is, by the very quantities that should be measured. Einstein started to distinguish between "coordinate distances" (*Koordinatenabstände*) and "natural distances" (*natürliche Abstände*), as measured by rods and clocks (cf. Einstein, 1913, 490).

In a small enough (in astronomical proportions) region of space–time, where the $g_{\mu\nu}$ may be considered constant (i.e., taking on the Minkowski values) to a sufficiently accurate approximation, the coordinate differentials dX_ν can be measured directly by rods and clocks; ds, oriented in any way, can be calculated as the root of the sum of squares of the co-ordinate differentials. In a finite region, however, there is, in general, no choice of co-ordinates for which special relativity is valid: the distance ds is not determined solely by the sum of the coordinate differentials dx_ν of its end points, but the functions $g_{\mu\nu}$ must also be introduced if the measurements made by the rods and clocks are to yield the same invariant interval ds in every position and in every orientation.

It is in particular in the last sections of Einstein's systematic exposition of the *Entwurf*-theory, presented to the Berlin Academy, *Die formale Grundlage der allgemeinen Relativitätstheorie* (Einstein, 1914), that Einstein explicitly addressed the implications that the new theory can have for the "epistemology of geometry":

> Before Maxwell, the laws of nature were, in spatial relation, in principle integral laws: this is to say that distances between points finitely separated from one another appeared in the elementary laws. ... From this viewpoint, the propositions of geometry are to be considered as integral physical laws, since they deal with distances of finitely separated points. Through and since Maxwell, physics has undergone a throughgoing radical change in gradually carrying through the demand that distances of finitely separated points may no longer appear in the elementary laws, that is, "action at a distance theories" [Fernwirkungs-Theorien] are replaced by "local-action theories" [Nahewirkungs-Theorien]. In this process it was forgotten that also Euclidean geometry—as employed in

> physics—consists of physical propositions that from a physical viewpoint are to be set precisely on the side of the integral laws of the Newtonian point mechanics. In my opinion, this signifies an inconsistency from which we should free ourselves (Einstein, 1914, 1079-1080; tr. Ryckman, 2005, 63).

This passage might be considered the *coup de grace* for the "Helmholtzian tradition" in the epistemology of geometry. Helmholtz's approach is, by its very nature, a "distant geometrical" point of view, in which finite fixed distances are regarded as freely movable in space. General relativity, appropriating the mathematical apparatus that had emerged from the "Riemannian tradition," had fully implemented a near-geometrical point of view, by banning finite point-distances from the laws of nature, in which now only the distances ds of infinitely near points may occur.

In pre-general-relativistic theories, a coordinate system was thought of as a scaffold of congruent tiles made of rigid rods (and clocks) defined independently of the physical fields spread on it. In general relativity, the functions $g_{\mu\nu}$ that enter into the definition of ds themselves represent the potentials of a physical field, subject to partial differential equations and which contain at the same time all the information about the coordinate system. Only after having definitively renounced—via the so called "hole argument" (Norton, 1984; Stachel, 1980)—the independent physical role of a coordinate system was Einstein able to present his field equations in November 1915: "a real triumph," as Einstein put it, of the mathematical formalism developed through the work of Riemann, Christoffel, Ricci, and Levi-Civita (Einstein, 1915, 778).

It is worth noticing that such a formalism has only a tenuous connection to geometry. In particular, Einstein—even in his first systematic presentation (in 1916) of his newly completed general theory (Einstein, 1916)—does not refer to the "curvature of space–time." The Riemann–Christoffel tensor is introduced only as a merely analytical tool (Janssen, 1992; Reich, 1994, 204-5), in as much as it is the only tensor "which can be obtained from the fundamental tensor $g_{\mu\nu}$ by differentiation alone" (Einstein, 1916).

A "geometrical" issue emerges only when it comes to comparing the values of the $g_{\mu\nu}$ predicted by the field equations, (e.g. a spherically symmetric asymptotically flat solution Einstein, 1916, §22) with the values obtained by measurement. Roughly, the $g_{\mu\nu}$ can be found empirically as the factors by which the coordinate differentials dx_ν must be multiplied in order to ensure that ds^2 has the same value (normed as the unit interval) measured locally, in a flat region of space-time (Einstein, 1916, §4), in every position and in every orientation. In the case considered by Einstein (Einstein, 1916, §22), for a unit measuring rod put radially to the spherical field ($ds^2 = -1;\ dx_2 = dx_3 = dx_4 = 0$), we have $-1 = g_{11}dx_1^2$, whereas a tangential position ($dx_1 = dx_3 = dx_4 = 0$) it would measure $-1 = g_{22}dx_2^2 = -dx_2^2$. In the presence of a real gravitational field—if "the same rod independent of its position and its orientation can serve as the measure of the same extension" (Einstein, 1916, 820)—it is impossible to chose a coordinate system so that coordinate distances always correspond to real distances as in Euclidean geometry. Similarly, if one assumes that a unit clock at rest ($dx_1 = dx_2 = dx_3 = 0$) always measures the same $ds = 1$, the only non-vanishing coordinate differential dx_4 has to be multiplied by the correcting factor g_{44} ($1 = g_{44}dx_4^2$) (Einstein, 1916, 820).

Concretely, to directly measure the numerical value of the *ds*, we might use a small enough rigid rod, for instance, a rock salt crystal, and a fast enough uniformly-running clock, such as a cadmium atom emitting its red line. General relativity assumes that "the ratio between the wave length of the red cadmium line and the lattice constant of rock salt is an *absolute constant*" (Flamm, 1916, 451; my emphasis), which is not affected by the presence of the gravitational field. This is nothing else than the "operational" reformulation of the *Riemannian postulate* that the length of a line must be independent of its position. Once an atom has been chosen as unit clock, proper time *s* is calculated as the integral of the infinitesimal element *ds* along a time-like path.

Of course this lets emerge a difficulty of which Einstein became immediately conscious. The reproducibility of such complicated atomic structures in determinate circumstances—for instance in a strong electromagnetic field—is far from being obvious. However, the very possibility of making measurements in general relativity presupposes the constancy of the relative lengths of rods and the relative periods of clocks whatever gravitational or electromagnetic fields they have passed through. Einstein made this presupposition explicit as early as in a letter to Michele Besso in late 1916: "your observation about the equivalence of phyic[cally] different measuring rods and clocks (and subjected to different prehistories)," Einstein writes, "is fully correct" (CPAE 8, Doc. 270, 349; Einstein, 1972, 86). This is, so to say, the *fact* at the basis of the Riemann–Einstein geometry of space–time. The investigation of the epistemological status of this presupposition is precisely the fundamental problem around which the discussions on the role of rods and clocks in general relativity revolved.

4.3. **Beyond Riemann. Levi-Civita, Weyl, and the Geometrical Development of the Riemannian Tradition.** In 1917—at the request of the editor of the *Naturwissenschaften* —Moritz Schlick wrote a semi-popular two–part paper on relativity, destined to become a classic (Schlick, 1917). Relying on Einstein's insistence that the quantitative comparison of lengths and times is possible only by means "measuring-rods and clocks" (Schlick, 1917, 163, tr. 1920, 13), Schlick could effectively present general relativity to a philosophical audience as the heir of the well known discussion between Helmholtz and Poincaré on the role of rigid bodies in the epistemology of geometry. Of course Schlick was well aware that the notion of a rigid body was already modified by special relativity (Schlick, 1917, 182, tr. 1920, 55) and that it is completely meaningless in general relativity (Schlick, 1917, 183, tr. 1920, 55). However, Schlick could believe that the role which "rigid bodies" had played in the Helmholtz–Poincaré debate was simply taken up by the infinitesimal rigid rods (and clocks with infinitesimal periods) in Einstein's theory.

Although Einstein was enthusiastic about Schlick's paper (CPAE 8, Doc. 297; cf. Howard, 1984), the role of rods and clocks in general relativity is hardly understandable in the context of Helmholtz's philosophy of geometry and its conventionalist incarnation in Poincaré's work. The choice of a certain rod as a unit rod is of course arbitrary, but this is irrelevant: it only corresponds to the choice of the unit of measure in which we agree to express the lengths of lines. On the other hand, the fact that "two of our little rods can still be brought into coincidence [zur Deckung] at every position" in space (Einstein, 1917, 57; tr. 2005, 108), despite the different physical circumstances they have passed through, is not a matter of convention: it represents a central feature of the Riemann–Einstein geometry. It is only when Einstein's theory forced the mathematical community to let the geometrical content of the Riemannian tradition re-emerge after decades of abstract algebraic development that the problematic nature of Einstein's assumption could fully emerge.

In 1916/1917, Levi-Civita (Levi-Civita, 1916, but see also Hessenberg, 1917/18 and Schouten, 1918–1919), had famously recognized the geometric meaning of the Christoffel symbols and of the covariant differentiation: they determine the parallel displacement of vectors (Reich, 1992). As Hermann Weyl systematically showed in his 1917 lectures on relativity at the ETH Zurich (Weyl, 1918b, §15f.), the Ricci calculus can be translated into more intuitive geometrical terms (Reich, 1992).

Roughly, the line element ds can be seen as a vector the components of which are the coordinate differentials dx_ν relative to the chosen coordinate system. In a Euclidean region of space (where there exists a coordinate system in which the g_{ik} are constant), when a vector is parallel-transported around a loop it always returns to its original position with the same length and direction: the Christoffel symbols $\begin{Bmatrix} rs \\ i \end{Bmatrix} = \Gamma^i_{rs}$ (Weyl, 1918b, 99)—the components of the "affine connection," in the terminology that Weyl will soon adopt (Weyl, 1919b, 101)—can be made vanish identically. However, this property does not hold in the general case: the Riemann–Christoffel tensor R^i_{jhk} measures precisely the change in the direction resulting from parallel transport around the loop (Weyl, 1918b, §16; cf. Scholz, 1994, §4). Einstein, was extremely impressed by Weyl's geometrical derivation of the curvature tensor (cf. the letter to Weyl of 8.3.1918, CPAE, Doc 476, 670), which he praised in a enthusiastic review of Weyl's book (Einstein, 1918b).

Weyl also announced to Einstein (CPAE 8, Doc. 270) the draft of a 10 page paper (published after some difficulties as Weyl, 1918a), in which the disturbing asymmetry between length and direction in Levi-Civita's parallel transport (Afriat, 2009) could be dropped, in the name of a purely infinitesimal geometry (Weyl, 1918c). In this kind of geometry the determination of magnitudes does not take place directly at a distance: transporting a unit vector along a closed path, one might arrive at the end of the journey around a loop with a different transported measuring unit, depending on the path. Weyl introduced later the "length-curvature" tensor f_{ik} (*Streckenkrümmung*) as the measure of such a change of length, just like the Riemann "vector-curvature" tensor (*Vektorkrümmung*) measures the change of direction. Riemannian geometry is only a special case of a Weylean geometry where the $f_{ik} = 0$ (Weyl, 1919a, 109).

As is well known, Weyl suggested that, by dropping the last distant-geometric feature of Riemannian geometry, namely, the independence of the ratio of the measure-units from their position, a possible unification of "gravitation and electricity" might be achieved: when separated and brought together, rods and clocks would have different relative lengths and periods if they have passed through electromagnetic fields. Roughly, the gravitational field reveals itself in the non-integrability or path-dependency of direction, but the electromagnetic field reveals itself in the non-integrability of length (cf. e.g. Scholz, 1994, §5).

Against Weyl's "stroke of genius" (CPAE 8, Doc. 498, 710), Einstein raised a famous "measuring-rod objection" (CPAE 8, Doc. 498, 710; CPAE 8, Doc. 515), published as an addendum to Weyl's paper (Einstein, 1918a). If Weyl's theory were true, Einstein claimed, "the relative frequency of two neighboring atoms of the same kind would be different in general" (Einstein, 1918a, 40; tr. in O'Raifeartaigh, 1997, 35). Such behavior would contradict the empirical fact that energized atoms emit sharp, separated lines independently of their prehistory. Replying to Einstein, Weyl insisted that it is epistemologically unsatisfying to resort to the behavior of complicated atomic structures to measure the invariant ds. In fact, one can learn how a clock behaves, e.g., in a strong electromagnetic field, only from "a dynamical theory based on physical laws" (Weyl, 1918c, 479). Weyl did not intend to deny the physical behavior of atomic structure pointed out by Einstein, but to urge a dynamical explanation for the existence of such a "central office of standards" [Eichamt] (Weyl, 1919a, 103), for the fact that in Riemannian geometry "the measuring unit (the cm) can be chosen once and and all (of course the same in every position)" (Weyl, 1919b, 110).

Since Weyl could not rely on any dynamical models at that time, at the Meeting of Natural Scientists in Bad Nauheim in September 1920, he famously started to speculate that atoms might not really *preserve* their radius if transported, but *adjust* it every time anew to the space–time structure, in particular to the radius of curvature of Einstein's static spherical universe (Weyl, 1920, 650). The apparent Riemannian behavior of rods and clocks is then enforced by the physical mechanism of adjustment, making the "real" non-Riemannian geometry of space–time in principle unobservable.

In the discussion which followed Weyl's paper, Einstein complained that, if the "equality" of rods an clocks "would dependent on their prehistory," one would lose "the possibility of coordinating [zuzuordnen] a number, ds, to two neighboring points" (Einstein's reply to Weyl, 1920, 650). Renouncing such "empirically based coordination," the theory would be deprived of its empirical content. Einstein could, however not avoid acknowledging that it is a shortcoming of general relativity: it would be preferable if measuring rods are "introduced *separately*," but "constructed as *solutions* of differential equations" (Laue, 1920, 650).

The importance of this epistemological ideal and the impossibility of fulfilling it at the present stage of development of physics (cf. Fogel, 2008, ch. 3, 4) emerges in several parts of Einstein's correspondence with Weyl himself (CPAE 8, Docc. 472, 507, 512, 551, 661), with Walter Dällenbach (CPAE 8, Docc. 299, 863), Besso (CPAE 8, Doc. 604), Adriaan Fokker (CPAE 9, Doc. 76), and others (Goenner, 2004). It reveals Einstein's ambiguity between the provisional acceptance of rods as and clocks as metrical indicators (Howard, 1990, 1994) and his commitment to the higher epistemological standard on which Weyl had insisted (Ryckman, 2005, §1.4).

For our purposes, it is important to emphasize that this discussion about the role of rods and clocks in general relativity has of course nothing to do with the "Helmholtzian" consideration about the role of rigid bodies in the foundation of geometry, as Schlick thought: it pertains exclusively to the status of the "Riemannian" requirement that the lines, one-dimensional measuring threads, should be independent of their position (Weyl, 1919a, 102). The Helmholtzian standpoint presupposes that space is, as Weyl pictorially put it, a uniform "tenement house [Mietkaserne]" (Weyl's commentary in Riemann, 1919, 45, n. 6): it is a rigid scaffold of congruent tiles, which is independent of any physical process, and serves as a form for the phaenomena, a conception that general relativity made untenable. The Riemannian standpoint assumes on the contrary only the existence of a central "office of standards [Eichamt]" (Weyl, 1919a, 103), i.e., the global availability of the unit of measure.

The "conventionalist" agenda that Schlick by that time had treated systematically in his epistemological monograph (Schlick, 1918) confused two problems emerging from two traditions, the one that draws on Helmholtz and the other that goes back to Riemann, which must however be carefully distinguished. This can be seen by the high level of mathematical sophistication (Scholz, 2004) necessary to bring them together, a task that Weyl himself had started to work on in those years (Weyl, 1922, 1923, 1925/1988) in the attempt to single out group theoretically the Riemannian "class of Pythagorean metrics" from all possible classes of "Finsler metrics" (Finsler, 1918), as the one endowed with a a uniquely determined affine connection (Scholz, 2004), an attempt that, due to its philosophical inspiration (Ryckman, 2005, §6.3.2), remained mostly on the brink of a philosophical discussion (but see, e.g., Becker, 1923).

5. The Emergence of Logical Empiricism as a Progressive Blurring of the Helmholtzian and the Riemannian Traditions

Thus, the Einstein–Weyl debate had raised a central epistemological problem. As Arthur S. Eddington—the "apostle" of relativity to the English-speaking world—explains in the second edition (Eddington, 1920a) of his report on relativity (Eddington, 1918): "In Einstein's theory it is assumed that the interval *ds* has an *absolute value*, so that two intervals at different points of the world can be immediately compared" (Eddington, 1920a, X). Operationally this means that "atoms which are absolutely similar will measure by their vibrations *equal values* of the absolute interval *ds*" (Eddington, 1920a), that can be normed as the unit interval. The hypothesis that there are no systematic differences in the previous histories of different atoms of the same substance, even if they have passed through strong gravitational or electromagnetic fields, ensures that this norm can be replicated everywhere. It is however an assumption that depends on quantum theoretical considerations about the structure of matter, and thus which lie totally outside the macroscopic setting of general relativity: "The general course is to start with the 'interval' as something immediately measurable with scales and clocks," but, as Eddington points out, "in a strict analytical development the introduction of scales and clocks before the introduction of matter is—to say the least of it—an inconvenient proceeding" (Eddington, 1920b, 152).

5.1. **The Early Reichenbach's anti-Conventionalist Stance.** Schlick was then right to emphasize the crucial epistemological role played by rods and clocks in general relativity as the only means to coordinate the abstract mathematical apparatus of the theory with concrete physical reality. However, this role clearly cannot be understood within the framework set by the Helmholtz–Poincaré debate on the role of rigid bodies in the foundations of geometry. It is the Riemann–Weyl opposition that, historically and systematically, should be regarded as the correct setting in which to understand Einstein's own wavering attitude toward rods and clocks as guarantors of the physical content of his theory of gravitation.

The incompatibility of Einstein's new theory with 19th century conventionalism was simply, but masterfully, explained in the brief monograph on relativity, *Relativitätstheorie und Erkenntnis apriori* (Reichenbach, 1920b), that the young Hans Reichenbach—who had just participated in Einstein's 1919 summer term lectures on general relativity in Berlin[2]—had finished writing in June 1920 (cf. CPAE 9, Doc. 57, June 1920):

> It was from a mathematical standpoint asserted that geometry has only to do with conventional stipulations with an empty schema containing no statements about reality but rather chosen only as the form of the latter, and which can with equal justification be replaced by a non-Euclidean schema.* Against these objections, however, the claim of the general theory of relativity presents a completely new idea. *This theory makes the equally simple and clear assertion that the propositions of Euclidean geometry are just false*
>
> * Poincaré has defended this conception [Poincaré, 1902]. It is characteristic that from the outset he *excludes Riemannian geometry for his proof of equivalence*, *because it does not permit the shifting of a body without a change of form.* If he had known that it would be this geometry which physics would choose, he would not have been able to assert the arbitrariness of geometry (Reichenbach, 1920b, 104, n. 1; tr. 1965, 109, n. 1; translation modified; my emphasis).

In this well known passage, Reichenbach shows that conventionalism presupposes the validity of the Helmholtzian requirement that that the congruence of bodies is independent of position, it presupposes the existence of incompatible, but unique sets of congruence-relations, among which, once and for all, a choice can be made. For this reason, as we have seen, Poincaré had explicitly connected his conventionalism to the work of Lie. The notion of congruence naturally relates the concept of tessellation with the properties of a group of rigid motions mapping the space onto itself.

In Einstein–Riemann geometry, on the contrary, measurement is assured by the possibility of comparing *small measuring rods*. In the failure to extend the local Euclidean behavior of such rods over larger regions of space, Reichenbach points out, "*the invalidity of Euclidean geometry is considered proven*" (Reichenbach, 1921a, 383, tr. 2006, 42; my emphasis) at least if we attribute the length 1 to the rod in all positions and in every orientation. As Reichenbach rightly notices, this impossibility makes manifest "the *absolute character of the curvature of space*" expressed by the Riemann–Christoffel tensor. (Reichenbach, 1920b, 31, tr. 1965, 33; my emphasis)

Reichenbach has thus separated what Schlick had irremediably confused: the Helmholtzian measuring procedure based on the free mobility of finite bodies and the Riemannian one on the transportability of small one-dimensional rods. As Reichenbach knows perfectly well, however, this latter assumption is far from being obvious: "Weyl's generalization of the theory of relativity," Reichenbach writes, "abandons altogether the concept of a definite length for an infinitely small measuring-rod" (Reichenbach, 1920b, 73, tr. 1965, 76), so that "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, 1920b, 87, tr. 1965, 91; translation modified)

Reichenbach is aware that this point can be easily understood by resorting to Levi-Civita's notion of the parallel transport of vectors. This is not surprising since in the lectures on relativity that Reichenbach had followed in Berlin, Einstein

[2]The Hans Reichenbach Collection at the University of Pittsburgh HR 028-01-04 and 028-01-03

had used for the first time the concept of parallel displacement in introducing the Riemann tensor (CPAE 7, p. 11, note 179). Reichenbach summarizes:

> Weyl's theory represents a possible generalization of Einstein's conception of space which, although not yet confirmed empirically, is by no means impossible ... In *Euclidean geometry* a vector can be shifted parallel to itself along a closed curve so that upon its return to the point of departure it has the same direction and the same length. In the *Einstein–Riemannian geometry* it has merely the same length, no longer the original direction, after its return. In *Weyl's theory* it does not even retain the same length. This generalization can be continued. If the closed curve is reduced to an infinitely small circle, the changes disappear. The *next step in the generalization* would be to assume that the vector changes its length upon turning around itself. There is no "most general" geometry (Reichenbach, 1920b, 76, tr. 1965, 79; my emphasis)

Put in these terms, it appears clear that the transportability of infinitesimal rigid rods is the distinctive feature of the entire class of Riemannian geometries if considered as a special case of a more encompassing class (such as that of Weyl geometries). It has therefore nothing to do with the problem of the choice between Euclidean and non-Euclidean geometry (which is between special cases of Riemannian metrics) as in classical conventionalism.

As we have seen, general relativity assumes the Riemannian behavior of clocks, the relative periods of which is the always the same whenever they are brought together; Weyl suggested that "the frequency of a clock," might be "dependent upon its previous history" (Reichenbach, 1920b, 77, tr. 1965, 79)

The fact that this is contradicted by our knowledge of the behavior of clocks does not necessarily contradict Weyl's theory, since it could be assumed that the influences suffered by the clocks "compensate each other on the average"; hence "the experiences made until now, according to which, say, the frequency of a spectral line under otherwise equal conditions is the same on all celestial bodies, can be interpreted as approximations" (Reichenbach, 1920b, 77, tr. 1965, 79) Thus the real geometry of the world might be Weyl's geometry, in spite of the empirically observed Riemannian behavior of rods and clocks.

Reichenbach was however skeptical about Weyl's presumption to deduce the real geometry of space from an epistemological principle, the principle of the relativity of magnitude: "Weyl's generalization must be investigated from the viewpoint of a physical theory, and only experience can be used for a critical analysis" (Reichenbach, 1920b, 73, tr. 1965, 77) especially if one consider that Weyl's geometry is not even the only possible non-Riemannian geometry that one can possibly take into consideration. Reichenbach sent his book to Weyl who responded privately (Rynasiewicz, 2005) and publicly by arguing that his theory did not make any pretense to deduce the non-integrability of lengths by pure reason, but only show that "it must be understood as the *outflow* [Ausfluß] *of a law of nature*" (Weyl, 1921c, 475; emphasis mine). As we have seen, it is precisely this alternative that plays a central role in Einstein's epistemological reflections about geometry.

5.2. **Einstein's Misleading Reference to Poincaré in *Geometrie und Erfahrung*.** It has puzzled historians that Reichenbach was ready to give up his convincing analysis of the role of rods and clocks in general relativity and embrace conventionalism after having exchanged a few letters with Schlick (Schlick and Reichenbach, 1920–1922). In the absence of additional information, one can only speculate that the publication of Einstein's celebrated *Geometrie und Erfahrung* (Einstein, 1921, the expanded version of a lecture given on 27 January 1921) might have played a relevant role in convincing Reichenbach of the correctness of Schlick's epistemological stance.

In his celebrated lecture, Einstein, after having introduced, with a reference to Schlick's "book on epistemology [Schlick 1918]" (Einstein, 1921, 5; tr. 1954, 235) a rigid distinction between "axiomatic geometry" and "practical geometry," claims in fact that without a sort of "Helmholtzian" (see below on p. 24) approach to geometry as a natural science concerning the physical behavior of practically-rigid bodies, he "should have been unable to formulate the theory of relativity" (Einstein, 1921, 6-7; tr. 1954, 235) . On the other hand, however, Einstein recognized that Poincaré was in principle, *sub specie aeterni*, right by claiming that it is always possible to make rigid bodies agree with any kind of geometry by changing the physical laws that govern their behavior Einstein, 1921, 8; tr. 1954, 236). Only the sum geometry plus physics, $G+P$ as Einstein famously concludes, can be compared with experience Einstein, 1921, 7s. tr. 1954, 236; cf. Friedman, 2002a).

However, on closer inspection the reference to Poincaré in Einstein's *Geometrie und Erfahrung* is highly misleading (Friedman, 2002a). The problem Einstein was addressing had clearly nothing to do with the Helmholtz–Poincaré alternative between the empirical or conventional choice between Euclidean and non-Euclidean sorts of Riemannian geometries. Even if the name of Weyl is not mentioned, Einstein was concerned with a feature of Riemannian geometries as such, precisely that feature of which Weyl's work had revealed the contingency (Ryckman, 2005, §4.5).

As we have seen, measurement in general relativity presupposes that if two rods that "are found to be equal once and anywhere, they are equal always and everywhere" (Einstein, 1921, 9 ; tr. 1954, 237). The same assumption must be made for clocks, or, more specifically, for atomic clocks: "The *existence of sharp spectral lines* is a convincing experimental proof" (Einstein, 1921, 9; tr. 1954, 238; my emphasis) of their Riemannian behavior. Against this assumption it could be objected that rods and clocks are complex atomic structures, the behavior of which Einstein, 1921, 9; tr. 1954, 237 cannot be simply read off from observation, but is of necessity "theory laden."

Einstein could then recognize that someone is *sub specie aeterni* right who, like Weyl or Eddington, argues that rods and clocks are not "irreducible elements" but "composite structures," which must "not play any *independent part*" in theoretical physics /Einstein, 1921, 8; tr. 1954, 236; my emphasis). However, Einstein could also point out that, *sub specie temporis*, i.e., "*in the present stage of development* of theoretical physics," he was justified from starting from the plausible assumption about their behavior, even if it is external to the framework of general relativity (Einstein, 1921, 8; tr. 1954, 237; my emphasis).

The issue at stake is then the presupposition that the ratio of clock periods and rod lengths is an absolute constant. Precisely this, Einstein observes, "enables us to speak with meaning of the mensuration, *in Riemann's sense of the word*" (Einstein, 1921, 8; tr. 1954, 237; my emphasis).

Thus the reference to Poincaré cannot be taken "literally," but must be interpreted at most as an analogy. One can assume as *a fact*, *? la* Helmholtz, that the ratio of two atoms is the same whenever they are compared, as Einstein did, or decide, to adopt a sort of Poincarean strategy, to save non-Riemannian geometry, by blaming the apparent Riemannian behavior of atomic clocks on the dynamical mechanism of an adjustment to the world curvature (Weyl, 1921a,c).

Einstein's formula $G + P$ seems then to allude to such a strategy of "doubling geometry" (Vizgin, 1994, 146): an unobservable "world geometry"—as Eddington called it (Eddington, 1921)—which is not "the geometry of actual space and time" (but from which the unification of electromagnetism and gravitation may be achieved) is introduced on a deeper level of stratification with respect to the "natural geometry," which is the "geometry of Riemann and Einstein, not Weyl's generalized geometry" (Eddington, 1921, 121).

5.3. **Schlick's Influence on Reichenbach's Conversion to Conventionalism.** The background of *Geometrie und Erfahrung* (cf. Ryckman, 2005, §3.5) must not have been not difficult to guess even if Einstein did not mention his real interlocutors by name. Schlick's review of *Geometrie und Erfahrung* (Schlick, 1921), clearly shows that he was aware of it. Schlick points out that Einstein's theory of measurement presupposes that "two measuring rods are *always* and *overall* equally long, if they *once* and *somewhere* were found as equal" (Schlick, 1921). As Schlick remarks, this condition is "confirmed by experience," even if Weyl has "tried to drop it" (Schlick, 1921). Thus, Schlick was not only as well-aware of Weyl's theory as was Reichenbach, and but also that this theory was the polemical goal of Einstein's lecture. However, unlike Reichenbach, never attempted a real confrontation. On the contrary, Schlick, supported by Einstein's flattering reference to his book in *Geometrie und Erfahrung*, could apparently not restrain himself from taking Einstein's reference to Poincaré as a confirmation that the Helmholtz–Poincaré debate was the right framework from which the new theory could be understood as he had suggested.

Schlick's *Erläuterungen* to Helmholtz's writings on geometry (Helmholtz, 1921, tr. Helmholtz, 1977)—which he edited in 1921 together with Paul Hertz in 1921—show how Einstein's formula $G+P$ could be integrated with apparent success into the framework of the discussion of Helmholtz and Poincaré of the role of rigid bodies in the foundations of geometry (Pulte, 2006): Helmholtz claimed that which bodies are "actually" rigid depends on the laws of physics, so that "real" physical geometry is discovered by empirical investigation; Poincaré objected that one can always change the laws of physics, in order to preserve the simplest Euclidean geometry. In reality it should be argued—"as by Einstein in *Geometrie und Erfahrung*" (Schlick's commentary in Helmholtz, 1921, **; n. 31; tr. 1977, 31; n. 31)—, that one chooses certain bodies as rigid, i.e. which geometry holds in the actual world, if this choice leads to the simplest possible physics.

Ironically, one can look at Paul Hertz's *Erläuterungen* to Helmholtz's more technical papers to understand why Schlick's appropriation of Einstein's formula $G+P$ was based on a misunderstanding. Schlick seems to have not given enough philosophical relevance to the difference between "Riemann [who] gives an elementary law (differential law)" and "Helmholtz [who] conversely formulates his axioms for systems of finitely separated points" (Hertz's commentary in Helmholtz, 1921, 57; n. 1; tr. 1977, 58; n. 1)[3]. One has to distinguish on the one hand the "Helmholtzian

[3]It might be interesting to compare this passage with that of Einstein's quoted above on p. 11

axiom"—which was "criticized and completed by Sophus Lie with help of group theory" (Hertz's commentary in Helmholtz, 1921, 57; n. 1; tr. 1977, 58; n. 1)—according to which the volume of *finite bodies* does not depend on position, from that of Riemann in which only that the length of *infinitesimally rigid rods* does not depend on position. It is worth quoting Hertz's commentary at length:

> We therefore have to emphasize, as an especially important feature of *Helmholtz's axiom*, the following proposition: two point systems which once coincide can also be brought into coincidence in every other situation, even when each is connected to another system. As is known, Riemann considered a more general case: finite rigid bodies need not always be movable, but infinitely small bodies should be able to go anywhere; thus, in other words, the axioms stated here by Helmholtz should hold for infinitesimal rigid bodies: there should be a line element independent of the path. In this [Riemann's axiom] Weyl* sees a vestige of prejudices about geometry "at a distance". The length relationship between two extensions could depend upon the path along which an infinitesimal comparing rod was brought from one to the other. H. Reichenbach** can already seek a further generalization: that namely a material extension, after a rotation about itself, might no longer coincide with the same extension as previously (Hertz's commentary in Helmholtz, 1921, 57; n. 1; tr. 1977, 58; n. 1).
>
> * See e.g. Raum, Zeit, Materie [Weyl, 1921b], §16; Math. Zeitschr. 2 (1918) [Weyl, 1918c]. ** Relativitatstheorie und Erkenntnis a priori ['Relativity theory and a priori knowledge'], Berlin, 1921, p. 76. [Reichenbach, 1920a, 76][4]

As we have seen, Einstein in *Geometrie und Erfarung*—as Schlick himself noticed in his review—was precisely concerned with the validity of Riemann's axiom, since, needless to say, Helmholtz's axiom is not valid in spaces of variable curvature.

It is then surprising that Reichenbach himself—who Hertz mentions for having proposed a possible extension of Riemannian geometry—in his review of the Schlick–Hertz edition of Helmholtz's writings, though praising the technical parts of Hertz's presentation (Reichenbach, 1921b, 422), could see the main result of Helmholtz's reflections on geometry in the connection "of the axiom of congruence with the behavior of rigid bodies; even Poincaré," Reichenbach concludes, "has not expressed conventionalism more clearly" (Reichenbach, 1921b, 421).

5.4. **Reichenbach's Capitulation to Schlick's Conventionalism.** Reichenbach's capitulation to Schlick's philosophy could have not been more unconditional, as one can realize reading between the lines of his beautiful 1922 overview of contemporary philosophical discussions about relativity (Reichenbach, 1922a). Reichenbach awkwardly tries to convince himself and his readers that his previous opinion that the metric "expresses an objective property of reality," "does not contradict conventionalism" (Reichenbach, 1922a, 356; tr. 1978, I, 34f). In 1920 he simply "*forgot* to add the proposition of the *definition of the metric through rigid bodies*" (Reichenbach, 1922a, 356; tr. 1978, I, 34f; my emphasis). When this definition is added, one has to agree with Schlick that "a metric emerges only after the physical laws have been established (the P of Einstein's formula)"(Reichenbach, 1922a, 356;

[4]The footnotes are Paul Hertz's. Reichenbach's passage to which Hertz refers us was quoted above on p. 18

tr. 1978, I, 34f); in principle one can change the metric "provided one changes the laws of physics correspondingly" (Reichenbach, 1922a, 356; tr. 1978, I, 34f).

Thus, Reichenbach took it for granted that Einstein's formula $G + P$ should be read the context of 19th century conventionalism: In principle it is always possible to make rigid bodies agree with any kind of geometry (G) we please by changing the physical laws (P) that govern their behavior, for instance by introducing a "field of force" (Reichenbach, 1922a, 365-366) that suitably deforms all our measuring instruments. Anticipating the main lines of his future philosophy of geometry, Reichenbach reached the conclusion that "[d]epending on the choice of the field of force, one gets a different geometry" (Reichenbach, 1922a, 365-366). In this sense, Reichenbach points out, "material objects do not define a single geometry, but a "*class of geometries; this is precisely the meaning of conventionalism*'. By changing the definition of rigidity, a chagne that can be interpreted as the effect of a force,'[o]ne obtains then a Riemannian geometry of different measure-determination". (Reichenbach, 1922a, 365-366).

This understanding of Einstein's formula, however, is rather puzzling. It is sufficient to consider the point that Reichenbach made immediately thereafter. Reichenbach remarks that the entire class of Riemannian geometries—of both the Euclidean and the non-Euclidean sort—"is based on an axiom, that denotes *an empirical fact* [einen empirischen Tatbestand]," the axiom that "two natural measuring rods, which can be brought to superposition once, can be superposed again after they have been transported along different paths." The assuption of the "transportability of rods" is "*the axiom of the class of Riemannian geometries"*, that is "the possible geometries according to Einstein" (Reichenbach, 1922a, 366).

After Weyl's clarification (Weyl, 1921c, 475),[5] Reichenbach started to recognize that one of Weyl's main achievements is precisely to have shown that "the *axiom of the Riemann class* for natural measuring rods" is not"*logically* necessary," (Reichenbach, 1922a, 368). While Einstein had "simply *accepted* the univocal transportability of natural measuring-rods" as a given fact, Weyl urged "that the validity of this axiom," should be "understood as an outflow of a law of nature" (Reichenbach, 1922a, 368). Reichenbach did not find Weyl's explanation of the univocal "transportability [Uebertragbarkeit] through the adaptation of the measuring-rods to the curvature of the world" (Reichenbach, 1922a, 368, n. 1) satisfying. However, he had to admit that Weyl's speculative argument had clearly put the finger on a hidden presupposition that Einstein's theory had uncritically assumed.

What Reichenbach, however, failed to appreciate is that Einstein's $G+P$ should be understood precisely on the background of Weyl's (and Eddington's) strategy of "doubling geometry," by opposing the apparent Riemannian natural geometry, forced by mechanism of the adjustment, to the real non-Riemannian ether or world-geometry and not to the choice between classes of Riemannian geometries, between the Euclidean and the non-Euclidean geometries. By the contrary, by 1922 (Reichenbach, 1922b) the essential lines of Reichenbach's conventionalism were already written in stone. The solution of the problem of space, Reichenbach claims, is to be found "only in this conception we call conventionalism, which goes back to *Helmholtz* and *Poincaré*" (Reichenbach, 1922b, 40; tr. 2006, 135).

Again, Reichenbach could consider this conclusion acceptable only by blurring what Hertz had called the "Helmholtzian axiom" with the "Riemanniann axiom".

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[5]See above on p. 18

On the one hand he believes that the main philosophical lesson of general relativity is that "[t]he definition of congruence is ... *arbitrary*, and what is congruent in one geometry is not necessarily congruent in another" (Reichenbach, 1922b, 33; tr. 2006; 127; my emphasis). On the other hand Reichenbach also points out that "[t]his 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, 1922b, 35; tr. 2006; 129; my emphasis).

In this way, however Reichenbach completely confuses what he had so carefully distinguished in his first 1920 monograph. There cannot be any *univocal* definition of *congruence* of *bodies* in Riemannian manifolds of variable curvature, where the "Helmholtzian axiom" does not hold, and two bodies that are congruent here, in general, cannot be reproduced in another location. In all Riemannian manifolds (with variable or constant curvature), however, there is an arbitrary, but univocal definition of the *unit of length*: two unit rods have the same length wherever they are compared.

As Reichenbach rightly points out, although rather in passing, in his more technical 1924 monograph (Reichenbach, 1924), in this respect Weyl had raised a relevant philosophical problem. He realized that we cannot simply accept this "fact" as a fortunate circumstance: "it cannot be an *accident* that two measuring rods are equal at every place in a neighborhood comparison" (Reichenbach, 1924, 64; tr. 1969, 91; my emphasis); Weyl had therefore rightly required that "this fact must *be explained*" (Reichenbach, 1924, 64; tr. 1969, 91; my emphasis). A similar passage from a 1925 paper (Reichenbach, 1925) of Reichenbach makes this point eloquentely:

> The word adjustment, first used in this way by Weyl, is a very good characterization of the problem. It *cannot be a coincidence* if two measuring rods placed next to each other are of the same length regardless of their location; it *must be explained* as an adjustment to the field in which the measuring rods are embedded as test bodies. Just as a compass needle adjusts to its immediately surrounding magnetic field by changing its direction, measuring rods and clocks adjust their units of measure to the metric field. ...
>
> Of course, the answer can only arise from a *detailed theory of matter* about which we have not the least idea; it must explain why the accumulation of certain field loci of particular density, i.e., the electrons, express the metric of the surrounding field in a simple manner. The word "adjustment" here thus *only means a problem without providing an answer* ... Once we have this theory of matter, we can explain the metrical behavior of material objects; but at present the explanation from Einstein's theory is as poor as Lorentz's or the classical terminology. (Reichenbach, 1925, 48; my emphasis; tr. 2006, my emphasis)**

It is difficult to disagree with Reichenbach's remark that Weyl's speculative theory of the adjustment of rods and clocks to the world curvature only allows the problem to emerge, but it does not furnish a plausible solution. However, it is just as hard not be puzzled by the fact that Reichenbach did not realize that it was precisely *this* problem that Einstein intended to address in *Geometrie und Erfahrung*: at the present stage of development of physics, in absence of a theory of matter, general relativity can only assume as a fortunate circumstance that the dimensions of those atomic structures that we use as rods and clocks do not depend on what happened to them in the past. This is indeed a poor explanation as Reichenbach rightly notices; in principle it would be preferable to have theory that can account for this remarkable behavior. Einstein mentioned Poincaré not in order to support classical conventionalism, but to allude to Weyl's and Eddington's conceptually more rigorous, but physically less advantageous attitude toward the relations between geometry and physics (Eddington, 1923, ch. 7).

6. The Assessment of Reichenbach's Conventionalism as a the Merging of the Helmholtzian and the Riemannian Traditions

6.1. **Einstein's Rhetorical Use of the Helmholtz-Poincaré Opposition.** Einstein had returned to the issue of the epistemological status of rods and clocks again in his July 1923 Nobel prize lecture. He insisted that "it would be *logically more correct* to begin with the *whole of the laws*" (Einstein, 1923a, 3; tr. 1923/1967, 484; my emphasis) and not with an "artificially isolated part" such as rods and clocks (Einstein, 1923a, 3; my emphasis). However, "we are not ... sufficiently advanced in our knowledge of Nature's elementary laws to adopt *this more perfect method*" (Einstein, 1923a, 3; tr. Einstein, 1923/1967, 484; my emphasis). Einstein here is more explicit that in *Geometrie und Erfahrung* about who he was referring us to: "At the close of our considerations we shall see that in the most recent studies there is an attempt, based on ideas by Levi-Civita, Weyl, and Eddington, *to implement that logically purer method*" (Einstein, 1923a, 3; tr. 1923/1967, 484; my emphasis), in their attempt "to replace Riemannian metric geometry" (Einstein, 1923a, 3; tr. 1923/1967, 484; my emphasis) with a more general one from which the "identity between the gravitational field and the electromagnetic field" might be derived (Einstein, 1923a, 3).

There is then evidence, Einstein's philosophical reflections on the role of rods and clocks in general relativity, dispersed in several writings of that time, must be understood against this background. In a 1924 review (Einstein, 1924) of a book by a minor Neo-Kantian (Elsbach, 1924; cf. Howard, 1990, 2010), Einstein distinguishes two different "standpoints" on the question about the relation between geometry and experience: according to "standpoint A," the "concept of the interval corresponds to something experiential"; on the other hand, according to "standpoint B," to the "practically-rigid measuring body is accorded no reality" "only geometry with physical sciences taken together" can be compared with experience (Einstein, 1924, 1690-1691). In a brief paper published one year later, *Nichteuklidische Geometrie und Physik* (Einstein, 1925), Einstein expressly attributed the standpoint A to Poincaré and standpoint B to Helmholtz (Einstein, 1925, 253, tr. in Pesic, 2007, 161).

Einstein did not hide his sympathy for the latter, without whom, he claimed, "the formulation of relativity theory would have been practically impossible" (Einstein, 1925, 253, tr. in Pesic, 2007, 161). The Logical Empiricists could apparently find here the umpteenth confirmation of their reading of Einstein's work. However, once again, Einstein's praise of the Helmholtzian attitude toward geometry—that nearly literally reproduces his appreciation for practical geometry in *Geometrie und Erfahrung*[6]— cannot be taken at face value. It is the "chain of thought

[6]See above on p. 19

Weyl–Eddington–Schouten" (letter to Besso,5.6.1925 Einstein, 1972, 204) to which Einstein—with growing skepticism—was alluding.

The names of Helmholtz and Poincaré are used to symbolize in a brief non-technical account published on a literary magazine, the *Deutsche Literaturzeitung*, two different approaches toward the relationship between geometry and physics. Einstein had assumed a sort of "Helmholtzian" attitude, by accepting 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 and Eddington had challenged him from a more critical "Poincaréian" point of view, by raising a *questio iuris* question, by asking with what right one admits such a presupposition in general relativity, if the latter has nothing to say about the behavior of rods and clocks.

As Einstein put it in an article of the *Encyclopedia Britannica*, there are the "consistent thinkers" (like Weyl or Eddington), who rightly consider it preferable "to allow the content of experience [Erfahrungsbestände] to correspond to geometry and physics conjointly" (Einstein, 1926, 609). On the other hand, there are those who, like Einstein himself, at the present time stick to a more pragmatic interpretation of geometry as the study of the "laws regulating the positions of rigid bodies" (Einstein, 1926, 609).

The reference to the role played by "rigid bodies" in geometry must not be understood in the framework of 19th century conventionalism. It refers again to "the Riemannian restriction"—as Eddington calls it in the same 1926 edition of the *Encyclopedia Britannica*, (Eddington, 1926, 907)—about the the result of the comparison of units of length at a distance: "The assumption of Riemannian geometry," Eddington writes, "is that the result does not depend on the intermediate steps the route of transfer" (Eddington, 1926, 907). As Eddington points out, this is however "an assumption, not an *a priori* necessity" (Eddington, 1926, 907). One may claim that such an assumption is experimentally well confirmed, so that "the geometry of space and time is strictly Riemannian as Einstein supposed" (Eddington, 1926, 908). However, the "experimental verification only extends to a limited degree of accuracy" (Eddington, 1926, 907). It should be preferable to regard the "Riemannian restriction" as "a deduction not an axiom of our geometry" (Eddington, 1926, 908).

6.2. **Reichenbach on Riemann and Helmholtz.** It is interesting to notice that Reichenbach, in his classic *Philosophie der Raum-Zeit-Lehre*, which may have already been finished in 1926 (cf. the letter to Schlick cited in Schlick, 2006, vol. 6, 175), was completely aware of the fact that "the comparison of two units of lengths at different locations" (Reichenbach, 1928, 24-25; tr. 1958, 17) is a problematic issue. Reichenbach's analysis reveals how here "definitions and empirical statements are interconnected" (Reichenbach, 1928, 24; tr. 1958, 16) The choice of a rod as unit rod is of course conventional, we could agree on the Paris meter or on something else. However, Reichenbach pinpoints that "it is an *observational fact* [beobachtbare Tatsache], formulated in an *empirical statement* [Erfahrungssatz]" (Reichenbach, 1928, 24; tr. 1958, 16; my emphasis) that two measuring rods equal in length in Paris will be found equal at every other spatial point. It would be possible to imagine a world, in which "[i]f two of these copies were transported and compared locally, they would be different in length" if compared elsewhere (Reichenbach, 1928, 24; tr. 1958, 17). In such a world "a special definition of the unit of length would have to be given for every space point." (Reichenbach, 1928, 24; tr. 1958, 17; my emphasis) We simply happen to live in the fortunate world in which units of length are reproducible everywhere.

As Reichenbach points out, this *"physical fact makes the convention univocal* [eindeutig]," i.e., independent of the path of transportation (Reichenbach, 1928, 24; tr. 1958, 17; my emphasis). The choice ofthe unit of measure is a matter of convention, but the statement about the "univocalness [Eindeutigkeit] of the convention is ... *empirically verifiable* and *not a matter of choice*" (Reichenbach, 1928, 25; tr. 1958, 17; my emphasis). Reichenbach refers us to §46 of the book for more clarification, a reference that, curiously, survived in the English translation, where, however, §46 was suppressed. It is precisely there that Reichenbach discusses at length Weyl's theory, that is, the theory that revealed the contingency of this empirically verifiable fact.

As we have seen, the error at the basis of Reichenbach's geometrical conventionalism can be easily seen in an attempt to extend this reasoning to the notion of congruence. In the general case of a world endowed with a geometry of variable curvature, there is no "univocal," i.e., position-independent, definition of congruence on which all its inhabitants may come to agree: two finite bodies which are congruent in a flat part of space, cannot have this congruence reproduced in a curved part of the same space. In such a world there might be, however, a univocal definition of the unit of measure: the inhabitants of different regions of the world may agree in using the same units, so that they can compare the length of lines that they measure. If they find the length in question is 5, they may decide once and for all whether 5 centimeters, 5 kilometers, 5 light years, etc., are meant.

Again, Reichenbach confuses the "Hemlmholtzian axiom" of the independence of the *congruence of bodies* from position, with the "Riemannian axiom" of the independence of the *length of lines*, chains of rods, that connect two points, from position. Unfortunately, Reichenbach elevates this confusion to a philosophical agenda: "[w]hile *Riemann* prepared the way for an application of geometry to physical reality by his mathematical formulation of the concept of space. *Helmholtz* laid the philosophical foundations" (Reichenbach, 1928, 48; tr. 1958, 35). In particular "he recognized the connection of the problem of geometry with that of rigid bodies" (Reichenbach, 1928, 48; tr. 1958, 35). For this reason, according to Reichenbach, "Helmholtz's epistemological lectures must therefore be regarded as the source of modern philosophical knowledge of space" (Reichenbach, 1928, 48; tr. 1958, 35)

The Helmholtzian and Riemannian traditions have been blurred into one single line of development: "The solution to the problem of space described here is to be attributed principally to the work of *Riemann, Helmholtz, Poincaré, and Einstein*" (Reichenbach, 1929b, 60; tr. Reichenbach, 1978, 179; my emphysis). General relativity, however, cannot be regarded as the heir of this mathematical tradition simply because, as we have tried to show, such a tradition never existed. Poincaré explicitly excluded the Riemannian geometries of variable curvature from his conventionalism, precisely because they were at odds with the Helmholtzian axiom based on the existence of rigid bodies and its group theoretical definition furnished by Lie. Reichenbach not only completely neglected the group theoretical implications of the work of Helmholtz and of Poincaré (Friedman, 1995), most of all he did not appreciate philosophically the fact that Riemann's work evolved along a different non-geometrical tradition whose geometrical significance re-emerged only after general relativity in Levi-Civita's notion of the parallel transport of vectors.

Precisely because he did not appreciate the difference between these two traditions, Reichenbach could simply delude himself that, even if "there are not rigid bodies" in general relativistic space–time, the "definition" of congruence can be redirected to "*infinitesimal measuring instruments*" (Reichenbach, 1928, 286; tr. 1958, 250). However, we clearly cannot stipulate arbitrarily which rods of infinitesimal lengths are rigid and which clocks of infinitesimal period can be regarded as ideal clocks Torretti, 1983, 238-239. As we have seen, general relativity is built precisely on the Riemannian assumption that the relative lengths and periods of infinitesimal rods and clocks are not affected by the presence of a gravitational field. Again, Reichenbach's geometrical conventionalism confuses the definition of congruence with the choice of the unit of measure, with the norm of the ds as the unit interval, two issues that paradoxically appear clearly distinct in Reichenbach's own semi-technical presentation. Which atomic period we use as a unit clock, "[f]or instance, the linear unit may be defined by means of the wave length of the red cadmium line," (Reichenbach, 1929b, 30; tr. 1978, I, 161) is of course conventional. One can use another atom, let's say an atom of sodium with a yellow emitting line, but this would leave the geometry of space–time unaffected. On the other hand, that the relative periods of atomic clocks is the same wherever they are compared, is a *fact*, which "is taught to us by experience," it is the fact at the basis of Riemannian geometry: "For the fact that the objects in question are similar is, of course, *not established by definition* but is a *fact that must be discovered*" (Reichenbach, 1929b, 30; tr. 1978, I, 161).

The possibility of measurements in Riemann–Einstein geometry presupposes this empirically discovered "special property" of our rods and clocks that have always the same relative length and relative rate of ticking wherever they are compared, so that "there is no need to store a special unit at a definite location." It is a property that characterizes all Riemannian geometries (Euclidean and non-Euclidean). The fundamental question is then the following: "What would happen if the measuring rods would not posses the mentioned *special property* [Vorzugseingeinschaft]?" (Reichenbach, 1928, 332).

Reichenbach unfortunately raised this question precisely in the above mentioned §46, of the appendix of his book (Reichenbach, 1928, 331-373), a paragraph that did not even make it to the English translation. However, this was exactly the question Einstein, Weyl, and Eddington were addressing in their debate about the role of rods and clocks in general relativity. In spite of Einstein's, didactically effective, but misleading references, the framework from which this issue can be understood is not provided by Helmholtz's and Poincaré's appeal to rigid bodies in geometry, but by Levi-Civita's re-geometrization of Riemann's work via the notion of parallel transport of vectors. It was Weyl's attempt—which Reichenbach discussed at length in the appendix—to put the direction and the length of vectors on an equal footing (Afriat, 2009) that let emerge the contingency of a central feature of Riemannian geometry from which the very destiny general relativity as a physical theory stands or falls.

7. Einstein's Late Reflections on Rods and Clocks in General Relativity and his Last Dialogue with Reichenbach

7.1. **Einstein, Weyl and Eddington after 1930.** By that time, after the discovery of an "absolute length" $\frac{h}{mc}$ in the Dirac theory of the electron, Weyl had actually already completely abandoned his 1918 gauge theory (Weyl, 1931). The material electron had assumed the role of world-radius as an absolute standard of measure (Weyl, 1934). It began to become clear (London, 1927) that the gauge invariance ties the electromagnetic field to the Schrödinger–Dirac field of the electron ψ (Dirac, 1928a,b) and not to the gravitational field g_{ik} as Weyl had originally thought (Weyl, 1929a,b). Weyl's invitation to abandon all "geometrical capers [Luftsprünge]" (Weyl, 1931, 343); cf. Scholz, 2006), was, however, not followed by Einstein, who, as is well known, in the subsequent years tried several geometrical paths in the search for a unified theory of gravitation and electromagnetism, which invariably led to a dead end.

Reichenbach himself had presented, in two very readable semi-popular papers (Reichenbach, 1929a,c), Einstein's last attempt in which another parallelism, the "Fernparallelismus" (Sauer, 2006), takes the place of the usual "Riemannian" Levi-Civita's parallelism, introducing a curvature-free, but non-Euclidean space–time, with non vanishing torsion (the measure of the closure failure of a parallelogram made up of two vectors and their reciprocal parallel transports).

Around 1930, Einstein himself presented his theory to a larger public in some semi-popular contributions (Einstein, 1929, 1930a,b,c), in which he showed increasing epistemological confidence in the power of mathematical speculation (Dongen, 2010). Material particles should appear as solutions of the field laws (portions of the field with high density without singularities), from which ideally also the behavior of rods and clocks as atomic structures should be derived. However, Einstein admits, "[a]s long as the questions are not satisfactorily solved, there will be a justified doubt as to whether such far-reaching deductive methods may be granted to physics at all" (Einstein, 1930a,b,c, tr. in Pesic, 2007, 177)

To describe the actual provisional status of theoretical physics, Einstein resorted again to the language of "rigid bodies." In his 1933 Herbert Spencer lecture at the Oxford University, Einstein makes clear that, epistemologically, theoretical physics "is really exactly *analogous* to Euclidean geometry," which is a formal axiomatic system on the one hand, but on the other might be regarded as "the science of the possibilities of the relative placing of actual rigid bodies" (Einstein, 1933, 165). It is against the background of such a "*logical parallelism* of geometry and theoretical physics," (Einstein, 1933, 165), that Einstein insists that it has been a "fatal [verhängnisvolle] error"—as he put it in *Physik und Realität* (Einstein, 1936)—that this conception of geometry as a branch of physics "has fallen into oblivion" (Einstein, 1936, 321, tr. 356). (Einstein, 1936, 321, tr. 356).

The epistemological problem Einstein was addressing is put in an equally popular but more direct way in Eddington's 1938 lectures at Trinity College, Cambridge:

> We notice that relativity theory has to go *outside its own borders* to obtain the definition of length, without which it cannot begin. It is the microscopic structure of matter which introduces a definite scale of things Since we have *separated molar physics from microscopic physics* primarily out of consideration of the grossness of our sensory equipment, it would be unreasonable to expect to find it complete in itself. We can

> only make it logically complete as far as the point where its roots stretch down into *physics as a whole*.
> ...
> The secret of the union of molar and microscopic physics—of relativity theory and quantum theory—is *"the full circle"*. ... Generally we enter on the circle at the junction now under discussion, where relativity theory takes its standard of length from quantum theory (Eddington, 1939, 76-77)

As we have seen, general relativity must at last rely on a "quantum-specified standard ... reproducible at the remotest times and places" (Eddington, 1939, 79). But it is at least plausible that "the quantum-specified standard does not provide an exact definition of length in strong electric or magnetic fields," since it not exactly reproducible in those circumstances (Eddington, 1939, 79). It is "quantum theory" that must "ultimately be able to calculate precisely how much a crystal standard expands or contracts when placed in a magnetic field, or how a wave-length is modified" (Eddington, 1939, 81). Only from the standpoint of "physics *as a whole*" (Eddington, 1939, 76), as a union of molar and microscopic physics, can we then expect an epistemologically satisfying solution of the problem. At the present state, however, "appeal must be made to quantum theory for the definition of the interval ds, which is the starting-point of relativity theory" (Eddington, 1941, 693).

In those years, e.g., in his Yale lectures in 1930–1931 (Weyl, 1932) and in *Mind and Nature* 1934 (Weyl, 1934), Weyl also never tired of emphasizing that, in principle, "only the theoretical system as a whole" should "be confronted with experience" (Weyl, 1932, 78), a system in which "all parts of physics and geometry finally coalesce into one indissoluble unity" (Weyl, 1934, 45). In his 1949 English augmented-translation (Weyl, 1949) of his 1927 monograph (Weyl, 1927), Weyl insisted that the behavior of rods and clocks "by which Einstein measures the fundamental quantity ds^2" should come out "as a remote consequence of the fully developed theory" (Weyl, 1949, 288). Geometry and physics can only be "put to the test as a *whole*" (Weyl, 1949, 134).

7.2. **Einstein, Reichenbach and the Anonymous non-Positivist.** In his *Autobiographical Notes* (Einstein, 1949a) for Schilpp's Library of Living Philosophers, which came out in the same year, Einstein again seems to embrace in princinple such an epistemological ideal, although defending the legitimacy of his more opportunistic attitude. As Einstein famously pointed out, "in a certain sense, is inconsistent," to consider measuring rods and clocks "as theoretically *self-sufficient entities*" (Einstein, 1949a, 57); "strictly speaking" they "would have to be represented as *solutions* of the basic equations (objects consisting of moving atomic configurations)" (Einstein, 1949a, 57).. However, he still pragmatically recognizes that "it was better to permit such inconsistency—with the obligation, however, of eliminating it at a later stage of the theory" (Einstein, 1949a, 57).

The issue Einstein was raising, as he makes particularly clear in the final "Remarks," (Einstein, 1949b) were the same he had been concerned with thirty years earlier:

> Everything finally depends upon the question: Can a spectral line be considered as a measure of a "proper time", (*Eigen-Zeit*) $ds = g_{ik}dx_idx_k$ (if one takes into consideration regions of cosmic dimensions)? Is there such

> a thing as a natural object which incorporates the "natural-measuring-stick" independently of its position in four-dimensional space? The affirmation of this question made the invention of the general theory of relativity *psychologically* possible; however this supposition is *logically not necessary* (Einstein, 1949b, 685; last emphasis mine)

The fact that general relativity still assumes that there are "physical objects, which (in the macroscopic field) measure the invariant *ds*", shows that "a complete theory of physics as a totality . . . does not yet exist" (Einstein, 1949b, 685); according to Einstein, in a complete theory "the objects used as tools for measurement do not lead an independent existence alongside of the objects implicated by the field-equation" (Einstein, 1949b, 685)

Because of this, it was not particularly enlightening that Einstein, addressing Reichenbach's discussion (Reichenbach, 1949) of the question if "geometry . . . verifiable (viz., falsifiable) or not," resorted to a imaginary dialogue that opposes "Reichenbach, together with Helmholtz" on the one hand and Poincaré on the other (Einstein, 1949b, 677-678). As is well known, the anonymous "Non-Positivist"—who substitutes for Poincaré by the end of the dialogue—escapes this alternative by supporting the holistic claim that "no 'meaning'"—in Reichenbach's sense (meaning=verifiability)—can be attributed to "the individual concepts and assertions" but only "to the entire system," (Einstein, 1949b, 677-678), i.e., only to "*the completely developed theory* of relativity (which, however, does not yet exist at all as a finished product)" (Einstein, 1949b, 677-678). It is of course not clear for whom the non-Positivist stands (Howard, 1990), but it seems not overwhelmingly speculative to argue that the anonymous interlocutor shares Weyl's and Eddington's epistemological stance.

The words with which Weyl, in late 1950 (Weyl, 1951), on the occasion of 50 years of general relativity, sketched his debate with Einstein in the 1920s seems to confirm this reading. Even if Weyl has no intention to difened a "theory in which I no longer bilieve", he still argues that the theory had put general relativity in front of a legitimante epistemological problem: "the *definition*" of the metric field "with help of rods and clocks" can of course "only be regarded as a temporary connection to the experience." In principle, "it must be *derived*" from the laws of physics, "in which relation the measurement results which are read off from those bodies stay to the fundamental quantities of the theory" (Weyl, 1951, 81).

The contemporary reaction of logical empiricists to Einstein's epistemological remarks shows, however, that they still read them in the context of the age-old, but still discussed problem (Hempel, 1945) of the choice between Euclidean or non-Euclidean geometries. Reichenbach considered Einstein's position as a "witty defense of conventionalism" (Reichenbach, 1951, 135), to which he answered resorting again to Helmholtz's and Poincaré's conceptual resources, from which he defended his "verifiability theory of meaning" (Reichenbach, 1951). Rudolf Carnap's 1956 preface to the English translation of Reichenbach's classical 1928 monograph (Reichenbach, 1958) considers the main achievement of Reichenbach precisely in having established the connection of Poincaré's work to that of Einstein (also see Carnap's 1958 seminar on the foundations of physics published as Carnap, 1966).

7.3. **The Discussion on the "Riemannian axiom" in the 1960s.** It is then not surprising that the entire long chapter dedicated to Weyl's theory was omitted by Maria Reichenbach and John Freund in their widely read English translation of

Reichenbach's masterpiece (Reichenbach, 1958) and remains untranslated to this days (Coffa, 1979). This decision was of course comprehensible. Weyl's theory in his original form was physically untenable, as Weyl himself did not hesitate to admit in occasion of the reiussue of his 1918 paper (cf. addenum to Weyl, 1918c, published in Weyl, 1956, 192). However, the fact Weyl's theory was regarded as philosophically and epistemologically uninteresting is the sign of a more fundamental misunderstanding, a misunderstanding that, given the enormous influence of Reichenabach's book, might have not been free of consequences on the later development of the philosophical investigation of space-time theories. Weyl's failed 1918 attempt to unify gravitation and electricity had raised a fundamental epistemological question about the status of the nothing but obvious Riemannanian behavior of rods and clocks which provides general relativity with its empirical content. This issue continued to play a not negligible role, in the foundational discussions of general relativity in the physical community in the subsequent decades.

Just after the publication of Reichenbach's book in English, the Irish physicist John Lighton Synge, making a "a plea for chronometry" (Synge, 1959), suggested introducing as the basis of general relativity the chronometric version of the "*Riemannian hypothesis*" (Synge, 1960, 105), that is the "chronometric assumption which makes space–time Riemannian" (Synge, 1960, 107): the ratio of frequencies of atomic clocks is a *"natural constant,"* independent of the world-line on which the observations are made (Synge, 1960, 106).

At about the same time, however, convinced of the epistemological shortcomings of this approach, Robert F. Marzke (Marzke, 1959) and, a little later, Wolfgang Kundt and Dieter Hoffmann (Kundt and Hoffman, 1962) attempted to construct clocks using the reflection of light between two world-lines, without resorting to complicated atomic structures (Marzke, 1959). As Marzke explains in 1964, in a paper written with John A. Wheeler (Marzke and Wheeler, 1964), a theory should be able to describe the behavior of its own probes. "The conceivability of alternative theories," such as Weyl geometry, raised the question, "[h]ow accurately has the *Riemann postulate* been tested" (Marzke and Wheeler, 1964, 59). General relativity assumes that "the ratio" of world-lines "is independent of the choice of route of inter comparison" (Marzke and Wheeler, 1964, 58), but, from the epistemological point of view, the inter-comparison should "be carried through without any recourse to measuring rods or clocks of atomic constitution" (Marzke and Wheeler, 1964, 62). General relativity, "in and by itself" should provide "its own means for defining intervals of space and time" without leaning on quantum theory. In this spirit, Ehlers, Pirani, and Schlild (Ehlers, Pirani, and Schild, 1972) had famously suggested the use of light rays and freely falling particles (following Weyl, 1921d) instead of rods and clocks; however "an additional *Riemannian axiom*" (Ehlers, Pirani, and Schild, 1972, 82) was still needed, to force the *Streckenkrümmung* to vanish everywhere.

Thus, this debate reproduced at a higher level of sophistication the technical and philosophical issue (Carrier, 1990; Coleman and Korté, 1995) that Einstein, Weyl, and Eddington had faced some decades earlier. This confirms beyond a reasonable doubt that precisely the role of the "Riemannian axiom" was at the heart of Einstein's reflections about rods and clocks: hence these reflections do not bear any connection with the problem of whether the choice among various different Riemannian geometries is conventional or empirical, as logical empiricists believed, a problem which, on the contrary, make sense only under the assumption of the "Helmholtzian axiom" of free mobility. It is again a sign of a curious misunderstanding that Reichenbach's well-informed 1922 remarks on the Weyl-Einstein-Eddington debate were omitted also from the 1878 English translation of his philosophical writings (Reichenbach, 1978) because—as the editors surprisingly point out— it was "of no historical importance" (Reichenbach, 1978, I, 38)

8. Conclusion

Riemann's and Helmholtz's two brief writings on the foundation of geometry, although ??? they had both undeniably an enormous influence on the history of the sometimes stormy relationship between geometry and physics, undergo different destinies. The intuitive attractiveness of Helmholtz's conviction that spatial measurement necessarily presupposes the rigid motions of bodies with the volumes they fill initially hindered the full appreciation of Riemann's more abstract and general assumption that it is sufficient to assume the length of the paths between any two points—relative to some standard interval—can be univocally determined.

Helmholtz's approach was in fact immediately extremely successful; developed technically by Lie's work on continuous group, it was brought at the center of the philosophical scene by the elegant prose of Poincaré's popular writings; on the contrary, Riemmann's approach somehow evolved subterraneusly from the inconspicuous work of Christoffel, to Ricci's and Levi-Cvita's absolute differential calculus, and never made the headlines of the philosophical debate. It was general relativity that rescue this mathematical tradition from the oblivion to which it would have probably been consigned otherwise.

Using Logical Empiricism as a case study, this paper has tried to show that the profound difference between these two traditions was never fully appreciated in the philosophical debate a misunderstanding that had longstanding consequences on the history of philosophy of science. Whereas the emergence of general relativity can be described as a process of progressive "emancipation" from the distant-geometrical approach that dominated the "Helmholtzian tradition" to let emerge the near-geometrical implications of the "Riemannian" one, early logical empiricists' philosophy of space and time emerges on the contrary as as a sort of unfortunate "collision"—to borrow Norton's term (Norton, 1999)—of the "Riemannian" and the "Helmholtzian" tradition. The logical empiricists tried to interpret Einstein's new approach to geometry and physics, the very expression of the Riemannian tradition, projecting it on the background the Helmohltzian one.

With the mathematical technique developed by Riemann, Christoffel, Ricci and Levi-Civita Einstein's theory of gravitation inherited the tacit assumption, that, once a unit of measure has been chosen once and for all, the length of world-lines can be attributed a unique value, independent of the coordinate system chosen. Einstein's insistence against Weyl on the indispensability of rods and clocks in general relativity pivoted precisely on the status of this "Riemannian assumption," that the ratio of any two world-lines is an absolute constant. Schlick and Reichenbach, however, mislead by some of Einstein's remarks, tried to interpret Einstein's rods-and-clocks parlance under the light of the "Helmholtzian assumption," of the relation between congruence and the free mobility of rigid bodies. Helmholtz's requirement that the congruence of bodies should be independent of position was irremediably blurred with Riemann's idea that the length of lines should be independent of position. To put it more intuitively: spatial measurements do not require rigid bodies, but only inextensible threads (Freudenthal, 1956, 374, see also Torretti, 1999, 163)

Abbreviations

CPAE Albert Einstein (1987-). *The Collected Papers of Albert Einstein.* Ed. by Diana Kormos Buchwald. 13 vols. Princeton University Press.

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