“But One Must not Legalize the Mentioned Sin”. Phenomenological vs. Dynamical Treatment of Rods and Clocks in Einstein’s Thought

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Abstract

The paper offers a historical overview of Einstein’s oscillating attitude towards a ‘phenomenological’ and ‘dynamical’ treatment of rods and clocks in relativity theory. Contrary to what it has been usually claimed in recent literature, it is argued that this distinction should not be understood in the framework of opposition between principle and constructive theories. In particular Einstein does not seem to have plead for a ‘dynamical’ explanation for the phenomenon rods contraction and clock dilation which was initially described only ‘kinematically’. On the contrary textual evidence shows that, according to Einstein, a realistic microscopic model of rods and clocks was needed to account for the very existence of measuring devices of *identical construction* which always measure the *same* unit of time and the *same* unit of length. In fact, it will be shown that the empirical meaningfulness of both relativity theories depends on what, following Max Born, one might call the ‘principle of the physical identity of the units of measure’. In the attempt to justify the validity of such principle, Einstein was forced by different interlocutors, in particular Hermann Weyl and Wolfgang Pauli, to deal with the genuine epistemological, rather then physical question whether a theory should be able or not to described the material devices that serve to its own verification

Keywords: Albert Einstein, Relativity Theory, Rods and Clocks, Hermann Weyl, Wolfgang Pauli, Constructive Relativity, Conformational Holism

> Intrinsically Brobdingnag and Lilliput are precisely the same; it needs an intruding Gulliver—an extraneous standard of length—to make them appear different
>
> A. S. Eddington

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1. Introduction

In the last years, mainly thanks to Harvey Brown's seminal work (for the most part collected in Brown, 2005), increasing attention has been drawn to Einstein's 'self-confessed sin' of treating rods and clocks as 'primitive', 'self-sustained', or 'unstructured' entities. Brown rightly insisted that Einstein was admittedly aware of the fact that rods and clocks are actually 'structured dynamical entities', 'composite bodies', the behavior of which depends on the structural properties of the forces responsible for the microstructure of matter. Therefore—as Einstein put it paradigmatically in an often quoted passage from his self-written 'obituary' for the volume edited by Paul Arthur Schilpp for the library of Living Philosophers—the 'sin' of treating rods and clocks as ultimate elements incapable of further explication should not be 'legalized' (Einstein, 1949a, 59). It was a temporary expedient, necessary only because physics at that time did not posses the adequate conceptual tools to construct rods and clocks explicitly from the fundamental physical quantities in the theory, from fields alone, or in worst case scenario, from fields and particles together (Barbour, 2007, 587)

Einstein's waving attitudes towards the role, indispensable or provisional, of rods and clocks in both of his theories, has been usually cast in the well-known opposition between 'constructive' and 'principle' theories, which Einstein had explicitly introduced in 1919 (Einstein, 1919b), but addressed in several occasions starting at least from 1907 (see Howard, 2005, for more details). In Einstein's original stance toward special relativity as a 'principle theory', the geometry of space-time appeared to be 'defined' through the behavior of 'rods' and 'clocks', whose contractions and dilations were postulated, without introducing a realistic microscopic model of their material constitution. However, when a suitable 'constructive theory' of matter would eventually be at hand, rods and clocks will be thought as rather complicated physical systems obeying fundamental dynamical laws. Symmetry properties of space-time will turn to be nothing but a codification of the symmetries of the laws governing matter. In particular 'length contraction' and 'time dilation' in special relativity should be described in the final analysis as a consequence of the dynamical laws that govern rods and clocks.

Brown's non-standard approach to special relativity has been challenged from different sides. John Norton (Norton, 2008) and Michel Janssen (Janssen, 2009), to mention only the more influential reactions, have defended the correctness of what one might call the 'mainstream' view that Lorentz invariance reflects the symmetry of Minkowski space-time, so that rods and clocks exhibit their characteristic behavior because the laws of nature governing their functioning conform to symmetries of space-time. However, at first glance, this approach seems to fail to make sense, or at least to address, the many passages in which Einstein, until the end of his life, insisted again and again that rods and clocks should be treated as solutions of the dynamical equations, and not introduced separately as structureless entities.

This paper has not the ambition to solve the vexed question of the "arrow of explanation in special relativity" (Gorski, 2010), whether it is the behavior of matter and fields which explains the symmetries of space-time or if it is the other way around. It would rather attempt to take cue from this debate to offer an overall historical survey of Einstein's repeated concerns about a tension between 'phenomenological' and 'dynamical' treatment of rods and clocks. The paper will therefore follow as close as possible the historical development (or rather, as we shall see, the surprising constancy) of Einstein's stance towards this issue in published and unpublished writings. In particular the paper will document the context in which Einstein, probably, at the beginning of 1917, seems to have firstly expressed in private correspondence his concerns about the use of complicated material systems as measuring devices (section 2). It will consider the circumstances in which Einstein in the 1920s, pressed by the epistemological objections raised by different interlocutors, in particular Hermann Weyl (Scholz, 2007) and the young Wolfgang Pauli (Stachel, 2005), felt the urgency to articulate his point of view in public writings (section 3). Thereby, he outlined a two-stage epistemological strategy, waving between a provisional and a principled attitude toward the use of rods and clocks in relativity theory (section 4), to which he remained faithful until the end of his life (section 5). Even if other interlocutors were worth being taken into consideration (in particular Arthur Stanley Eddington), we decided to concentrate on the German debate, which offers for obvious reasons, a particularly rich dialogical network made of private correspondences, public confrontations, and philosophical disputes.

Some of this material has been analyzed in some excellent historical and philosophical literature (Stachel, 1989; Howard, 1994; Ryckman, 2005; Howard, 2007; Fogel, 2008) to only mention a few. However, as far I can see, there is no overall historical-critical overview of Einstein's stance towards this issue over the years. This is unfortunate since the question wether the behavior of measuring devices should be derived from the theory equations and or stipulated independently has been often discussed in the recent philosophical debate by appealing to a limited group of passages written by Einstein in disparate occasions. As, I will try to show, if inserted in their proper historical setting, Einstein's dissatisfaction toward his initial 'operational' treatment of rods and clocks and his correspondent appeal to a 'dynamical' explanation seem never actually address the question in which the contemporary debate is mostly interested: wether the space-time lives a 'parasitic' existence at the expense of the dynamical phenomena of rods contraction and clock dilation, or whether on the contrary rods contract and clocks slowdown because they adapt to the structure space-time. In particular there is no textual evidence that Einstein ever presented in his arguments in favor of dynamical explanation of rods and clocks in the context of his opposition between constructive and principle-theories, so that constructive relativity would finally 'dynamically' explain, what relativity as a principle theory had described only 'kinematically'.

It is undeniable that Einstein expressed early on the conviction that a dynamical account of rods and clocks was needed in special as well as in general relativity. However, not in order to explain rods contractions and clock dilations. What needed to be 'dynamically' explained, was precisely the very opposite fact that, put it in a somehow provocative way, there exist clocks which *do not* slow down and rods which *do not* shorten in any circumstances. More precisely, in special as well as in general relativity, it is more or less tacitly assumed that *identically constructed* rods and *identically constructed* clocks, which measure respectively the *same length* and show the *same rate of ticking* when they are held side by side at one location at a certain time, they will always agree on their readings, if carefully put next to each other at rest again *whatever their intermediate histories might have been.* In other terms a dynamical explanation should account for the very existence of 'good' rods and clocks of 'identical construction' which always measure the same true 'proper time' and 'proper length' whatever it might have happened to them in the past.

Relativity theory does not give any clue for assuming that physical systems with these characteristics really exist in nature nor does it tell us how to recognize and identify them if they did. Rather, one has to proceed at least to a tentative and provisional identification. For instance experience furnishes sufficient reasons to regard two atoms of the same substance as identical clocks. After it has been 'stipulated' that atoms could play the role of good clocks (i.e. that the frequency of an atom is always its proper frequency), then it is expected that these systems should behave according to the predictions of the theory. In this sense the theory is said to be empirically testable. In particular, if we compare the rates of two *identical atomic clocks*, moving with respect to one another, special relativity predict that we should observe the transverse doppler effect; similarly if we compare the rates of two *identical atomic clocks* moving along different trajectories in a gravitational field, according to general relativity we are except to observe the gravitational redshift.

However, it is also clear that these two effects are taken to be an empirical confirmation of the time dilation effects predicted by the theory only if the assumption is granted that *two atoms of the same substance are identical clocks*, which, if held side by side in relative rest, always reliably measure the same proper time. The problem emerges when one realizes that such material devices are rather complicated physical systems, which behave in conformity to some dynamical laws. Thus, without an adequate knowledge of such laws, there is, no way to tell in advance, if a given clock is in fact a ideal clock, whose functioning is not significantly affected by the presence, of accelerations, electromagnetic fields, or by the gravitational field itself. Thus, it can the be objected, that what we actually compare with experience is not simply the predicted *geometrical* effect of time dilation, but the latter together with the *physical* laws governing the natural process that we have chosen as a clock.

It this sense, Einstein expressed early on the conviction that in relativity theory, as in any physical theory, one seems to have been entitled to the right to expect a dynamical explanation of how the measuring instruments work, possibly without calling on other branches of physics. The initial 'black-boxing' of rods and clocks as unstructured entities was only a provisional, even if probably unavoidable, compromise, a 'sin' that should however not be remitted. In principle, the dynamics of both the rods and clocks and the other physical systems should be describable within the framework of relativity theory. As we shall see, however, what requires a dynamical explanation is not the physical process of, say, slowing down of clocks, but the very existence of 'identically constructed' (*gleich beschaffene*) clocks which always measure the same 'true' proper time in every circumstances.

In oder to properly appreciate this point, which has been sometimes misunderstood in the current philosophical debate, it is in my opinion important to realize, that the entire discussion of the role of rods and clocks in relativity theory turned on a tacit, but fundamental assumption. Max Born has once aptly called it (although only in passing and only referring to special relativity), the "*the principle the physical identity of the units of measure*" (Born, 1920, 191; my emphasis; tr. Born, 1922, 211). The importance of such principle, in spite of some illuminating works on this issue (Pierseaux, 2003; see also Pierseaux, 1999), has not be sufficiently recognized in philosophical literature (Brown mentions it, but only in a footnote Brown, 2005, 80, fn. 41). However, I would venture to claim that the destiny of special and general relativity as physical theories—and not only as mathematical manipulation of quantities—depends *entirely* on the validity of such principle, which should be actually considered one of the central issue, if not the central issue of the epistemology of relativistic geometry. In order to make relativity theory physically meaningful, we have to assume the actual existence in nature of physical processes, permitted by some physical laws, which can be used to define and reproduce the units and lengths and time.

Thus, the epistemological question emerges wether a theory should be able to describe material objects and processes that serve as its means of verifications, a position shared by most relativists, or wether by the opposite approach, such description should lie outside the domain theory, a position that, as it can be easily guessed, prevailed among physicists working on quantum mechanics. Einstein's oscillation between 'phenomenological' and 'dynamical' treatment of rods and clocks should be see as the attempt of positioning himself between these two extremes, that can be considered as paradigmatically incarnated by Weyl and Pauli. The real meaning of Einstein's arguments, as often happens in the history of science, can be understood only when one realizes in response to whom they were formulated (for the 'dialogical approach' to the history of science, cf. Beller, 1999). From this point of view, Einstein's unease toward the treatment of rods and clocks as unstructured entities remains after all extraneous to the concerns of the supporters of 'Lorentzian pedagogy' in special or general relativity as well to those of their opponents. It should rather be understood against the background of a truly philosophical question, which—to use Einstein's own rhetorical hyperbole—is after all nothing but "Pilate's famous question: 'What is truth?'" (Einstein, 1949b, 676).

2. 1917-1920. Einstein on Rods and Clocks in Private Correspondence

Pressed by his former student W. Dällenbach, at the beginning 1917 Einstein was ready to admit in private correspondence that his treatment of material rods and ideal clocks as unstructured entities was a provisional compromise, that would *be eliminated when a suitable theory of the material structure of rods and clocks would be found. In the immediately following years Einstein was forced to further articulate his epistemological standpoint pressed by the crosswise objections of Weyl and the young Pauli*

2.1. Einstein 1917 letter to Dällenbach

In a letter dated 31 October 1916 Einstein made in passing a remark to his friend Michele Besso that, in the following years, turned out to be matter of considerable debate: "Your comment about *the equivalence* of phyic[cally] different measuring rods and clocks (and *subjected to different prehistories*) is fully correct" (Einstein to Besso, 31-10-1916, CPAE, 8, Doc. 270, 349; my emphasis), however, Einstein points out, "this assumption plays tacitly a role also in Galilei-Newton's theory" (Einstein to Besso, 31-10-1916, CPAE, 8, Doc. 270, 349). Judging from the tone in this passage, Einstein seemed to consider this hypothesis sufficiently natural (in spite of the fact, that as we shall see it is actually quite implausible in the classical framework), and not worthy to be addressed explicitly. Taking distance and duration as basic measurable quantities presupposes the existence of reproducible standards of length and time, rods and clocks, functioning independent of their pre-histories.

In pre-general relativity theories rods and clocks are used to directly measure coordinate differences (at least in those coordinate systems for which the laws assume their simplest form) assuring their physical significance. A 'body of reference' can be in principle physically constructed as as cubic grid made of rigid rods in relative rest with clocks at the nodes, synchronized using light rays.[1] In general relativity space-time measuring devices are not to be admitted as the physical counterparts of the 'coordinates distances', which are no longer physically observable quantities; real distances can be calculated from coordinate distances only if one knows the quantities $g_{\mu\nu}$, which play at the same time the role of the gravitational potentials. Still it is assumed that the so called line element ds^2 has a 'natural' distance—a definite, experimentally determinable, value (up to a constant), that can be directly measured with 'transportable' rods and clock.[2]

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[1] Many passages starting from the 1905 paper on the electrodynamics moving bodies can be quoted to support this view (eg. Einstein, 1905, 892; Einstein, 1907, 437). Einstein famously needed some years to free himself from the prejudice that "it is really necessary to burden the beautiful concepts of space time with the clumsy rigid rods and clocks" (CPAE, 4, Doc. 1, 105, fn. 60, Manuscript on relativity, prob. 1912).

[2] As is well-known, Einstein needed to at least realize that "there cannot exist relationships between the space-time coordinates x_1, x_2, x_3, x_4 and the results of measurements obtainable by means of measuring rods and clocks that would be as simple as those in the old relativity theory" (Einstein and Grossmann, 1913, 1257). It became necessary to distinguish coordinate distances (which does not have physical significance), and natural distances (which have a unique, physically measurable value (up to an arbitrary constant) "This is the element's length as measured by a moveable unit measuring rod and moveable clock. By definition, this *natural length is a scalar, and must therefore be equal to the magnitude up to a constant, which we set to* 1. This gives the relation between coordinate differentials, on one hand, and measurable and measurable lengths and times, on the other; since they have this dependence on the $g_{\mu\nu}$,

Concretely, to measure the magnitude of the ds^2, we might use, say, a rock salt crystal as a rod and a cadmium atom emitting its red line as a clock. We can arbitrarily chose a certain number of spacings between the atoms of the crystal as unit of length or number of wave crests emitted by the radiating atom as unit of time. It is tacitly assumed that, that the ratio of such units is an *absolute constant*,[3] which is not affected by gravitational field (i.e. that is more precisely it does not depend on second derivative of the $g_{\mu\nu}$), and possibly by other external influences (in particular the presence of an electromagnetic field).

As we shall see, the choice of this kind of physical systems is of course not casual. There was overwhelming spectrographic evidence, in spite of the missing theoretical explanation (Bohr, 1913a; Bohr, 1913b; Bohr, 1913c), that atoms of the same kind are absolutely identical; the same astonishing identity is shown by the atomic lattices of the crystal of a given substance, as revealed by X-rays diffraction (Laue, Friedrich, and Knipping, 1913; Born, 1915). Such physical systems might therefore adequately play the role 'identically constructed' rods and clocks, that can be used to reproduce identical units of measure at remotest places and times, and thus to physically 'norm' the ds^2 as the unit interval.

The use of atoms or crystalline structures as clocks and rods makes at the same time particularly intuitive that these measuring devices are clearly not unstructured physical systems. Precisely for this reason, probably after 15 February 1917, Einstein had to admit to his former Zurich student Walter Dällenbach, that, from an epistemological point of view, the idea that the ds^2 could be found directly through a measurement done by rigid rods was at most a provisional compromise similar to the 'ponderomotive' definition of the electromagnetic field strengths, in which the electric force is defined as something which causes motion of an electric charge, and in turn, an electric charge is something which exerts electric force:

> Dear Dällenbach!
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> Your remarks are, in my opinion, to a large extent correct.[4] Strictly speaking, the concept of ds^2 volatilizes in an empty abstraction: ds^2 *cannot be rigorously considered as the result of measurements*, not even in the absence of electromagnetic fields. You have rightly indicated the reasons why it is so. Nevertheless, in a *didactically reasonable presentation of the theory* the ds^2 must be so considered, *as if* it were rigorously measurable. The issue here runs analogously to that in electrical science, where the definitions for $\mathfrak{e}$ and $\mathfrak{h}$ are given, even though these definitions do not

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coordinates by themselves have no physical meaning" (Einstein, 1913, 494). Natural distances are measured by a "moveable unit measuring rod and a moveable clock" (Einstein, 1913, 490; my emphasis), which always measure the same velocity of light *c in vacuo*. As Einstein points out in a footnote: "We will make the assumption that this is achievable at all locations and at all times" (Einstein, 1913, 490, n. 1). Einstein justifies this assumption reminding to the equivalence principle: The outcome of any local experiment (in a freely falling laboratory) is independent of its location in space-time.

[3] I borrow this from Ludwig Flamm (Flamm, 1916, 451), which makes this presupposition of general relativity particularly clear.

[4] Einstein refers to a private conversation.

> hold out against strict criticism. A logically satisfying presentation can be achieved (*a posteriori*), so that the a single, *more complex solution* is related to the observed facts. *A measuring-rod would then be an atomic system of a certain type that does not play any special role in the theory.* Thus a four-dimensional continuum *can* still be maintained and, in upholding the postulate of general covariance, it then has the advantage of circumventing the arbitrariness in the choice of coordinates (Einstein to Dällenbach, after 15-02-1917, CPAE, 8, Doc. 299, 591; my emphasis).

Surprisingly enough, this early passage contains the somehow ambivalent stance about rods and clocks that Einstein will consistently defend until the end of his life. Measuring rods and clocks are made of ordinary matter, that is, they are constructed of particles kept together by non-gravitational forces, which at that time meant electromagnetic interactions. The description of their behavior inevitably implies the comparison of the forces internal rods and clocks with the external ones. Such considerations ultimately depend on a theory of electromagnetic interactions involved in material structure. The length of a crystal and rate of an atomic clock might be affected by a strong variable electromagnetic field. But also in absence of electromagnetic effects, Einstein, points out, "we obviously do not know whether the [gravitational] field forces [. . .] or acceleration, do not (in principle) affect the yardsticks and clocks directly" (Einstein to Dällenbach, after 15-02-1917, CPAE, 8, Doc. 270 8, Doc. 565, 803; my emphasis).

Einstein initial treatment of rods and clocks, which made it necessary to introduce them as separate entities in the framework of his theory—he had just defended this point view at length in his popular book on relativity, finished at the end of 1916 (Einstein, 1917a)—was then a logical defect that ought to be eliminated. In general, Einstein seems to be convinced that a satisfactory theory should not simply postulate the existence of the material structures suitable to be used as measuring devices, possibly without abandoning the field theoretical framework of general relativity. Then rods and clocks would not play 'any special role in the theory', as opposed to all other physical objects.

However, in Einstein's theory, the role of matter as the source of the gravitational field was an external ingredient not determined by the theory, but rather has to be prescribed independently, letting open the question whether the theories of the electromagnetic field alone or possibly of the electromagnetic field and the gravitational field together will form a sufficient basis for a theory of matter.[5] After some initial doubt (Einstein, 1916a), the letter to Dällenbach

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[5] Einstein's theory was consciously formulated regardless of "specific assumptions about the constitution of matter" (Einstein, 1916b, 810), that is, of everything which is not the gravitational field, including the electromagnetic field. In particular Einstein rejected the "unfounded hypotheses about the structure of the electron or matter" (Einstein to Weyl, 23-11-1916, CPAE, 8, Doc. 278, 365), that had motivated Hilbert's theory. Hilbert had in fact attempted, along the line of Gustav Mie's thoughts, to use general relativity to construct a theory of matter, explaining "hitherto hidden processes in the interior of atoms" (Hilbert, 1915, 407). One can find here the model of a theory that deduce from itself the properties of atomic

shows that Einstein started to nurture some hopes, that general relativity might constitute the starting point for building a field-theory of matter; however, he was open (Stachel, 1993) to consider an atomistic alternative, that is to drop the 'continuum' and with it, the principle of general covariance. Einstein offered to Dällenbach a balanced analysis of pro and contra of both point of views:

> However, you have also understood correctly the disadvantage attached to a continuum. If the molecular interpretation of matter is the correct (practicable) one, that is, if a portion of the world must be represented as a finite number of moving points, then the continuum in modern theory contains much too multifarious possibilities. [...] Indeed, I see principal difficulties here [abandoning the continuum] as well. Electrons (as points) would be final conditions (building blocks) in such a system. *Do final building blocks exist at all? Why are they all similar in size?* Is it adequate to say: God in His wisdom has made them all the same size, each like any of the others, because He so wished? If it had suited Him, He could also have made them dissimilar. Thus we are better placed with the continuum conception, because elementary building blocks to not have to be provided from the outset (Einstein to Dällenbach, after 15-02-1917, CPAE, 8, Doc. 299, 591; my emphasis).

Thus, even if Einstein left open the possibility of a 'molecular interpretation of matter', where electrons are final 'building blocks', he seemed to be cautiously inclined to search for a continuum theory, in which the ultimate constituents of matter, the electrons or ions, would be treated as 'solutions' to some non-linear field equations.[6] In this context, it would be then possible to find some 'more complex solution' of such equations which present characteristics that make them suitable to be used as rods in clocks. Rods and clocks would not be introduced 'by hand', but would be treated as a physical systems just like any other. In a possible general relativistic theory of matter, the dynamics of both rods and clocks and all other material systems will be then described by the same theory. As we shall see, this would open the question, how the theory would then be related to reality; i.e. in which sense we could claim that the prediction made by the theory, e.g. about the values of the $g_{\mu\nu}$, could be said not be 'verified' by

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systems. Referring to the fact that an atom always measures the proper time $d\tau$. Hilbert pointed out that "This assumption has only a *provisional character*" (Hilbert, 1916/1917, 284; my emphasis). When physics is finally "fully developed, then the axiom must appear as a *consequence* of the general theory" (Hilbert, 1916/1917, 284; my emphasis).

[6] Already in January 1808, Einstein expressed his concern about the fact that Maxwell-Lorentz electrodynamics had to postulate the existence of electrons and their laws of motion separately. He argued that, "in my opinion a satisfying theory should so be constructed that the electron appears as solution" (Einstein to Sommerfeld, 14-01-1908, CPAE, 5, Doc. 73, 88). Starting from 1909 Einstein attempted to formulate a nonlinear theory of the electromagnetic field hoping to derive the atomistic properties of both radiation (light quanta) and matter, so that the electron would cease to be "a stranger in Maxwell-Lorentz Electrodynamics" (Einstein, 1909a, 192; see also Einstein, 1909b). In particular, in order to derive the existence of electrons of the same mass and charge, he came to recognize that it was sufficient to include in his equations some constant with dimensions of a length (cf. McCormmach, 1970, for more details).

observing the behavior of rods and clocks. Although Einstein had addressed a similar issue in the past, at least in passing,[7] in the following years he would be forced to face this in a more systematic way.

2.2. Rods and Clocks in the first Edition of Weyl's Raum, Zeit, Materie

The letter to Dällenbach constitutes a significant textual evidence, the importance of which, as far I can see, has not been enough emphasized in literature.[8] Here, Einstein's position about the role of rod- and clock-like material systems in relativity theory was already clearly defined in all its essential elements together with Einstein's stance toward the possibility of developing a relativistic theory of matter. In particular, already here, the key to reconcile this more careful stance with Einstein's repeated emphasis on the ideal of 'geometry as a branch of physics' concerning the behavior of physical rods and clocks, was to emphasize the 'provisional' aspect of the latter view (cf. Howard, 2005). As I will try to show, Einstein was forced by various interlocutors to address this issue in more or less popular writings; however, he will never change his stance towards this issue until this end of his life.

It might be significant that the letter was written to Dällenbach. Since 1916, he had been working on his doctoral dissertation at the ETH Zurich on a generally covariant extension of Lorentz electrodynamics (see below on page 19) under the guidance of the mathematician Hermann Weyl. Coming back from the war, Weyl had immersed himself in the study of Einstein's theory (Weyl, 1917; cf. Weyl, 1955, for Weyl's later recollections). Dällenbach's objection to Einstein possibly reflects discussions that he might have had with Weyl himself. In fact, during the Summer term of 1917 Weyl gave a series of lectures on Einstein's theory, which would be published a year later, as *Raum, Zeit, Materie* (Weyl, 1918a), destined to become one the first and most influential relativistic textbooks on relativity. Here, Weyl expressed his reservations towards Einstein's attitude of treating rods and clocks as unstructured entities in special relativity.

Weyl seems to believe that special relativity could make predictions about the behavior of *unaccelerated* clocks and rods without a detailed knowledge of the dynamical laws governing their behavior, but only by assuming the

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[7] Already, in 1911 Einstein seems to have argued, referring to the question of the definition of field strength via the unit charge, that the idea of a physical theory should not be regarded "a conceptual system whose individual parts" (CPAE, 3, Doc. 11, 325), might "correspond *immediately* to experiential facts" (CPAE, 3, Doc. 11, 325; my emphasis), but as a theoretical construction which "is true or false, i.e., corresponding or not corresponding to experience, *only as a whole*" (CPAE, 3, Doc. 11, 325; tr. in Howard, 1994, 91–92). As Don Howard showed in a classical paper (Howard, 1990), this attitude might display the influence of Pierre Duhem. See also below footnote 14. In the following, it will become progressively more clear that there is little doubt that the whole discussions on the role of rods and clocks in relativity theory should be understood on the background of this opposition between 'individual parts' vs. 'whole system', rather than that between principle vs. constructive theories.

[8] It is, for instance, never mentioned in the otherwise very careful reconstructions provided by Ryckman, 2005 and Fogel, 2008. Stachel, 1993 analyzes the letter but only considering Einstein's stance toward the continuum/discontinuum alternative.

Lorentz-Covariance of such laws. However, he emphatically points out that this 'agnosticism' was no longer allowed when the dynamical systems that we use as rods and clocks undergoes acceleration. In order to assure global scaling factor of the Lorentz transformations is the unity (cf. Weyl, 1923, 171, for more details), special relativity had to assume that "the static length of a measuring rod" (Weyl, 1918a, 138; tr. Weyl, 1922a, 176) and the "'proper time' of the clock" (Weyl, 1918a, 138; tr. Weyl, 1922a, 176) are the *same* in all inertial systems, so that the ratio of times indicated by different clocks and ratio of lengths determined by different rods is is always the *same* in all inertial systems. However, there is no other way to determine the equality of lengths or of intervals with respect to *different* inertial frames without transporting a rigid rod or ideal clock from one form another. As Weyl put it, special relativity presupposes that "a linear measuring rod" (Weyl, 1918a, 138; tr. Weyl, 1922a, 176), "after coming to rest in an allowable system of reference" (Weyl, 1918a, 138; tr. Weyl, 1922a, 176), "it shall have *the same [...] static length*" (Weyl, 1918a, 138; my emphasis; tr. Weyl, 1922a, 177); and a that a clock "shall always have the *same propertime* when it has come to rest (as a whole) in an allowable system of reference" (Weyl, 1918a, 138; my emphasis; tr. Weyl, 1922a, 177)[9]

However, the very notion of a rigid body preserving the same length is rather problematic in special relativity.[10] In particular no body can remain rigid while being continuously accelerated to another 'allowable' system; if we push the body at one and the same moment at different points, the motion will only gradually be communicated to the whole body, since no signals with arbitrarily high velocities are allowed. Hence, in special relativity "no body exists which *remains objectively always the same no matter to what influences it has been subjected*" (Weyl, 1918a, 138; my emphasis; tr. Weyl, 1922a, 176); we can speak at most of the 'rigid motion' of a non-rigid body. The rigid motion of an extended body is achieved by careful application of forces to different parts of the body; thus its transfer from one inertial system to another cannot be discussed without entering into dynamical considerations, based on some model of the body and of the effects of acceleration on its shape.

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[9] Einstein had explicitly pointed out this issue in a footnote of his 1907 *Jahrbuch*-paper, after having discussed the problem of the global scale factor in the Lorentz transformations (Einstein, 1907, 419–420): "[t]his conclusion is based on the *physical assumption* that the length of a measuring rod or the rate of a clock do not undergo any permanent changes if these objects are set in motion and then brought to rest again" (Einstein, 1907, 420, fn. 1; my emphasis; see also Einstein, 1910, 156, fn. 2). Einstein had to assume that the influence of accelerations on rods and clocks "may be neglected" (Einstein, 1907, 455), so that their lengths and ticking rates do not depend on the "prehistory of their motion" (Einstein, 1907, 429, fn. 1). Under this assumption, it is possible to use the *same* measuring rod and the *same* clock in different inertial systems cf. also Lorentz, 1910. Brown famously labeled this assumption 'boostability' of rods and clocks (Brown, 2005, 30, 81, 121).

[10] As is well known, this was shown by Born, 1909, whose definition of rigidity (the so-called 'Born rigidity') was later taken up by Ehrenfest, 1909; Herglotz, 1910; Noether, 1910; Laue, 1911. Einstein never directly participated to the discussion, which however might have played a relevant role in forcing him to abandon the notion of rigid a 'body of reference' (Stachel, 1989; Maltese and Orlando, 1995).

We are not in the better position when we consider the behavior of ideal clocks even if the difficulties due to their spatial extension can be neglected. The claim that a clock records the passage of proper time $\int ds$ along its trajectory is equivalent to what Arnold Sommerfeld has called the "(unprovable) assumption" (in Blumenthal, 1913, 71); that one can ignore the effects of acceleration on its internal workings. More precisely, the assumption depends on our knowledge of the physical constitution of the clock. Mechanical clocks—a pendulum or a balance wheel—might not be very resistant to strong accelerations (such as smashing them against an obstacle); even an atomic clock accelerated by means of a strong electric field might be ionizided and thus stop working as a clock[11]

According to Weyl, we can at most assume that the "rods and clocks which we shall use satisfy this condition to a sufficient degree of approximation" (Weyl, 1918a, 138; tr. Weyl, 1922a, 177). In particular, we can make a reasonably plausible guess only for quasi-stationary motions, that is for slow accelerations, and nothing can be asserted about what happens during arbitrary acceleration. In this sense Weyl speaks of an 'adiabaticity' assumption in analogy with slow processes in thermodynamics. When a gas is warmed sufficiently slowly (strictly speaking, infinitely slowly), it will pass through a series of thermo-dynamic states of equilibrium, similarly "only when we move the measuring rods and clocks steadily, without jerks, will they preserve their static lengths and proper-times" (Weyl, 1918a, 138; tr. Weyl, 1922a, 177). However, "[t]he limits of acceleration within which this assumption may be made without appreciable errors" (Weyl, 1918a, 138; tr. Weyl, 1922a, 177) can be established "*only when we have built up a dynamics based on physical and mechanical laws*" (Weyl, 1918a, 139; my emphasis; tr. Weyl, 1922a, 177).

Weyl makes here a simple point on which, later, most relativists will substantially agree. In special relativity coordinate differences are said to be measured by means of rigid rods and clocks. However, in doing so we actually use certain concrete physical processes governed by dynamical laws. Only the knowledge of such laws would allow explanation of the proper behavior of rods and clocks, removing the need to postulate it. However, Weyl also makes particularly clear that what has to be explained 'dynamically' was the very existence of rods and clocks of *identical construction*, which remain physically *the same* despite the influences they were subjected to. This is the fundamental condition of the experimental testability of the theory.

It is in fact precisely because "a sodium-molecule which is at rest in an

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[11] After a talk in Zurich in 1911 (Einstein, 1911a), Einstein confessed that, "according to the theory of relativity we do not know what happens" (Einstein, 1911b, V) to an accelerated clock. We can, for instance, imagine it moving along approximate polygonal path composed of n successive small linear paths in which it moves in uniform motion. However "[t]he sudden change of direction could produce an immediate change in the positions of the hands of the clock [Uhrzeigerstellung]" (Einstein, 1911b, V). Thus, Einstein limited his considerations to very slow accelerations: "the influence of such sudden change should recede the longer the clock [. . .] moves uniformly, i.e. the bigger are dimensions of the polygon" (Einstein, 1911b, V).

allowable system *remains objectively the same*" (Weyl, 1918a, 146; my emphasis; tr. Weyl, 1922a, 185), that we can predict that "ν', the frequency of a sodium-molecule moving with a velocity v" (Weyl, 1918a, 146; tr. Weyl, 1922a, 185), will differ by a Lorentz factor from "the frequency ν of a sodium-molecule which is at rest", if "*both frequencies being observed in a spectroscope which is at rest*" (Weyl, 1918a, 146; my emphasis; tr. Weyl, 1922a, 185). Then, if we compare the rates of two identical atomic clocks, moving with respect to one another, with the help of light signals we might observe, or not, a 'time dilation effect' predicted by the theory. However, this comparison is meaningful under the assumption that if the two atoms were carefully slowed down and put next to each other they would always tick at the same rate.[12]

In other terms, it would be preferable to have some special-relativistic theory of matter which admit certain discrete solutions of special character, such as static or periodic solutions, that make them suitable to be used as rods and clocks. However, Weyl points out, "the Maxwell-Lorentz theory" (Weyl, 1918a, 162; tr. Weyl, 1922a, 203; slightly modified), the most immediate candidate for a matter theory, being linear, is incapable of solving "the problem of matter", that is to account for the existence of electrons, which have a definite invariable mass, and, in addition, a definite invariable charge. In particular it was incomprehensible why the separate parts of the negative or positive charge did not repel each other, without introducing some non-electromagnetic force exerting a "cohesive pressure" (Weyl, 1918a, 163; tr. Weyl, 1922a, 208) of tremendous strength. Thus the electrons are treated "as something given *a priori* as a foreign body to the field" (Weyl, 1918a, 165; tr. Weyl, 1922a, 206). Weyl did not hide his (clearly too optimistic) hopes that it was possible to build a "purely dynamical view of matter" (Weyl, 1918a, 161; tr. Weyl, 1922a, 202) *à la* Gustav Mie (Mie, 1912b; Mie, 1912a; Mie, 1913), in which matter would turn out to be an "offspring of the field" (Weyl, 1918a, 161; tr. Weyl, 1922a, 203), rather than as "its carrier" (Weyl, 1918a, 161; tr. Weyl, 1922a, 203) Atoms and electrons would not be regarded as "ultimate invariable elements" (Weyl, 1918a, 161; tr. Weyl, 1922a, 203), but as stable 'knots', particle-like solutions of some set of Lorentz covariant non-linear partial differential equations (cf. Scholz, 2006, for more details).

Weyl nurtured some hopes that such a theory would explain the existence and properties of electrons in electromagnetic terms and indirectly, those of atoms. In this way one would also be able to account for the stability of atomic

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[12] The use of a quantum mechanical systems (moving positive ions) as fast moving clocks was suggested by Einstein in 1906 (Einstein, 1906). The reason for this choice (beside of the fact that ions in canal rays move very fast) was based on the fact that we can reasonably assume that all ions are 'identical', so that their spectral lines as measured by a co-moving spectrograph are always the same. Then we can consider such an ion as a clock of definite frequency; "this frequency is given, for example, by the light emitted by *identically constituted ions at rest with respect to the observer*" (Einstein, 1907, 422; my emphasis). It is worth emphasizing Einstein's claim that ions can interpreted as 'identically constructed' clocks. Cf. also Eddington, 1918, which makes particularly clear that the dimensions of hydrogen atoms at rest is assumed to be *the same* in all inertial systems can be used to assure the correspondence of the measuring units in different frames.

spectra that one can use as clocks or the distribution of atom patterns in a rigid crystalline structure that one can use as rods. However, in the absence of any reliable relativistic theory of matter assuring the existence of localized stable structures suitable to become space-time measuring devices, Weyl was more akin to avoid the use of rods and clocks as measuring instruments altogether: "since the behavior of rods and clocks is to some extent problematic for establishing of physical laws" (Weyl, 1918a, 139) one may use of less structured entities and derive the metric structure (up to the choice of a unit of measure) from the observation of light rays and free-falling particles alone (provided certain global assumptions about the entire space-time are added), that is measuring devices that could be described within the framework of special relativity, or of the available special relativistic field theories, such as Maxwell electrodynamics (cf. Weyl, 1918a, 139–140)

Thus, in the first edition of his handbook, Weyl already sketched the main lines of an epistemological ideal on which he will remain faithful until the end of this life (for an excellent philosophically oriented overview of Weyl's work, cf. Bell and Korté, 2011): a theory, according to Weyl, should in principle avoid the use of measuring devices, the behavior of which is extraneous to the framework of the theory (see below section 5.3). It is, after all, the theory which tells us which physical objects, should they exist, can be used as suitable measuring devices. For instance our knowledge of the electric field is admittedly only inferred from the behavior of matter, which is the only seat of charge (cf. Weyl, 1918a, 60). In particular, we define the electric field strength in terms of the 'ponderomotive force' on the unit charge. However,—Weyl would use this example again and again in his epistemological writings—this definition can be regarded only as a provisional starting point. It sufficient to consider the fact that we can empirically determine the charge of the test body, only by observing the curvature of its path in an electric field as determined according to Lorentz force law; thus we have to know the momentum of the charged body at any given time, as well as the strength of the electric field, to calculate its charge.

In general, Weyl insists, it does not make sense to "test a single law detached from this theoretical fabric" (Weyl, 1918a, 60; tr. Weyl, 1922a, 67). We need a "whole network of theoretical considerations to arrive at an experimental means of verification" (Weyl, 1918a, 60; tr. Weyl, 1922a, 67) which constitutes a self-supporting system of cross-references: Only this "inseparable theoretical whole" (Weyl, 1918a, 61; tr. Weyl, 1922a, 67) can be meaningfully confronted with experience. Needless to say, Weyl displayed the same attitude towards the role of the behavior of rods and clocks in general relativity. A proper description of their behavior would require not only Einstein's gravitational-geometric theory, but a theory of matter as well, which however general relativity was unable to provide.

So for instance, one could consider general relativity as empirically adequate, because the predicted red-shift of solar atoms is observed. The gravitational redshift presupposes however again that two "sodium atoms at rest" (Weyl, 1918a, 197; tr. Weyl, 1922a, 246), those placed in the photosphere of the sun, and the ones on the earth, "are *objectively fully alike*" (Weyl, 1918a, 197; my emphasis; tr. Weyl, 1922a, 246), that is emit the 'same' number of wave crests in a given time (if measured by a spectrograph relatively at rest). However, the very existence of identical atoms is not understandable from the point of view of general relativity, which just as special relativity relies on a merely phenomenological theory of matter "in which the atomic fine structure of matter is disregarded" (Weyl, 1918a, 216).

In the impossibility of providing a general-relativistic theory of matter accounting for the existence of the very structures that we use as rods and clocks, Weyl insisted again that it would be more accurate to resort to testing procedures that involve less dynamical theory than others. In particular, Weyl initially believed that in general relativity light signals would suffice to determine the geometry of space-time. Two g_{ik}-systems that agree on light rays would determine the same space-time geometry up to a constant λ: "the arbitrary remaining proportionality factor can only be determined through the individual choice of a unit of measure" (Weyl, 1918a, 183). After this arbitrary choice have been made once and for all, it is possible to assign specific values to the duration of time intervals and space intervals.[13]

2.3. Weyl's theory and the Einstein-Weyl Correspondence

As Weyl would later recall (Weyl, 1946), it was during the 1917 lectures on relativity that he realized that the assumption of the constancy of the scale factor λ—and thus of the global availability of the units of length and time—was only a contingent feature of the Riemannian geometry adopted by general relativity, an assumption which in principle could be dropped. Following mainly Tullio Levi-Civita (Levi-Civita, 1916), Weyl interpreted the length ds of two adjacent events as an infinitesimal vector in space-time, whose components are the coordinate-differentials dx_i of its end points. In Riemannian geometry, in the general case, a vector transported without changing its components (it can be parallel-transported) around a loop returns to its original position with the same length but a different direction (non-integrability of direction); Weyl realized that a coherent non-Riemannan geometry might be build in which the such an unpleasant asymmetry between length and direction is avoided (non-integrability of length; for more details cf. e.g. Scholz, 1994; Vizgin, 1994, ch.3; Ryckman, 2005, sec. 6.4; Bell and Korté, 2011, 4.1.3).

Weyl sent the drafts of his book to Einstein on 1 March 1918, also announcing a 10 page paper, in which he suggested that, by removing such asymmetry between length and direction of a vector (Afriat, 2009), one could formulate a

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[13] It must be noted that in a paper finished in October 1917 and published in March 1918 (just about the time the drafts of Weyl's handbook were finished), Erich Kretschmann had already shown that one must use also use free-falling particles to force the factor λ to be an arbitrary constant, and not a function of position. Resorting to the constant curvature of the spatial slices of the world in Einstein's new 'cylindrical' cosmological model, one can eliminate this degree of freedom as well, by norming the constant factor $= 1$ so that curvature of space is constant (Kretschmann, 1918, section 23). This solution of the problem, as we shall see, would be brought to the most radical speculative implications by Weyl himself (and Eddington).

theory unifying gravitation and the other know non-gravitational interaction, electromagnetism: the non-integrability of direction would account of gravitation and the non-integrability of length for electromagnetism. In dropping the comparability of lengths at distance, not only the ten components of the metric tensor g_{ik}, but also the four quantities φ_i appear, which happen to behave like the potentials of the electromagnetic field. Moreover Weyl had clearly the hope of finding an explanation for "the existence of the electron and the peculiarities of the hitherto unexplained processes in the atom that could be deduced from the theory" (Weyl, 1918b, 477), and with them the existence of bulky material structures.

When Einstein received the draft of the paper on 6 April 1918, he was famously impressed by Weyl's 'stroke of genius'; however he confessed that he was incapable to overcome what he labeled the 'measuring rods objection' against the theory that he should have communicated to Weyl in person during a stay in Zurich in late March (cf. CPAE, 8, Doc. 498, 710 and commentaries). The well-known objection runs roughly as follows. Einstein immediately attributed to the magnitude of a time-like vector ds connecting two adjacent events a concrete physical meaning: it is the time recorded by a clock (some physical system running periodically) carried by a particle which includes those two events in its history.

Put in this way, Weyl's theory would predict that the ratio of the rate of ticking any two physical clocks is not constant, but it is affected by the electromagnetic potentials φ they have encountered. A clock would measure its proper time $\int ds$ in absence of an electromagnetic field but it would measure $\int \varphi_\nu ds_\nu$ (where φ_ν is a function of position) if it has been placed for a long time, say, in a charged metallic box. In this way, however, the rate of ticking of clocks would depend on the physical circumstances they have been through in the past. The equality of two line elements, ds_1 and ds_2 in Einstein's notation, would not be 'physically' definable in a univocal way. As a consequence—as Einstein wrote to Weyl on April 1918—"Rel. Theory would fully lose its empirical basis" (Einstein to Weyl, 15-04-1918, CPAE, 8, Doc. 508, 721).

Since the Prussian academy accepted to publish Weyl's paper only together with Einstein's objection (cf. CPAE, 8, Einstein to Weyl, 15-04-1918„ Doc. 509), Einstein attached to a letter to Weyl on 19 April (CPAE, 8, Doc. 512) a detailed response (later published as Einstein, 1918b from which citations are taken). Light rays, Einstein claimed, are not sufficient as space-time measuring devices; "there would indeed be an indeterminate factor left in the line-element ds (as well as in the g_{ik})" (Einstein, 1918b, 478). This indeterminacy can be removed using rods and clocks, that allowed to make a global choice of the measuring units. In particular "a time-like ds can be measured directly by a standard clock whose world-line is contained in ds" (Einstein, 1918b, 478), so that $\int ds$ would measure the elapsed time between two arbitrary time-like separated events. This definition would become physically empty "if the length of a unit measuring-rod (resp. the rate of ticking of a unit clock) depended on its history" (Einstein, 1918b, 478), as Weyl's theory implies.

Fortunately, nature happens to behave differently. Einstein presented at this point a powerful argument that would become famous. In particular he added a fundamental detail that was curiously not emphasized in previous correspondence with Weyl by identifying explicitly clocks with *atomic* clocks. In this perspective, if Weyl's theory were true, "chemical elements with spectral-lines of definite frequency could not exist" (Einstein, 1918b, 478) and "the relative frequency of two neighboring atoms of the same kind would be different in general" (Einstein, 1918b, 478). In Weyl's theory the final radiuses of, say, two hydrogen atoms separated and then reunited might differ, depending on the different circumstances they might have passed through. As a consequence of the spectral lines would not be sharply defined, but should show intermediates values. Experience, however, shows that atoms of a particular element are all 'identical', and can emit only radiation with definite energies, that is each type of atom gives off a unique set of colors. Frequencies and therefore lengths can be transported in an integrable way. Consequently, "it seems to me that one cannot accept the basic hypothesis of this theory whose depth and boldness every reader must nevertheless admire" (Einstein, 1918b, 478).

Some days later, on 27 April 1918, Weyl expressed his intention to write an *Erwiderung*, a reply, which he sent on 28 April and that would be published alongside with Einstein's *Nachtrag* (Weyl, 1918c). Weyl defended his theory by expressing again the very same concerns about the use of rods and clocks as measuring instruments that could be found in his handbook (cf. above section 2.2).

As he pointed out, even in special relativity the assumption that "a rigid rod has *always the same rest-length* if it is at rest in an inertial frame" (Weyl, 1918c, 478; my emphasis; tr. O'Raifeartaigh, 1997) (i.e. the 'world stripe' limited by the world-lines of its end-points is of constant with) is particularly troublesome. No less problematic is the assumption, "that under the same circumstances, a standard clock has *the same period* in standard unit" (Weyl, 1918c, 478–479; my emphasis; tr. O'Raifeartaigh, 1997, 35) (i.e. assuming that $c = 1$). For small accelerations, Weyl continues, most real clocks will behave as an ideal clock, that is they record the passage of elapsed proper $\int ds$ time along their world-lines: however, this "is certainly not the case when the clock (or atom) experiences the effect of a strongly varying electromagnetic field" (Weyl, 1918c, 479; tr. O'Raifeartaigh, 1997, 35). The frequency of an 'electric' clock would be modified by an electric field, displaying, for instance, a 'Stark effect' (the electric analogue of the 'Zeeman effect'), so that their spectral lines would be shifted (cf. e.g. Epstein, 1916, 1).

This situation is of course not different in general relativity. Exactly, "[b]ecause of this problematic behavior of rods and clocks" (Weyl, 1918c, 479; tr. O'Raifeartaigh, 1997, 35), Weyl points out, "I have relied in my book *Raum, Zeit, Materie* only on the observation of light-signals for the measurement of the g_{ik}" (Weyl, 1918c, 479; tr. O'Raifeartaigh, 1997, 35). Weyl erroneously refers the reader to Kretschmann's paper (Kretschmann, 1918) to support his actually incorrect claim (cf. footnote 13; in Weyl, 1919b, 194 specified that also free-falling particles are needed); but this of course does not affect the epistemological meaning of his reservations against the use of too structured entities as measuring instruments.

In general relativity, one can at most assume that a clock at rest in a static gravitational field measures "the integral $\int ds$ in the absence of an electromagnetic field" (Weyl, 1918c, 479; tr. O'Raifeartaigh, 1997, 35); but additional assumptions are needed to assure that non-gravitational forces do not affect the internal dynamics of our clocks. In principle, "[h]ow much a clock behaves in arbitrary motion in the common presence of arbitrary gravitational and electromagnetic fields *can only be determined by the computation of the dynamic based on physical laws*" (Weyl, 1918c, 479; my emphasis; tr. O'Raifeartaigh, 1997, 35). Whether the relative length and periods of physical rods and clocks depend on their prehistories or not "has to be justified by an explicit dynamical calculation in both Einstein's theory and mine" (Weyl, 1918c, 479; tr. O'Raifeartaigh, 1997, 36).

For the time being Weyl could tentatively point out that it was plausible that "an oscillating system of definite structure that remains in a definite static field will behave in a definite way" (Weyl, 1918c, 479; tr. O'Raifeartaigh, 1997, 36), because "the influence of a possibly turbulent history will quickly dissipate" (Weyl, 1918c, 479; tr. O'Raifeartaigh, 1997, 36) making the effect on the electromagnetic field the atomic clock behavior practically unnoticeable. His theory was then probably not "in contradiction with this experimental situation (which is confirmed by the existence of chemical elements for the atoms" (Weyl, 1918c, 479; tr. O'Raifeartaigh, 1997, 36). However, Weyl clearly manifested his conviction that it would be preferable to determine the properties of physical systems that can serve as rods or clocks from the field equations of the theory—which he hoped would be able to account for the granular structure of matter—instead of assuming them separately as primitive entities. Without such a 'dynamical' explanation, the relation between "the mathematical *ideal* of vector-transfer, on which the construction of the geometry is based" has no simple and obvious connection "with the *real* situation regarding the movement of a clock". Weyl considered 'his' geometry to hold in reality because of his conceptual superiority—it is the 'truly infinitesimal geometry' (Weyl, 1918d)—and not because of its correspondence with the behavior of quite complicated material measuring devices (on this topic cf. also Weyl to Einstein, 19-05-18, CPAE, 8, Doc 544, 767; and Einstein reply on, 31-05-1918, CPAE, 8, Doc 551, 777)

What is important to emphasize for our purposes is again the fact that the feature of rods and clocks that was thought to require a 'dynamical' explanation, was, so to say, their 'memoryless', the fact the they stay objectively *the same* no matter what influences they have been subjected to (cf. Weyl, 1918c, 479). Writing to Einstein on 15 June 1918, Dällenbach, who could not hide his fascination for Weyl's theory, made this point particularly clear. Dälllenbach recognized, that "there must be some principle" (Einstein to Dällenbach, 15-06-1918, CPAE, 8, Doc. 299, 391), which "connects the different world-points of my experience along a world-line" (CPAE, 8, Doc. 564, 801), something "which is presupposed as invariant, as stable" (Einstein to Dällenbach, 15-06-1918, CPAE, 8, Doc. 564, 801), that can be considered "again and again as *the same*" (Einstein to Dällenbach, 15-06-1918, CPAE, 8, Doc. 564, 801; my emphasis), allowing the comparison of the length of two tracts along a world line. However, Dällenbach goes on, "you chose for this purpose so complicated things as 'rigid bodies' or 'clocks'. It seems to me to be arbitrary" (Einstein to Dällenbach, 15-06-1918, CPAE, 8, Doc. 564, 801).

As we have mentioned, Dällenbach was finishing at that time his dissertation under Weyl's supervision (which will be published in October Dällenbach, 1918). There he tried to develop an electromagnetic model of the stability of matter. In this perspective a bulk of matter would be nothing but "a bulk of very many positive or negative electrons" (Dällenbach, 1918, 524) kept together by Lorentz invariant forces, a model that he believed could be 'easily' generalizes to a generally covariant theory. There was then no reason to expect that rods and clocks should not be treated as equilibrium solutions of such a theory as any other bulk of matter.

Einstein's answer to Dällenbach (after 15 June 1918) represents to my knowledge the best summary of the Weyl-Einstein debate and it is worth to be cited at length:

> Weyl's theory is wonderful as conception, a genial intellectual achievement [geniale Gedankenleistung], but I'm convinced that nature is different. *If two ds at distant points were measured with measuring rods or, in a certain way, were found to be equal, they would still be found equal, if they were measured in a different way.* This *is a deep property of our world*, which must find expression in the foundation of physics [. . .] I know that Weyl does not admit it. He would say that *clocks and measuring-rods must appear as solutions*; they do not occur in the foundation of the theory. But I find: if the ds is measured by a clock (or a measuring-rod), is something independent of prehistory, construction and the material, then this invariant as such must also play a fundamental role in the theory. Yet, if the manner in which nature really behaves would be otherwise, then spectral lines and well-defined chemical elements would not exist (Einstein to Dällenbach, after 15-06-1918, CPAE, 8, Doc. 299, 391; my emphasis).

Thus Einstein admitted again to Dällenbach that in principle it would be better to consider rods and clocks as 'solutions' of some general-relativistic theory of matter, instead of assuming them as unexplained postulates. However, differently from Weyl, Einstein believed that for the time being, it was after all sufficient to regard the mathematical invariance of the ds as having a physical counterpart in the surprising 'identity' atoms unmistakably testified by spectrographic data: as he wrote to Besso some days later on August 1918, "otherwise, sodium atoms and electrons of all sizes would exist" (Einstein to Besso, 20-08-1918, CPAE, 8, Doc. 299, 391; my emphasis).

At the end of November 1918, Weyl sent Einstein the draft of a new paper on his unified field theory (the revised version was finished in January, and will be published as Weyl, 1919a), announcing the plan of writing a revised edition of his handbook (finished in August and published as Weyl, 1919b), since the first two editions—not least because of Einstein's enthusiastic review (Einstein, 1918a)—had rapidly sold out. After having read Weyl's paper, Einstein reiterated his main objection against his theory (among many others). If Einstein could put the finger on the manifest lack of empirical confirmation of Weyl's theory, he was however also forced to admit that the existence of identical atoms was essential to secure the physical significance of the *ds*; after all they are the only physical systems that present a peculiar kind of identity requested by the theory: it is the "[t]he existence of spectral lines (*of electrons of certain size*)" (Einstein to Weyl, 21-11-1918, CPAE, 8, Doc. 661, 954; my emphasis), and thus of "of clocks in depend of their prehistory" (Einstein to Weyl, 21-11-1918, CPAE, 8, Doc. 661, 954), that "let appear as natural to treat from the beginning the *ds* as an invariant".

It is comprehensible that for Einstein the issue was of essential importance for the very physical meaningfulness of his theory. As we have mentioned, it is precisely because all atoms of the same type are all absolutely 'identical', on the earth or on the photosphere of the sun, that we can consider red shift observations as an empirical 'test' of general relativity. This effect was however very hard to detect (Hentschel, 1994), as the results of the "american measurements" (Einstein to Weyl, 27-09-1918, CPAE, 8, Doc. 626, 894), performed by Charles Edward St. John (St. John, 1917), confirmed. It is not surprising that, by the end of the year on 16 December 1918, scarcely a month after the end of the World War, Arthur Stanley Eddington (who had just finished to write his influential report on relativity Eddington, 1918), could have the temptation to resort to Weyl's theory to save general relativity from possible refutation; if the redshift measurements failed, Weyl's theory might furnish an explanation, showing that atoms *might not* consistently measure their proper time if under the influence of an electromagnetic field (Eddington to Weyl 16 December 1918, SzZE, Weyl Papers, folder 91, p. 522). However, Einstein, in a letter of congratulations written to Eddington on 15 December 1918 for his successful solar eclipse expedition insisted that the red shift was an "absolutely inevitable consequence of relativity theory" (Einstein to Eddington, 15-12-1918, CPAE, 10, Doc. 216, 304), despite the contemporary overall negative evidence for it: "If it were proven that this effect does not exist in nature, the whole theory would have to be abandoned" (Einstein to Eddington, 15-12-1918, CPAE, 10, Doc. 216, 304).

2.4. Einstein, Weyl and the young Pauli's 'Observability' Criterion

Einstein and Weyl, beside their dispute concerning the existence of a "*central office of standards*" (Weyl, 1919a, 103; my emphasis) assuring the global availability of the unit of measures, after all agreed about the issue of the relations that a physical theory should entertain with the material devices that served to its verification, and possibly about the very program of a field theoretical approach to the problem of matter. In April 1919 Einstein himself had come to display a more concrete interest for "a theory which will account for the equilibrium of the electricity constituting the electron" (Einstein, 1919a, 349) (something along the line of Mie's theory), and more in general for "the possibility of a theoretical construction of matter out of gravitational field and electromagnetic field alone" (Einstein, 1919a, 356) (that is by considering only the quantities $g_{\mu\nu}$ and φ_ν).

However, a very different attitude towards these issues was emerging on the german physical scene at about the same time. In April 1919, the 19 year old student Wolfgang Pauli, who in January had already published a paper on general relativity (Pauli, 1919b), wrote a letter to Weyl, asking him several questions about his unified field theory. Weyl, full of admiration for Pauli's young age, referred him to the forthcoming third edition of his handbook (Weyl, 1919b), of which he had just received the proofs, and to the second draft of his paper (Weyl, 1919b) to be published in the *Annalen der Physik* (cf. Weyl to Pauli, 10-05-1919, Pauli, 1979, Doc. 1, 3). Despite Einstein's criticisms, Weyl's program had become even more ambitious. In particular, as he wrote to Pauli in the paper he had provided a more suitable presentation of the "Electron-problem", even without being able to solve it (cf. Weyl to Pauli, 10-05-1919, Pauli, 1979, Doc. 1, 3).

On the one hand he imagined that non-linear field equations—satisfying adequate regularity conditions—should lead to a discrete set of solutions depending on some arbitrary but constant field quantity β. The problem of matter would than become an 'Eigenvalue' problem, where particle-like solutions would be comparable to a set of 'discrete eigenvalues' of a (non-linear) operator (cf. Weyl, 1919a, 129). On the other hand he expected that Einstein's field equation with cosmological constant (Einstein, 1917b) should be a consequence of his theory, making natural to identify this quantity with the radius of curvature of every slice of Einstein's spatially finite spherical universe. The speculations about the relations between the size of the electron (and thus of the atom) and size of the universe became the essential tract of Weyl's line of defense against Einstein's objection (cf. Weyl, 1919b, 260; for more details see Ryckman, 2005, sec. 6.4.2).

However, beside attempting to answer Pauli's main objection—the fact that the theory was unable to account for the difference between positive and negative charge (cf. Weyl to Pauli, 10-05-1919, Pauli, 1979, Doc. 1, 4)—Weyl encouraged the young student to publish his result independently (cf. Weyl to Pauli, 10-05-1919, Pauli, 1979, Doc. 1, 3). Pauli's paper was finished in July and published in October in the *Physikalische Zeitschrift*. As it appears from a second paper on Weyl's theory that Pauli submitted in November, initially, Pauli seems to have been attracted to Weyl's theory precisely because it offered "the possibility to explain and to understand the existence of the electron" (Pauli, 1919c, 461; tr. Mehra and Rechenberg, 2001, 382). As Weyl points out, the conservation laws for charge and mass in classical electrodynamics do not explain why all electrons have always the same mass and charge, also after having interacted with different environments for arbitrarily long time (cf. Weyl, 1919a, 128).

However, Pauli added a further "physical-conceptual" (Pauli, 1919a, 750; tr. Mehra and Rechenberg, 2001, 382) reservation, on which he will return in many occasions in the following years (cf. Hendry, 1984). In Maxwell-Lorentz theory, electrons are needed as the seat of charges, whereas Maxwell equations are valid only for the 'free field' outside of them. By the contrary, Pauli famously pointed out "[i]n Weyl's theory one operates constantly with the field strength in the interior of the electron" (Pauli, 1919a, 750; tr. Mehra and Rechenberg, 2001, 382), following the model of Mie's theory; physically, however, the field strengths are "defined only as a force acting on a test-body" (Pauli, 1919c, 750). Since there exist no smaller test-bodies than the electron itself the concept of an electric field strength inside of the electron is "an empty, meaningless fiction" (Pauli, 1919a, 750; tr. Mehra and Rechenberg, 2001, 382). Pauli introduced here a criterion for the meaningfulness of physical quantities, which will enjoy some success: "One would like to keep to the introduction into physics of only quantities observable in principle" (Pauli, 1919a, 742; tr. Mehra and Rechenberg, 2001, 742).

In this sense it might be that physics is altogether "on the wrong track in considering continuum theories for the field in the interior of the electron" (Pauli, 1919a, 750; tr. Mehra and Rechenberg, 2001, 382). The young Pauli—who was after all Ernst Mach's godchild—displayed already here the strong epistemological 'feeling', that the some individual abstract element that occur in the theory (e.g. the notion of 'field strength') must be immediately coordinate with the behavior of physically existent, even if idealized, objects used as probes (e.g. small bodies of a certain reproducible mass and charge). Unless the quantities introduced into the foundations of the theory can be directly identified with such real objects, the theory is thought to be physically empty. As we shall see Pauli, would need only some months to explicitly extend this epistemological consideration from general relativity to any attempt to construct general relativistic theory of matter (see below section 3.2).

The epistemological attitude of the young Pauli elicited a reply from Weyl. In a letter to Pauli dated 9 December 1919, Weyl defended his program of a unified field theory against Pauli's criticisms, even if he conceded that the problem of matter could probably not be completely solved in a field-theoretical framework without resorting to statistical considerations. In particular he argued that in some sense, if possible, "to measure the fields in the interior of the electron" (Weyl to Pauli, 09-12-1919, Pauli, 1979, Doc. 2, 6) provided that differences inside of the electron "grow to an immediately noticeable magnitude" (Weyl to Pauli, 09-12-1919, Pauli, 1979, Doc. 2, 6), producing some change in its external behavior. He speculated that some change inside of the electrons due to its acceleration would account for the fact that electrons in Bohr's stationary orbit do not radiate.

Only some days later on 23 December 1919, Max Born wrote to Pauli that he read "with great interest" (Born to Pauli, 23-12-1919, Pauli, 1979, Doc. 4, 10) his paper on Weyl's theory "in the new issue of *Verhandlungen der Deutschen Physikalishen Gesellschaft*" (Born to Pauli, 23-12-1919, Pauli, 1979, Doc. 4, 10). Born confessed to his future assistant, that he has been "especially interested" (Born to Pauli, 23-12-1919, Pauli, 1979, Doc. 4, 10) in Pauli's claim that "the application of the continuum theory to the interior of the electron" (Born to Pauli, 23-12-1919, Pauli, 1979, Doc. 4, 10) would be "meaningless, because one is then dealing with things that are unobservable in principle" (Born to Pauli, 23-12-1919, Pauli, 1979, Doc. 4, 10). The way out of "quantum difficulties" (Born to Pauli, 23-12-1919, Pauli, 1979, Doc. 4, 6), Born went on, should be sought having this problem in mind, renouncing to carry over the macroscopic concepts of space and time into the quantum domain, where length and distance cannot be in principle be measured.

It is interesting that also Einstein seemed to have took Pauli's objection very seriously, confessing to Born himself some weeks later: "Pauli's objection is directed not only against Weyl's, but also against everyone else's continuum theory. Even against one which treats the electron as a singularity" (Einstein to Born, 27-01-1920, CPAE, 9, Doc. 284, 43). However, Einstein, differently from Born, did "not believe that one must abandon the continuum in order to solve the [problem of] quanta" (Einstein to Born, 27-01-1920, CPAE, 9, Doc. 284, 43). Even if in principle the space-time continuum could be abandoned, as suggested by Born, Einstein was more inclined to search for "differential equations for which the solutions no longer have any continuum properties" (Einstein to Born, 27-01-1920, CPAE, 9, Doc. 284, 43).

From this point of view the failure of Weyl's theory to provide solutions corresponding to electrons appeared to Einstein a fundamental failure (cf. Einstein to Ehrenfest, 04-12-1919, cited in Seelig, 1960, 280; and Einstein to Besso, 12-12-1919, in Speziali, 1972, 148).[14] Einstein appeared progressively more convinced that material bodies should turn out to be a mere manifestation of the unique physical reality represented by the field. It is not surprising that Born, in his semi-popular Frankfurt lectures on relativity, which later became a widely read book (Born, 1920) would show the very opposite attitude: The field is only a "mere mathematical device for conveniently describing processes in matter" (Born, 1920, 171; tr. Born, 1922, 190). Before a test body is brought up, the field lines *in vacuo* has as little physical reality as the lines of latitude and longitude on the sea, before someone decides to determine position of a ship by means of astronomical observations (Born, 1920, 171; tr. Born, 1922, 190).

3. 1920-1923: Einstein on Rods and Clocks in the Public Debate

Even if Einstein thought that Weyl's theory was flawed, he clearly realized that the independence of rods and clocks from there prehistories was a fundamental presupposition of the empirical meaningfulness of relativity theories. Einstein

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[14] One might speculate that, after all, Einstein had already answered Pauli's epistemological challenge a decade earlier in a series of unpublished lectures on electromagnetism, which we have mentioned above (see footnote 7). Einstein explicitly considered it legitimate to extend the definition of the field strength obtained *in vacuo* inside ponderable bodies (where the electricity is assumed to be distributed continuously), even if there "the force thus defined is no longer immediately accessible to exp[eriment]" (CPAE, 3, Doc. 11, 325). See also Abraham, 1905, 22 for a similar remark. In other terms, "we extend the application of the concept to cases in which the definition [of the field strength in terms of force extorted to small charged bodies] finds no direct application" (CPAE, 3, Doc. 11, 325). It is in this context that, as we have mentioned, Einstein claimed (just like Weyl, cf. page 14), that the individual parts of the theory should not be confronted with experience, only the theory as a whole should be (see above footnote 7).

was forced to address this issue in public briefly at Bad Nauheim, and in a more articulate way in his famous lecture "Geometrie und Erfahrung."

3.1. *The rods, the clocks and Born's Principle of The Physical Identity of The Units of Measure*

Even if Einstein was convinced that Weyl's attempt to build a unified field theory was untenable, he had clearly gained an appreciation for the fact that the independence of rods and clocks from their prehistory, which he has deemed as sufficiently natural in the correspondence with Besso (cf. above section 2.1), was clearly not self-evident and the same essential to warrant the empirical content of both relativity theories. At the beginning of 1920, in a long unpublished article intended for *Nature* on the conceptual development of relativity, Einstein felt compelled to add explicitly that special and general relativity have to assume the "independence of measuring rods and clocks from the prehistory of their motions" (Einstein, 1920a, 12/280). When "unit measuring rods—compared in relative rest—are found to be equal" (Einstein, 1920a, 12/280), then they should always be equal when they are compared again. In a note he appended to the manuscript, Einstein insisted that "this sort of equality (also for clocks), one that is enduring and independent of the pre-history of the motion, is a central presupposition of the whole theory" (Einstein, 1920a, 280, n. 19).

As we have seen, Einstein was aware of this presupposition at latest at the end of 1916 (thus before Weyl's theory was formulated). Weyl's theory had the effect to force Einstein to mention explicitly the prehistory-condition among the presupposition of both of his theories. Only physical systems satisfying this condition really exist in nature, the line element ds can be considered a "phys[ically] significant invariant" (CPAE, 7, Doc. 19, 149). As Einstein wrote to Cassirer in June 1920 "Conceptual systems appear empty to me, if the manner in which they are to be referred to experience is not established" (Einstein to Cassirer, 06-06-1920, CPAE, 10, Doc. 44, 293). In particular "[w]ith the interpretation of the ds as result of measurement, which is obtainable by means of measuring rods and clocks the general theory of relativity as a *physical* theory stands or fall" (Einstein to Cassirer, 06-06-1920, CPAE, 10, Doc. 44, 293). General relativity is a physical theory *because* it is possible to determine physically the absolute value of ds. E.g. two time-like ds would be considered as 'equal', if between the beginning and the end of the interval the same number of vibrations emitted by identical atoms occurs. As we have seen, the gravitational red shift, just like the transverse Doppler effect in special relativity, can be taken as an empirical confirmation of general relativity, only because different atoms of the same substance can be regarded as *identically constructed* clocks reproducing the identical unit of time.

Put in more general terms, the empirical verification of the relativity theory presupposes what Max Born, in his above mentioned Frankfurt lectures on relativity, labeled the "principle of the physical identity of the units of measure" (Born, 1920, 191; tr. Born, 1922, 211). Even if Born mentioned the principle only in passing (and only referring to special relativity, cf. Pierseaux, 2003), it seems not exaggerated to claim that the validity of the principle was the *conditio* *sine qua non* of the physical content of a space-time theory: the quantities special and general relativity are dealing with posses a physical meaning, only if they are consistently measured with the same, reproducible 'identical' units. After all measurements in special and general relativity are at the last instance reducible to comparison of lengths and all predictions are tested by performing such measurements.

Weyl's theory had then the 'epistemological' merit to have insinuated the doubt that Born's principle, the constancy relative length of unit rods and of the relative periods of unit clocks, could be wrong, or at least that relativity theory gave no clue for assuming that it actually holds in nature. This was for instance Besso's point of view, as it can be inferred from Einstein's response to him in a letter dated 20 August 1920:

> The remark about Weyl's book refers to his theory of electricity. I understand your view. You think: *The invariance [Unveränderlichkeit] of the relative extension of bodies* does not need to be put in the foundation of the theory; it would be more beautiful if it comes out as a *consequence* or acceptable if it had *a special place* in the theory hypothesis. But do not forget that the [Weyl's] theory is based on a measuring rods geometry. Then it is assumed that the relative lengths of measuring rods is a function of their prehistory, whereas the *real* measuring rods and clocks are relatively invariant. Then the measuring-rods that one uses in the foundation of the theory are only *thought measuring* rods [nur gedachte Massstäbe] that behave differently from the *real ones*. This is repugnant (Einstein to Besso, 20-08-1920, CPAE, 9, Doc. 85, 347; my emphasis).

Einstein was ready to admit that it would be in principle better if 'the invariance of the relative extension of bodies' would come out as a 'consequence' of the theory, as Weyl had suggested.[15] However, he also saw the possible unpleasant implications of this attitude. This would have allowed Weyl to claim that a non-Riemannian geometrical law of vector-transfer—the behavior of the 'thought' rods—at the basis of his own theory, in spite of the fact that physical behavior of the 'real rods' turns out to be Riemannian. Einstein found this position repugnant, precisely for the reason that it would imply that the 'real rods' have a behavior, which contradicts that of the 'ideal ones'.

3.2. The Bad Nauheim Debate

A month later, in September 1920, at the 85th Bad Nauheim meeting of the German *Naturforscher Versammlung* (mostly famous for Einstein's confrontation

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[15] It is worth noticing that this concern was shared by other relativists at about the same time. Also, Eddington insisted that "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, 1920a, 152). It would be preferable to "define matter in terms of the elementary concepts of the theory; then we can introduce any kind of scientific apparatus, and finally determine what property of the world that apparatus will measure" (Eddington, 1920b, 191).

with his anti-relativity critics), Weyl pursued for the first time precisely this strategy, that he had anticipated in his 1919 writings (cf. above page 21). He outlined a now well-known speculative explanation for the discrepancy between the behavior of 'ideal' and 'real' rods. Roughly, Weyl suggested that atoms that we use as clocks might not *preserve* their size if transported, but *adjust* it every time to some constant field quantity, which he could identified with the radius of the spherical curvature of every three-dimensional slice of world, which would have furnished a natural unit of length. The geometry which is read off from the behavior of material bodies, would appear different from actual geometry of the space-time, because of the 'distortion' due the mechanism of the adjustment.

In his interventions at the Bad Nauheim Einstein maintained his characteristic ambiguous attitude towards the role of rods and clocks in the theory. After Weyl's talk, he insisted once again that the "arrangement of my conceptual system, for me it has become decisive [massgebend] to bring elementary experiences into the language of signs [Zeichensprache]" (Einstein's reply to Weyl, 1920, 650). For Einstein "temporal-spatial intervals are physically defined with the help of measuring rods and clocks" (Einstein's reply to Weyl, 1920, 650), under the assumption that "their equality is empirically independent of their prehistory." (Einstein's reply to Weyl, 1920, 650) Einstein insisted that precisely upon this assumption rests "the possibility of coordinating [zuzuordnen] a number ds to two neighboring world points" (Einstein's reply to Weyl, 1920, 650); if this would be impossible general relativity would be robbed of "its most solid empirical support and possibilities of confirmation" (Einstein's reply to Weyl, 1920, 650).

During the discussion after Max von Laue's paper on the redshift Einstein showed however a quite different attitude. Laue showed that the co-ordinate interval ϑ measured by an atom on the sun is transmitted unchanged by light signals (at least in a statical gravitational field[16]), so that the red shift emerges by confronting the frequency of such signals with those of an atom of the same type at rest measuring the proper time $d\tau$. Replying to some concerns expressed by the mathematician Georg Hamel, during the discussion, Einstein pointed out that "[s]ince the emitting atoms are to be regarded as a clock in the sense of the theory" (Einstein's reply to Laue, 1920), that is as identically constructed clocks, "then the redshift is one of the safest results of the theory". One can 'verify' that the relation $d\tau^2 = g_{44} d\vartheta^2$ established by general relativity actually holds in reality. After all, during the same session of the conference, two young Bonn spectroscopist Leonhard Grebe had communicated very favorable results (obtained in collaboration with Albert Bachem) measuring the shift of cyanide band of the sun spectrum (Grebe, 1920; cf. Hentschel, 1992, for more details).

At the same time Einstein, however, in the very same sentence, was again not afraid to admit that "[it] is a *logical shortcoming* of the theory of relativity in its present form to be forced to *introduce measuring rods and clocks separately instead of being able to construct them as solutions to differential equations*"

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[16] In the general case, the number of vibrations of an atom transmitted by light signals is coordinate dependent.

(Einstein's reply in Laue, 1920, 662; my emphasis). Thus Einstein apparently believed that it would be logically or epistemologically preferable, if the field equations of the theory would have suitable solutions corresponding to particles, from which in principle the stability of more complicated bulky configuration of matter could be reconstructed, including rod-like and clock-like structures. In particular only some months before his famous Leiden talk on aether and relativity (Einstein, 1920b) Einstein expressed again his preference on a theory in which particle-like matter are nothing but particular "condensations [Verdichtungen] of the electromagnetic field" (Einstein, 1920b, 14).

It was probably against this field-theoretical approach that the young Pauli, intervening at Bau Nauheim, had insisted on the necessity for material rods and clocks external to general relativity itself. Just like the field strength in the interior of the electron is meaningless since there is no smaller test particle than the electron, "one could claim something similar concerning spacial measurement, since there are no infinitely small measuring-rods" (Pauli's reply to Weyl, 1920, 650). If the time or space interval have a physical basis, only if some actual or possible physical process or object has a length or a duration, which is shorter or equal to the time interval in question. A distance smaller than the electron would be physically meaningless since there is physical process which could realize such an interval. The attempt to define the 'field strength' in the interior of rods and clocks, considering rods and clocks as solutions of the theory, would then deprive the theory of every empirical content. The latter is assured by the very possibility of concretely realizing the physical identity of the unit of measure, to define "the unit of length via their reference to the atomic latices of a certain crystal and the unit of time with the reference to vibration time of certain spectral lines" (Wiechert, 1921, 44), real existing physical systems which happen to be 'identically' reproducible with extreme precision. (Weyl, 1920, 650)

Pauli's objection, together with many others, appeared in a draft manuscript of his contribution on relativity the *Enzyklopadie der Wissenschaften* which must have already circulated at Bad Nauheim. Probably, after reading such draft, Weyl decided to give up on a possible field theory of matter in the sense of Mie (see letter of Weyl to F. Klein on 28 December 1920), and started to conceive particles as singularities (cf. Scholz, 2006). However, Weyl did not agree with Pauli on the necessity to assume rods and clocks as empirical indicators external to general relativity. The 4th edition of *Raum, Zeit, Materie* (Weyl, 1921a)—finished in November 1920—introduced the new strategy explained in Bad Nauheim. The idea that real clocks behave according to laws of Weyl's world geometry is contradicted by the behavior of 'atomic clocks', which emit spectral lines of a definite frequency. Weyl needed therefore to explain "the discrepancy between the idea of congruent transfer and the behavior of measuring-rods and clocks and atoms" (Weyl, 1921a, 280; tr. Weyl, 1922a, 308).

The mechanism of adjustment is the only way to explain the surprising fact that electrons and hydrogen nuclei have always the same mass and charge, and thus the very existence of identical atoms, and possibly the fact that such atoms, under given external conditions, settle into identical crystalline structures. However, the fact that all electrons have always the same charge is incomprehensible from the point of view of Lorentz-Maxwell theory, and it had to be introduced as separate hypothesis (cf. Weyl, 1921a, 281). Classical electrodynamics demands the overall conservation of the charge e ($de/dt = 0$), but the charge of an individual macroscopic body will not generally remain constant given the body's interaction with its environment.

Weyl had then some good reasons to claim that the only explanation for the fact that electrons have always the same charge, whatever their prehistory, might have been to assume that there is some field quantity of the dimension of a length (i.e. it is simply a number) to which the charge of the electrons 'adjust' itself in a certain proportion. This was after all the general framework that, e.g. Mie (who also gave a talk at Bad Nauheim, Mie, 1920) had tried to realize without any success (cf. also footnote 6). A certain state of equilibrium of the negative (or positive) electricity would have always been 'reestablished' whatever disturbance it might have experienced in the past, just like the magnetic needle on the earth re-orients its direction to the north of the earth magnetic field, despite what happened to it in the past.

Weyl's distinction between 'adjusting' and 'persisting' quantities was apt more to express a problem, rather than to offer a solution, and in this sense Weyl never abandoned it, even when he had become detached from the very idea of a unified field theory (see below section 5.3; for a very effective presentation of this distinction see Fogel, 2008, section 3.2). The very existence of 'identically constructed' rods and clocks should be "decided on the basis of the actually valid natural laws" (Weyl, 1921a, 281), and not assumed from the outset as separate hypothesis. Einstein's assumption that physical clocks exist which are not influenced by their prehistories was after all everything but obvious. Two identical 'classic' atomic systems with different prehistories would probably differ in some small detail due to their interaction with the environment, and their spectral lines would be slightly shifted, so that classically, a spiralling charge should emit light of all colors. Emerging quantum theory had already made clear that the spectral identity of atoms revealed by experience cannot be explained in this framework. The fact that all atoms of the same type are *exactly* identical clearly cannot depend in an initial agreement established in the past, which has been 'preserved' since then, in spite of the fact the atoms had passed through very different physical circumstances. It was more plausible to argue that they 'adjust' every time a new to a certain equilibrium value.

According to Weyl, without such a dynamical explanation of their persistence, the use of rods and clocks as empirical indicators should be possibly avoided. On 28 January 1921, he presented to the *Gesellschaft der Wissenschaften zu Göttingen* a paper in which he proved rigorously—in terms the relations between conformal and projective structure of space-time—that the metric g_{ik} (and thus the space-time geometry) can determined (up to a choice of a measuring unit) by the behavior of point particles and light rays "measuring rods and clocks are not necessary for this" (Weyl, 1921b, 101).[17] However, two g_{ik}-systems agreeing on free-falling particles and light rays trajectories might differ by a constant factor (one might still be the scaled replica of the other). Thus, no global definition of the measuring units, implied in the fully Riemannian structure of space-time, was not possible with this method (cf. Ehlers, 1988).

3.3. Einstein's 1921 Lecture on the Foundation of Geometry

On 27 January 1921 at the Berlin Academy's Leibniz-day public celebration, Einstein gave an address entitled "Geometrie und Erfahrung," published in March as a paper and later as booklet in expanded form (Einstein, 1921a). The influence that Einstein's talk exerted on the history of philosophy of science does not need to be emphasized (cf. Howard, 2009). It is not at all surprising that Einstein would consider the lecture as a good occasion to place himself in the epistemological spectrum—whose extremal positions were possibly represented by Pauli and Weyl—using a language that would be accessible to the a non-specialist, but 'cultured' public attending a lecture.

Einstein famously attached "special importance to the view of geometry" (Einstein, 1921a, 126; tr. Einstein, 1954a, 235) as the study of the laws of disposition of practically-rigid bodies, because "without it I should have been unable to formulate the theory of relativity" (Einstein, 1921a, 126; tr. Einstein, 1954a, 235). However Einstein emphasizes that there are good reasons to "reject the relation between the body of axiomatic Euclidean geometry and the practically-rigid body of reality" (Einstein, 1921a, 126; tr. Einstein, 1954a, 235). The geometrical behavior of physical bodies depend upon temperature, external forces, etc. Thus, as Poincaré has shown, if the behavior of bodies contradicts Euclidean geometry, the latter can always be saved, by blaming the apparent non-Euclidean behavior of rods and clocks to some distorting influences.

Of course, the supporter of practical-geometry could claim that it is always possible to isolate bodies from such distorting influences and thus "to determine the physical state of a measuring-body so accurately that its behavior relative to other measuring-bodies shall be sufficiently free from ambiguity, allowing it to be substituted for the 'rigid' body" (Einstein, 1921a, 127; tr. Einstein, 1954a, 237). This would imply however that one possesses a detailed knowledge of the dynamical laws which describe the material constitution of the measuring bodies. In this sense Poincaré could rightly point out that "[g]eometry (G) predicates nothing about the behavior of real things, but only geometry together

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[17] Weyl's result was pathbreaking. However, from an epistemological point of view one could object that the fact that light travels along null geodesics—just like the behavior of clocks—cannot be derived from the field equations of general relativity without calling upon the equations governing the propagation of electromagnetic radiation; nor at that time was it possible to derive the fact that free particles follow geodesic from the gravitational field equations. Weyl presented such result in the 5th edition of his *Raum, Zeit, Materie* (Weyl, 1923) before Einstein reached a similar result in a paper with Jakob Grommer (Einstein and Grommer, 1923; see Havas, 1989, for more details). On the opposition between principles that can be derived, field equations, and principles whose explanation is to be found in the dynamical equations governing particular matter fields see Malament, 2009.

with the totality (P) of physical laws can do so" (Einstein, 1921a, 126–127; tr. Einstein, 1954a, 236). Using symbols, Einstein famously claimed that "only the sum of (G) + (P) is subject to experimental verification" (Einstein, 1921a, 127; tr. Einstein, 1954a, 236). This, of course, is the exact opposite of the strict opposition between axiomatic and practical geometry from which the lecture started. Einstein maintains then a characteristic ambivalent stance which is well expressed by this famous passage:

> *Sub specie aeterni* Poincaré, in my opinion, is right. It is also clear that the solid body and the clock *do not in the conceptual edifice of physics play the part of irreducible elements, but that of composite structures*, which must not play any independent part in theoretical physics. But it is my conviction that in *the present stage of development of theoretical physics* these concepts must still be employed as independent concepts; for we are still far from possessing such certain knowledge of the theoretical principles of atomic structure as to be able to *construct solid bodies and clocks theoretically from elementary concepts* (Einstein, 1921a, 127, my emphasis; tr. Einstein, 1954a, 236).

In spite of the reference to Poincaré, the alternatives that Einstein describes in this famous passage strongly resemble those that Einstein had addressed in the letter to Dällenbach some years before and later discussed in the correspondence with Weyl: *Sub specie temporis*, in the present state in the development of physics, Einstein had adopted the view that rods and clocks should be used to give physical contents to physical quantities, special and general relativity deal with, in spite of the fact that both theories does not have the conceptual means to explain the stability of such material systems; *sub specie aeterni*, however, rods and clocks should emerge as solutions of a future theory of matter, possibly a filed-theory encompassing gravitation and electromagnetism, able to account for the existence of rods-like and clock-like matter configuration, just like that of any other.

Again, Einstein was clearly pleading for a 'dynamical explanation' of the behavior of rods and clocks; but it is important to emphasize once again which kind of behavior was initially assumed as obvious and it needed to be 'dynamically' explained. According to Einstein "[a]ll practical geometry is based upon a principle which is accessible to experience" (Einstein, 1921a, 127; tr. Einstein, 1954a, 237). Let us call two (irreversible) marks, upon a practically-rigid body a "tract" (Einstein, 1921a, 127; tr. Einstein, 1954a, 237). Then two tracts are said to be 'equal to one another', that is, they have the same length, if the 'marks' of the one tract can be brought to coincide with the marks of the other. An analogous process, can be imagined for clocks leaving (irreversibly) mark on a dial, which can be in some sense be brought to coincidence with marks left by another clock. It must be presupposed that this procedure leads to consistent results:

> If two tracts are found to be equal once and anywhere, they *are equal always and everywhere* [...] The above assumption for tracts must also hold good for intervals of clock-time in the theory of relativity [...]: if

> two ideal clocks *are going at the same rate at any time and at any place* [...], they will always go at the same rate, no matter where and when they are again compared with each other at one place. If this law were not valid for natural clocks, the proper frequencies for the separate atoms of the same chemical element would not be in such exact agreement as experience demonstrates. *The existence of sharp spectral lines is a convincing experimental proof* of the above-mentioned principle of practical geometry. This, in the last analysis, is the reason *that enables us to speak meaningfully of a Riemannian metric* of the four-dimensional space-time continuum (Einstein, 1921a, 127–128, my emphasis; tr. Einstein, 1954a, 237).

There is little doubt, that Einstein reiterates here his argument in favor of the fact that atoms are reliable clocks that he had used against Weyl's theory: the existence of well defined spectral lines for atoms of the same time independently of what might have happened to them in the past, provided a compelling 'empirical evidence' that the geometry of space-time is Riemannian.[18] If the choice of the units of measure is arbitrary, the ratio of such units can be regard as absolute dimensionless constant. It is under this presupposition we can norm time and length intervals once and for all, so that the length of the interval between any to pair of space-time points can be compared with that of any other in terms of these units. After all the entire content of relativity makes predictions about the value of length and time differences that these predictions can be tested empirically, it is essential that these units of measure should be physically reproducible.

Once such assumption has been granted, then the question which is the geometry of space (Euclidean and non-Euclidean) "must be answered by experience, and not a question of a convention to be chosen on grounds of mere expediency" (Einstein, 1921a, 127; tr. Einstein, 1954a, 238). Let's consider a small rigid rod, which has the same length in every position and in every orientation. If its length is determined solely by the coordinate differences of its end points then the geometry is Euclidean. If this turned out to be impossible, for any choice of coordinates, then the geometry of space would be non-Euclidean, and coordinates distances should be multiplied by the 'conversion factors' $g_{\mu\nu}$ to be translated in natural distances. In this sense practical geometry simply summarizes the laws

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[18] Michael Friedman, in one of the most cited and influential paper on "Geometrie und Erfahrung" claimed that Einstein's paper should be understood "against the background of a preceding conception of geometry—one that was dominant in the nineteenth century" (Friedman, 2002, 196). More recently Tom Ryckman (Ryckman, 2005) had insisted that Einstein's "Geometrie und Erfahrung" should be read against the Einstein-Weyl debate. I agree with Ryckman that Einstein's homage to the classical Helmholtz-Poincaré debate should be considered merely as a rhetorical device (see also below page 40). However, I disagree with Ryckman that the appearance of Weyl's theory was responsible for having made Einstein change his mind about the use of rods and clocks as unstructured entities. As I have tried to show, the correspondence with Dällenbach shows that Einstein's position was fully defined before Weyl's theory was conceived (cf. above section 2.1). Moreover, as Howard has shown, Einstein was committed to the ideal that only the theory 'as a whole' can be compared with experience at least in 1910-11 cf. above footnote 7 and footnote 14.

of disposition of practically-rigid bodies, and can be empirically confirmed or falsified.

This measuring procedure however implies that rigid rods actually exists, which have the 'same' length in every position and in every orientation. However, a 'rod' is made up of a definite number of atoms held to together by electrostatic forces governed by quite definite physical laws; it could be said to be rigid, that is to preserve the 'same' length, when it is expected to 'settle' every time into the 'same' equilibrium configuration in every position and in every orientation according to this laws. In this scenario, what is actually compared to reality is of course only the sum of geometry and the physical laws governing the behavior of rods.[19]

With a remark that seems to address Pauli's point at Bad Nauheim, Einstein admits that the practical approach to geometry would probably fail under submolecular level, since there are no smaller rods then electrons, so it does not make sense to 'measure' their magnitudes. The validity of the practical geometry 'inside' of elementary particles would be based on extrapolation. We could for instance raise the question, if "gravitational fields play a role in construction of matter" (CPAE, 7, Doc. 50, 377) and thus "continuum in the interior of atomic nuclei can considered as noticeably non-Euclidean" (CPAE, 7, Doc. 50, 377), even if in principle there are no rods or clocks smaller than the elementary particles. Einstein recognizes that this extrapolation might fail, just like the concept of temperature (defined as the average translational kinetic energy of the molecules), cannot be attributed to molecules themselves.

There is then enough textual evidence that Einstein's lecture was concerned by the very same problem that was discussed in the Einstein-Weyl correspondence. The phrasing used by Einstein in a paper published on 17 March 1921 the *Sitzungsberichte* (Einstein, 1921a) seems to confirm this reading. Riemann's geometry is based the following hypothesis: "I. Existence of transportable measuring rods, II. independence of their length from the path of transport. Weyl's generalization of the Riemannian metric maintains I. and drops II." (Einstein, 1921b, 262). However Weyl's attempt is refuted by the fact that "*we do not know any physical thing whose relative extension depends on its prehistory*" (Einstein, 1921b, 262; my emphasis).

Weyl's theory was then essential to force Einstein to justify in public (he had already recognized this point in private at latest in the above mentioned letter to Besso cf. above page 6) the fact that relativity theory—in oder to gain

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[19] Ebenezer Cunningham, one of pioneers of relativity in Britain, had emphasized this very same point in the revised version (Cunningham, 1921, finished in 1920) of his book on relativity (Cunningham, 1915). Cunningham remarks that the plates and the micrometer screw that Eddington used to verify the eclipses results are assumed to behave like 'rigid bodies'. However,"rigid body of experience is essentially a complicated dynamical system" (Cunningham, 1921, 131). In this sense as Cunningham acutely remarks, "its property of permanence is of the same kind as the permanence of the solar system as a whole" (Cunningham, 1921, 131). Thus, as Cunningham ironically put it, it is not clear if the Eddington's eclipse expedition "is as much a confirmation of this property of the rigid body as it is a verification of Einstein's theory of the nature of gravitation" (Cunningham, 1921, 131).

physical content—has to rely on the existence of rather complicated material structures, in stable or oscillating equilibrium, who always measure the same lengths and periods whenever they are compared regardless of their history. In his Princeton lectures held in May 1921, Einstein—after having presented the vision of geometry as a branch of physics (Einstein, 1922b, 2–4; 40–42)—was then forced to insist explicitly on the fact that this assumption was essential if the line-element ds is to be regarded as "directly measurable by our unit measuring rods and clocks" (Einstein, 1922b, 41; tr. Einstein, 1923a, 67), and and it is therefore "a uniquely determinate invariant for two neighboring events" (Einstein, 1922b, 41; tr. Einstein, 1923a, 67). In different terms, Einstein realized that the empirical meaningfulness of relativity theory was entirely based on "*the physical assumption*" (Einstein, 1922b, 41; my emphasis; tr. Einstein, 1923a, 67) that "*the relative lengths of two measuring rods and the relative rates of two clocks are independent, in principle, of their previous history*" (Einstein, 1922b, 41; my emphasis; tr. Einstein, 1923a, 67). Einstein considered it sufficient to point out that "this assumption is certainly warranted *by experience*" (Einstein, 1922b, 41; my emphasis; tr. Einstein, 1923a, 67). If not "there could be no sharp spectral lines; *for the single atoms of the same element certainly do not have the same history*" (Einstein, 1922b, 41; tr. Einstein, 1923a, 67), and "it would be absurd to suppose any relative difference in the structure of the single atoms due to their previous history *if the mass and frequencies* [Eigenfrequenzen] *of the single atoms of the same element were always the same*" (Einstein, 1922b, 41; my emphasis; tr. Einstein, 1923a, 67).

If we grant these assumptions, which, according to Einstein, "naturally arise from experience" (Einstein, 1922b, 41; tr. Einstein, 1923a, 67), then geometry is related to behavior of physically realizable physical systems, and its theorems are statements concerning their behavior, statements which may be proved to be true or false. One can verify the predictions of general relativity, e.g., the value of geometrical/physical quantity g_{44} by measuring the difference between the frequency with which a signal is emitted by an atomic clock on the surface of large mass and the frequency with which the signal is detected at the position of the other atomic clock. Of course, in principle, one can always account for the fact that the redshift is not observed, by assuming that the emitting atomic system on the sun are not 'identical' to the one on earth (that is they do not measure the 'same' proper time) in order to compensate for the appearance of the factor $\sqrt{g_{44}}$.[20] In this sense, what we compare to experience is always

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[20] A similar point was made by Ludwik Silberstein at the beginning of 1921, in his Toronto lectures on relativity: "Einstein's intransigent attitude" (Silberstein, 1922, 105) towards the redshift "proves only the strength of his belief that the atoms are or will turn out to be such natural, ideal clocks". Einstein assumed, "as he did already in other circumstances in the special relativity theory, that *the said two atoms are 'equal' to each other in the sense of the word that the proper times of their vibration periods are equal to each other*" (Silberstein, 1922, 105, fn.; my emphasis). Yet, "the hypothetical nature of the *sameness of atoms* in the explained sense of the word, such an attitude, though personally intelligible, is by no means necessary" (Silberstein, 1922, 105). According to Silberstein it is after all "only a guess" (Silberstein, 1922, 105), even if "a very reasonable one" (Silberstein, 1922, 105). To prove this

geometry (the value of the g_{44}) plus physics (the theory of atomic spectra).

3.4. The Consolidation of Pauli's Observability Criterion of Meaning

It is precisely because of these epistemological conundrums, that Weyl still believed that he had some cards to play. During 1921, the 'pivotal year', as it has been called, (Vizgin, 1994), Weyl (followed to some extent by Eddington, 1921a; Eddington, 1921b), presented his strategy of 'doubling the geometry' (the real 'aether geometry' and the 'body geometry' distorted by the mechanism of adjustment) in three papers intended for different audiences, finished in February (Weyl, 1921b), May (Weyl, 1921c) and July (Weyl, 1921d). As he put it in his reconstruction of the Bad Nauheim debate—which was finished in August 1921 (but published in the following year as Weyl, 1922b)—the problem of how "the *comparison of the units of the ds in different world locations* is possible" (Weyl, 1922b, 52) could not simply solved by returning to the happy coincidence that identical atoms exist which show consistently the same spectral signature.

However, Weyl had to face a much more fierce opponent who was emerging as the rising stars of German physics. In September 1921 Pauli's encyclopedia article on relativity theory (which was finished in December, but underwent to some improvements in April and May) was finally published, as part of the fifth volume of the *Enzyklopädie der Mathematischen Wissenschaften* and later as a book with the introduction of Pauli's mentor Arnold Sommerfeld (Pauli, 1921). The article was unanimously considered as a masterpiece, in particular by Einstein's himself (Einstein, 1922a). Part 5 contains a profound, rightly famous critique of the very idea of field-theoretical treatment of the problem of matter, that is of the very pretense of "continuum theories deriving the atomic nature of electricity from the differential equations" (Pauli, 1921, 774) governing the field. In particular Pauli insisted again on his "conceptual reservation [Bedenken]" (Pauli, 1921, 774) about the measurability of the field strength in the interior of the electron.

Weyl had actually abandoned Mie's program, suggesting that it would have been sufficient to consider the fields 'outside' of the electron (the volume integrals could be turned into a surface integrals; cf. Weyl, 1921c; see also Weyl, 1924, for a popular presentation). However, § 65 of Pauli's article entails a devastating epistemological 'deconstruction' of the evolution of Weyl's epistemological attitude, which was hard to dismiss. As Pauli points out, "[i]nitially, just as in Einstein's theory the gravitational effects are closely linked with the behavior of measuring rods and clocks, such that they follow from it unambiguously, so the same holds in Weyl's theory for electromagnetic effects" (Pauli, 1921, 762). However, If one attempts a direct physical interpretation of the geometrical law of vector-displacement valid in Weyl geometry as a description of the behavior of rods and clocks, then one arrives to a conclusion that does not conform to experience. The effect of the electromagnetic field should be noticeable in the

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guess one would need a consistent theory of atomic spectra—an issue on which Silberstein had extenstively written (Silberstein, 1920)—which is at least as uncertain as relativity theory.

spectral lines of a given substance, and as Pauli shows, the “the differences would increase indefinitely in the course of time” (Pauli, 1921, 763).

Since this is clearly in conflict with spectrographic data, Weyl could not avoid to renounce this interpretation: “The *ideal* process of the congruent transference of world length” (Pauli, 1921, 763; my emphasis), has “nothing to do with the *real* behavior of measuring rods and clocks; the metric field must not be defined by means of information taken from these measuring instruments” (Pauli, 1921, 763). Weyl’s distinction between ‘preservation’ and ‘adjustment’ was meant precisely to explain this discrepancy between the geometrical laws of transfer of vectors and the physical behavior of measuring-rods and clocks.

In this way, however, not only was Weyl “led to conjecture that a connection exists between the size of the universe and that of the electron, which might seem somewhat fantastic [etwas phantastisch]” (Pauli, 1921, 770). In Weyl’s theory the “the quantities g_{ik} and φ_i, are, by definition, no longer observable, in contrast to the line element ds^2 of Einstein’s theory” (Pauli, 1921, 763). In this way of course “no longer exists a direct contradiction with experiment” (Pauli, 1921, 763), but at the same time the theory appears “to have been robbed of its inherent convincing power” (Pauli, 1921, 763), since there “is no longer an immediate connection between the electromagnetic phenomena and the behavior of measuring rods and clocks” (Pauli, 1921, 763). As Pauli realized, Weyl could defend his theory only by introducing a ‘dynamical conspiration’ of nature to hide his ‘real’ non-riemannian geometry, that is, by making it in principle unobservable, and thus deprived of any physical content.

Thus, Pauli came to appreciate the definition of Weyl’s geometry as a ‘graphical representation’, that Eddington had suggested in his celebrated handbook on relativity *The Mathematical Theory of Relativity* (Eddington, 1923, dated August 1922) as somehow illuminating (cf. also Eddington, 1922). “The original form of Weyl’s theory” (Pauli to Eddington, 23-09-1923, Pauli, 1979, Doc. 45, 118), Pauli wrote to Eddington in a famous letter in September 1923, and had the ambition to make a statement about the behavior of rods and clocks: “it is however not in consonance with experience. The length of measuring rods and the periods of clocks in reality do not depend on their prehistory” (Pauli to Eddington, 23-09-1923, Pauli, 1979, Doc. 45, 118). By the contrary, “[i]n the second form of Weyl’s theory” (Pauli to Eddington, 23-09-1923, Pauli, 1979, Doc. 45, 118)—Pauli remarks—a “a very different point of view” (Pauli to Eddington, 23-09-1923, Pauli, 1979, Doc. 45, 118) was assumed “that you so effectively described with the difference between ‘natural geometry’ and ‘world geometry’, being the latter only a graphical representation of reality” (Pauli to Eddington, 23-09-1923, Pauli, 1979, Doc. 45, 118). Such theories—and Einstein’s new theory (see below on page 37) inspired by Eddington was no exception—present only a formal scheme in which “then we have not only no ‘natural geometry’, but also no ‘natural theory’” (Pauli to Eddington, 23-09-1923, Pauli, 1979, Doc. 45, 118)[21].

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[21] The expressions ‘world geometry’, ‘natural geometry’ and ‘natural theory’ are in English

Pauli found no logical contradiction in Weyl's (or Eddington's) approach, but rather an epistemological shortcoming. The letter to Eddington contains probably the most extensive profession of Pauli's philosophical *credo*. According to Pauli, the notion of field only makes sense if we are able "to indicate a reaction, which, in principle, would allow to measure the field strengths at every space-time point, if we so desire" (Pauli to Eddington, 23-09-1923, Pauli, 1979, Doc. 45, 116 in). In particular, the electromagnetic field strengths in the classical electrodynamics are defined as the force exerted to a charged test body. It is not required that we actually perform the measurement with a real test body; "it is essential only that we can realize it in principle every time we want" (Pauli to Eddington, 23-09-1923, Pauli, 1979, Doc. 45, 116). As soon as the reaction ceases to be specifiable or in principle executable the respective field concept is no longer defined (Pauli to Eddington, 23-09-1923, Pauli, 1979, cf. Doc. 45, 116):

> I adhere to the (of course, unprovable) viewpoint that each physical theory, which claims to provide a sensible answer to these questions, must start with a definition of the field quantities used; the definition should state how these quantities can be measured. It [the theory] must further reveal relations between electromagnetic quantities and those measured by other methods. (The most beautiful success of relativity theory was indeed that it yielded a deep and tight connection between the results of measurements of rods and clocks, the orbits of freely-falling mass points and those of light rays.) These postulates cannot be proved logically or by means of the theory of cognition [erkenntnistheoretisch]. However, I am convinced that they are correct (Pauli to Eddington, 23-09-1923, Pauli, 1979, Doc. 45, 117).

As Born's reaction to Pauli's objection to Weyl's theory already portents (cf. above section 2.4), Pauli's epistemological attitude will turn out to be extremely influential, especially among the Göttinger physicists that will start to develop quantum mechanics in the following ten years. As Pauli wrote to Bohr at the end of 1924 the "observable properties of the stationary states", such as the intensity of spectral lines should be considered as much "*something much more real* than the 'orbits'" (Pauli to Bohr, 31-12-1924, in Pauli, 1979, Doc. 79, 197) of electrons around the nucleus in Bohr-Sommerfeld atomic model.

This attitude clearly prevailed, among the Göttinger community. In his 1924 lectures on atomic mechanics, Born (Pauli has been his assistant in 1921/22) sketched in the same spirit the epistemological framework which he felt would ultimately prove successful in quantum theory, insisting that quantum laws "involve only observable quantities" (Born, 1925, 114), explicitly invoking special relativity as the source of such methodological principle (Born and Jordan, 1925, 479fn.). It is not hard to recognize the very same methodological stance will reemerge in the opening sentences of Werner Heisenberg's *Umdeutung* paper (Heisenberg, 1925). As is well known, in the conviction of following Einstein's

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in the original.

lead, Heisenberg (Pauli's successor as Born's assistant in Göttingen) would use the very same insistence on the neat distinction between observable and unobservable quantities to justify matrix mechanics. Heisenberg will be then be understandably surprised, when, some years later, Einstein would tell him after a talk in Berlin in 1926 that, "[i]t is the theory which decides what we can observe" (according to Heisenberg's recollections, AQHP, 25-02-1963, 9 and Heisenberg, 1966, 80; cf. Camilleri, 2009, section 2.4 for more details).[22]

4. 1923-1930s. The Assessment of Einstein's Two-Stage Strategy

On the background of his first contributions to a unified field theory, Einstein, in some more or less popular writings (in particular his Noble prize lecture), returned to justify his provisional use of of rods and clocks as indispensable devices to give the geometrical statements of the theory an empirical content, in spite of the fact that he agreed on the fact that they are complicated physical systems described by rather complicated physical laws.

When Pauli mentioned to Eddington his concerns about Einstein's new theory, he meant a series of notes that Einstein had published in the Proceedings of the Prussian Academy starting from the beginning of 1923 (Einstein, 1923b; Einstein, 1923c), to which he later added a paper in English in the september issue of *Nature* (Einstein, 1923e). By January 1923, on his return trip from Japan, Einstein's had admittedly become infatuated with Eddington's generalization of Weyl's theory. He famously wrote to Bohr from the shipboard: "Eddington has come closer to the truth than Weyl" (Einstein to Bohr 11-01-1923, cit. and tr. in Stachel, 1986b, 240). Weyl wrote a letter to Einstein, criticizing Einstein's paper. However, he was pleased to meet Einstein "on the same purely speculative paths" (Weyl to Einstein, 18-05-1918, cit. and tr. in Stachel, 1986b, 240) against which he "always protested before" (Weyl to Einstein, 18-05-1918, cit. and tr. in Stachel, 1986b, 240).

In this context, Einstein must have probably found even more pressing to give a justification of his attitude toward the relationships between theory and experience. Einstein had after all defended in many occasion a view *à la* Pauli, according to which relativity theory has an empirical content because its predictions may or may not correspond to the behavior of really existing rods and clocks; on the other he had often pointed out it was actually a "a logical inconsequence, because for the measurements it presupposes so complicated structures [Gebilde] such as measuring rods and clocks as given, instead of constructing them from theoretical concepts" (Winternitz, 1923, 224).[23] During

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[22] It must be emphasized, however, that, as Heisenberg pointed out in the next chapter of his Heisenberg, 1966, 111, it was apparently Einstein's remark that helped him to find uncertainty relations.

[23] The philosopher Joseph Winternitz—who was a good friend of Leopold Infeld (who mentioned him simply as 'Joseph' in his Infeld, 1942) and of Einstein's himself—rightly attributes this position to Eddington (cf. Eddington, 1923, 146). Einstein wrote a positive

the twenties Einstein found several occasions to address in public the role of rods and clocks in relativity theory. It is unavoidable to mention a famous passage from his delayed lecture for his 1921-22 Nobel prize delivered to the Nordic Assembly of Naturalists at Gothenburg in July 1923:

> The concept of the rigid body (and that of the clock) has a key bearing on the foregoing consideration of the fundamentals of mechanics, a bearing which there is some justification for challenging. The rigid body is only approximately achieved in Nature, not even with desired approximation; this concept does not therefore strictly satisfy the content-requirement [Inhaltsforderung].[24] *It is also logically unjustifiable to base all physical consideration on the rigid or solid body and then finally reconstruct that body atomically by means of elementary physical laws which in turn have been determined by means of the rigid measuring body [...] It would be logically more correct to begin with the whole of the laws and to apply the 'stipulation of meaning' to this whole first, i.e. to put the unambiguous relation to the world of experience last instead of already fulfilling it in an imperfect form for an artificially isolated part, namely the space-time metric. We are not, however, sufficiently advanced in our knowledge of Nature's elementary laws to adopt this more perfect method without going out of our depth.* 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, 1923d, 2-3; my emphasis).

This well-known passage is coherent with Einstein's stance on this matter in the previous years, even if he now refers more explicitly to special relativity. In Einstein's original approach an inertial coordinate system was thought to satisfy the *Inhaltsforderung*, if it could be actually realized as a material grid of identically constructed rigid rods (with identically constructed clocks at their nodes) at rest, which can be disposed according to the law of Euclidean geometry. It is then required that the laws of physics (for instance Maxwell electrodynamics) should assume the same and most simple form in these systems, including of course those laws that in principle would be responsible for the very existence of the rigid rods of the 'same length'. The latter, Einstein insists again, is in fact, however idealized, at the last instance nothing but a fairly complicated atomic system kept together by electromagnetic forces. As we have seen, Einstein considered logically preferable to proceed in the opposite direction: identical collections of atoms are said to be of the 'same length' if they assume the 'same' equilibrium configuration according to some yet unknown theory of matter. Then only the whole system, describing both the material measuring instruments and the other physical processes as well could be considered as able to satisfy the

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review of Winternitz's book (Einstein, 1924a), which is also a good document of his stance toward Kant's philosophy; cf. Hentschel, 1987.

[24] *Inhaltsforderung* is usually translated as 'stipulation of meaning'. However, the translation obscures the fact that the german expression seems to refer to the conditions that a theory has to satisfy to have physical content.

Inhaltsforderung.

Thus, again, the issue with which Einstein was concerned was of epistemological nature: are there 'individual parts' of a theory that are directly connected to experience or only the entire systems of physical laws is physically meaningful? Initially, Einstein adopted the stance, that it is only under the assumption of the actual existence of rods whose length "would be the same under all circumstances" (Lorentz, 1923, 390), one can give physical meaning to the claim that, in general relativity, it is in general impossible to recover distances measured by rigid rods and clocks form coordinate distances without knowing the quantities $g_{\mu\nu}$, which also play the role of the gravitational field. The reference to the of Weyl and Eddington at the end of passage we have just cited, seems to show that the attempts "to complete the basis of the general theory of relativity" (Einstein, 1923d, 2), by unifying the gravitational and the electromagnetic field had made clear that this epistemological attitude was untenable. The electromagnetic field, "is also the material from which we must build up the elementary structures of matter" (Einstein, 1923d, 2), of which, needless to say, also rigid bodies and clocks are made.

In such a theory, Einstein claims in a contemporary paper, "the field variables produce the field of 'empty space' as well as the electrical elementary particles that constitute 'matter'" (Einstein and Grommer, 1923, 2), eliminating the difference between the gravitational field ($g_{\mu\nu}$) and the other known 'physical' field, the electromagnetic field (φ_ν), which would turn out to be only different manifestations of a single field. At the end of 1923, after his works on Eddington's theory, Einstein published a paper (Einstein, 1923f) in which he suggested to account for quantum phenomena by means of differential equations that contain only the $g_{\mu\nu}$ and the φ_ν, would generate discontinuities by overdetermining the classical variables with more differential equations than field variables. The first original approach put forward by Einstein himself was published in 1925 in a paper, the title of which contains the the term 'unified field theory' for the first time (Einstein, 1925b).

One might venture to say that $(G) + (P)$ formula of the 1921 lecture, beside its general epistemological meaning, seems to bare some relations with the project to provide a field-theoretical account of matter, that is to unify the 'geometrical' field $g_{\mu\nu}$ and the other known 'physical' field, the electromagnetic field φ_ν (for Einstein's conception of unification, cf. Lehmkuhl, 2013). Such a theory would account at the same time for existence of the electrically charged constituents of matter which enter in the very constitution of rods and clocks, which in turn serve to define the gravitational field. In more popular writings from this period Einstein found the occasion to address in possibly simple terms the question of how the development of a general relativistic theory of matter would alter the relationships between the space-time theory and the material devices that serve to its verification. Some of these passages have already attracted the attention of some interpreters (cf. e.g. Howard, 2009), but I will report them as further testimony of Einstein's stance toward the issue of the relationship between theory and measuring devices.

In a 1924 recension of a book by Aldolf Elsbach, a minor Neo-Kantian philosopher, (Elsbach, 1924), Einstein distinguishes two different ‘standpoints’ on the question about the relation between geometry and experience:

> The position that one takes on these claims depends on whether one *grants reality to the practically-rigid body.* If yes, then the concept of the interval corresponds to something experiential. *Geometry then contains assertions about possible experiments; it is a physical science that is directly underpinned by experimental testing* (standpoint A). If the practically-rigid measuring body is accorded no reality, then geometry alone contains no assertions about experiences (experiments), but *instead only geometry with physical sciences taken together* (standpoint B). Until now physics has always availed itself of the simpler standpoint A and, for the most part, is indebted to it for its fruitfulness; physics employs it in all of its measurements. [. . .] But if one adopts standpoint B, which *seems overly cautious at the present stage of the development of physics*, then geometry alone is not experimentally testable. There are then no geometrical measurements whatsoever [. . .] Viewed from standpoint B, the choice of geometrical concepts and relations is, indeed, determined only on the grounds of simplicity and instrumental utility. Concerning the metrical determination of space, nothing can then be made out empirically [. . .] because, on this choice of a standpoint, geometry is not a *complete* physical conceptual system, but only a part of one such (Einstein, 1924b, 1690–1691; tr. Howard, 2009, 20).

The two points of view described in the passage clearly correspond to the unnamed ‘defender of practical geometry’ (standpoint A) and Poincaré (standpoint B) introduced in the 1921 lecture. In a brief paper published one year later, “Nichteuklidische Geometrie und Physik” (Einstein, 1925a), Einstein added another detail, expressly attributing the standpoint B to Hermann von Helmholtz:

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

As in the 1921 lecture on geometry and experience (using nearly the very same words) Einstein insists that the 'Helmholtzian' approach, in which the interval is regarded as measured by rods and clocks, made relativity theory physically possible. Relativity theory is nothing but a theory concerning the *Lagerungs-Gesetze*, the relative possible displacements of infinite many infinitesimal rigid rods (isolated from relative motions, temperature differences or other influences). However, the fiction of the existence of perfect rigid bodies that we had initially assumed as at least in principle possible might turned out to be untenable: relativity theory has already put restrictions to the use of rigid bodies of finite dimensions (see also Einstein, 1924c, 86) and the theory of elementary quanta might force us to abandon the notion of rigid body in atomic domain (Einstein, 1925a, 253): if for instance rigidity depends on the crystalline structure of the rod, we cannot proceed to indefinitely small rods without losing this structure.

Thus, Einstein considered the Helmholtz-like point of view as a necessary, but provisional stepping-stone; however, he was sensitive to the 'Poincarean' objection that by using rods and clocks we actually use certain physical processes, and thus we rely upon certain assumptions concerning the physical laws describing them. In principle 'geometry' can be confronted with reality only together with such laws. The claim that a certain geometry 'holds' in reality would depend on some internal characteristics of this whole system (coherence, simplicity, rigidity etc.; cf. e.g. Einstein, 1919b) and not on the direct correspondence of individual elements in the theory to physically existing measuring apparatuses.

The commitment to this double standard returns again in the entry "Space-time" for the 13th 1926 edition of the *Encyclopedia Britannica*. Here, Einstein introduces a similar opposition between the defender of practical geometry and the more sophisticated stance defender by unnamed "*consistent thinkers*" (Einstein, 1926, 609):

> A serious difficulty is encountered in the above represented interpretation of geometry in that *the rigid body of experience does not correspond exactly with the geometrical body.* In stating this I am thinking less of the fact that there are no absolutely definite marks than that temperature, pressure and other circumstances modify the laws relating to position. It is also to be recollected that the structural constituents of matter (such as atom and electron, q.v.) assumed by physics are not in principle commensurate with rigid bodies, but that nevertheless the concepts of geometry are applied to them and to their parts. For this reason *consistent thinkers have been disinclined to allow real contents of facts [reale Tatsachenbestände] to correspond to geometry alone. They considered it preferable to allow the content of experience [Erfahrungsbestände] to correspond to geometry and physics conjointly.* [...] Nevertheless, in the opinion of the author it would not be advisable to give up the first view, from which geometry derives its origin. This connection is essentially founded on the belief that the ideal rigid body is an abstraction that is well rooted in the laws of nature (Einstein, 1926, 609; my emphasis).

There is no need to emphasize that this passage rehearses Einstein's characteristic position between the two extremes of the epistemological spectrum.

It was legitimate (a) to provisionally resort to the point of view according to which "natural geometry refers to the directly observable specification of rods and clocks" (Pauli, 1926, 273) defended for instance by Pauli; and at the same time (b) to agree in principle with a more consistent position of thinkers like Weyl, who deny the possibility of an "experimental test of geometry" (Weyl, 1927, 96), insisting that individual laws of physics and those of geometry "can only be put to the test as a whole" (Weyl, 1927, 96).

5. 1930s-1950s. Einstein's last Reflections on Rods and Clocks

In the last twenty years of his life Einstein showed growing trust in the power of mathematical speculation. However, he did not change substantially his opinion about the role of rods and clocks in relativity theory. In particular one of the most often quoted passages from his "Autobiographical Notes" confessing his 'sin' towards his initial treatment of rods and clocks as unstructured entities, seems to address the very same epistemological concerns that have occupied him in the early day of general relativity.

5.1. The Einstein-Swann Correspondence

Cornelius Lanczos (who was Einstein's assistant during the period of 1928–29) in some popular writings (Lanczos, 1931; Lanczos, 1932) on Einstein's new theory based on distant-parallelism (Einstein, 1928a; Einstein, 1928b; Einstein, 1930d; cf. Sauer, 2006), described the physical community, after the triumph of quantum mechanics, as divided in two epistemological fronts: (1) the 'positivists', who reject the very use of differential-geometrical methods in physics, since they violate the observability criterion (even the ds^2 is strictly speaking unobservable since there are no infinitesimal small rods); (2) the 'metaphysicians', who are convinced that the deep structure of nature could be grasped only by means of speculative mathematical constructions (Lanczos, 1931, 104–105; cf. also Lanczos, 1932, 116). [25]

Several passages from different, mostly popular writings from the turn of thirties show traces of the fact Einstein was trying again to find a somehow precarious equilibrium between the 'positivistic' justification of relativity theory and the 'metaphysical' one, more in consonance with his works on unified field theory (Lanczos, 1931, 99).[26] On one hand, Einstein continued to warn his

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[25] The opposition is, of course, rough. Weyl, after the discovery of the 'matter fields' and possibility of deducing the existence of identical particles from their quantization (Weinberg, 1977, cf. Darrigol, 1986; Schweber, 1994, ch. 1), had become very critical of Einstein's "geometrical capers [Luftsprünge]" (Weyl, 1931, 56, cf. Scholz, 2006). He even conquered Pauli's hard-to-win admiration (Pauli to Weyl, 26-08-1929, Pauli, 1979, 518–519) for his application of his gauge invariance principle to the Pauli-Dirac theory of the spinning electron (Weyl, 1929a; Weyl, 1929b). However, Weyl did not embraced a positivistic epistemology (Weyl, 1932; Weyl, 1934a).

[26] In a letter to Moritz Schlick on 28111930, criticizing the positivism of the first Einstein wrote: "you will be surprised by Einstein the 'metaphysician'. But in this sense every four- and two-legged animal is, *de facto*, a metaphysician"

readers that one should not forget that the general relativistic field equations can be said to 'hold' in reality since they correctly predict the values of the $g_{\mu\nu}$ that in turn determine the observed behavior of rods and clocks (Einstein, 1928c, 164–165; Einstein, 1929b; Einstein, 1930a; Einstein, 1930c; Einstein, 1930b; Einstein, 1936, 356–357); on the other hand, parallely to his work on the unified field theory, he showed increasing confidence in the power of pure mathematical speculation, so that 'internal' criterions (such as simplicity) seems to furnish the only assurance of the correspondence of the abstract theory to reality: the field equations are 'correct' because they are the simplest generally covariant differential equations which the $g_{\mu\nu}$ can satisfy (Einstein, 1929a; Einstein, 1933, 15; Einstein, 1936, 369).

In this sense, in spite of Einstein's progressively stronger inclination to consider nature "actualized the ideal of mathematical simplicity" (Einstein, 1933, 15; cf. Norton, 2000; Dongen, 2010), he seems to have maintained his characteristic double-truth-doctrine concerning the role of rods and clocks in his theory. A particularly clear articulation of this stance is offered by Einstein's correspondence with the american physicists William Francis Gray Swann, which has recently drawn some attention (cf. Brown, 2005, 120, fn. 19; and in particular Hagar, 2008; cf. also Stachel, 1993). In a letter to *Nature*, written in September 1941 (but published only in December)—referring to the "recent discussion between Sir James Jeans and Sir Arthur Eddington" (Swann, 1941, 692)—Swann explained his non-standard view on length contractions as quantum mechanical phenomena that should be explained dynamically by a Lorentz-covariant theory of matter (Brown, 2005, 119–121)

In his reply Eddington repeated what he already explained at length in his Tarner lectures in 1938 (Eddington, 1939) and what we have tried to show in previous paragraphs. Relativity theory must refer to quantum mechanics but for a very opposite reason: "relativity theory has to go outside its own borders to obtain the *definition of length*, without which it cannot begin" (Eddington, 1939, 76; my emphasis). In oder to make the theory empirically meaningful we need to make statements about lengths in a remote star or at a remote epoch, implying that there are physical standards, which can be constructed there 'identically' to a corresponding terrestrial standard. This is possible only if the two standards are can be constructed from a common specification; for instance because they stay in definite numerical ratio withe some dimensionless pure number, such as h/mc, the 'wave-length' of the electron, which appears in the fundamental equations of Dirac's electron theory (cf. Eddington, 1941, 692–693), a constant which Eddington (cf. Eddington, 1933), differently from Weyl (cf. Weyl, 1934b), still hoped might be connected with the radius of curvature of the universe.

Swann remained unsatisfied with Eddington's reply and decided to write to Einstein himself in January 1942 (the episode is recounted in detail in Hagar, 2008). Einstein's response seems however to confirm Eddington's view.[27] In

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[27] The original German and a translation of the letter can be found in Hagar, 2008, form which all citations are taken.

special relativity ‘measuring rods and clocks (idealized, but in principle conceived as realizable)’ are treated as “independent physical objects” (Einstein to Swann, 24-01-1942, AEA, 20-624), which, linked as they are to the coordinates, will enter into the propositions of the theory. In this sense “Measuring rods and clocks are consciously *not treated as solutions under the basis of structural laws*” (Einstein to Swann, 24-01-1942, AEA, 20-624; my emphasis), that is of some fundamental Lorentz covariant laws of nature, such as Maxwell equations. Einstein emphasizes that “[t]his is well justified” (Einstein to Swann, 24-01-1942, AEA, 20-624), since experience shows, “the (in principle) existence of those objects that can serve as measure of coordinates appears better justified than any particular structural laws, e.g. Maxwell’s equations” (Einstein to Swann, 24-01-1942, AEA, 20-624). As we have seen, the widely empirically confirmed fact that identical atoms always emit the same spectral lines was sufficient to assure that they were reliable clocks, even if the very existence of stable of atoms was incomprehensible from the point of view of Maxwell’s electrodynamic.

There is, however, an alternative: “if one does not [*sic*] introduce rods and clocks as independent objects into the theory, one has to have a structural theory” (Einstein to Swann AEA, 20-624), a fundamental theory of nature. We need a theory in which “in which *a length* is fundamental [eine Länge fundamental eingeht]” (Einstein to Swann, 24-01-1942, AEA, 20-624; my emphasis). In other words we need a theory in which some dimensionless ‘pure number’ appears (just as Eddington pointed out). Such theory would lead “to the existence of solutions in which that length plays a determinant [constitutive] role [denen jene Länge bestimmend eingeht, sodass es nicht mehr eine kontinuierliche Folge ähnlicher Lösungen gibt], so that a continuous sequence of similar solutions no longer exists”. As many years before, in the letter to Dällenbach, Einstein still left open the possibility that quantum theory might play this role. However, he also insisted, that “[a]ny theory which has a universal length in its foundations” (Einstein to Swann, 24-01-1942, AEA, 20-624), and thus also a field-theory, would be able to produce “qualitatively distinct solutions of a certain extension” (Einstein to Swann, 24-01-1942, AEA, 20-624).

Only a fundamental length could explain the existence of discrete stable material structures in such a field theory (Stachel, 1986a). ‘universal length’ is then the key to understand the epistemological point of Einstein’s answer to Swann (a point that in my opinion has not been emphasized by the few authors mentioning the correspondence); it confirms that the problem at stake was again that of assuring the identity of the measuring units. Instead of postulating the existence of rod-like and clock-like localized and identical structures which can be used to reproduced such units, one could in principle derive their existence from a relativistic field theory of matter which would have only a discrete number of solutions.

Of course, this was only a program rather than a theory. The difficulty of accounting for the existence of matter in a field-theoretical framework had been addressed by Einstein in a brief note published in a rather obscure argentinian journal one year earlier (Einstein, 1941) and later in a paper written with Pauli (Einstein and Pauli, 1943). Under very general conditions, any attempt to base a unified theory on the Riemann tensor would necessarily involve singularities in particle-like solutions (Einstein and Bargmann, 1944; Einstein, 1944). This result represented, of course, a serious threat to Einstein's ambitions, given his deep conviction that a field theory "which claims to be complete (in contrast e.g. to the pure theory of gravitation)" (Einstein and Straus, 1946), should have rigorous solutions "which are regular in the entire space" (Einstein and Straus, 1946, 737).

5.2. Einstein's Epistemology in the Schilpp-Volume

The letter to Swann furnishes a further testimony of the fact that Einstein's epistemological attitude did not seem to have changed at all from the letter to Dällenbach from which we have started. The opposition between what one can be accepted *sub specie temporis*, as provisional compromise, and what should be maintained *sub specie aeterni*, in principle, returned again in a much more famous passage written in 1946, of the autobiographic notes for the Schillp's volume and quoted an infinite numbers of time:

> One is struck [by the fact] that the theory [of special relativity] [...] introduces *two kinds of physical things*, i.e., (1) measuring rods and clocks, (2) all other things, e.g., the electromagnetic field, the material point, etc. This, in a certain sense, inconsistent; strictly speaking measuring rods and clocks would have to be represented as *solutions of the basic equations (objects consisting of moving atomic configurations), not, as it were, as theoretically self-sufficient entities.* However, the procedure justifies itself because it was clear from the very beginning that *the postulates of the theory are not strong enough to deduce from them sufficiently complete equations.* If one did not wish to forego a *physical interpretation of the co-ordinates* in general (something which, in itself would be possible), it was better to permit such inconsistency, with the obligation, however, of *eliminating it at a later stage of the theory. But one must not legalize the mentioned sin* so far as to imagine that intervals are physical entities of a special type, intrinsically different from other physical variables ('reducing physics to geometry', etc.) (Einstein, 1949a, 59).

This passage is usually taken as evidence that Einstein himself wanted to explain rod contraction and clock dilation in a 'constructive' way. However, in my opinion, the opposition between constructive and principle theories is not the right framework to understand the passage, which actually does not differ significantly from the many others that we have quoted so far. Einstein assumed provisionally "[t]he existence (in principle) of (ideal, viz., perfect) measuring rods and clocks" (Einstein, 1949a, 59), which, if at rest in an inertial system, always measure the true proper time and proper distance, and thus furnish physical interpretation of the co-ordinates. This is the condition under which special relativity can acquire status of a physical theory, "which can be experimentally validated or disproved" (Einstein, 1949a, 57). However, relativity theory cannot account for the fortunate circumstance that we happen to live in a world in which such physical systems exist. It would be better if we possessed some special relativistic theory of matter powerful enough to account for the very existence of 'geometrical' objects (rods and clocks) just as of any other 'physical' object. Note that Einstein explicitly claims that this would not mean to 'geometrize' physics, but on the very contrary to eliminate the arbitrary difference between 'two types of *physical* things' the geometrical and non-geometrical ones (cf. Lehmkuhl, 2013).

Another beautiful passage referring to general relativity from Einstein's so called, 'Reply to criticisms' (Einstein, 1949b, 685), seems to confirm this reading:

> Everything finally depends upon the question: *Can a spectral line be considered as a measure of a 'proper time' ds (*Eigen-Zeit*) ds* ($ds^2 = g_{ik}dx_idx_k$*)* (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.* For the construction of the present theory of relativity the following is essential:
>
> (1) Physical things are described by continuous functions, field-variables of four co-ordinates. As long as the topological connection is preserved, these latter can be freely chosen.
> (2) The field-variables are tensor-components; among the tensors is a symmetrical tensor g_{ik} for the description of the gravitational field.
> (3) There are physical objects, which (in the macroscopic field) measure the invariant ds
>
> If (1) and (2) are accepted, (3) is *plausible, but not necessary.* The construction of mathematical theory rests exclusively upon (1) and (2). *For the objects used as tools for measurement do not lead an independent existence alongside of the objects implicated by the field-equations* (Einstein, 1949b, 685; my emphasis).

Einstein suggested again (just like in 1917) that he started with the provisional, but plausible assumption that in nature there are physical processes which can be used to measure the invariant ds, that is to 'norm' it as the unit interval, that is that rods and clocks which are not influenced by any gravitational field and always have the same relative length and period exist. However, as Einstein pointed out in a letter to the Australian medicine student Leonard Champion, this assumption "could be wrong even though the gravitational field equations are not [brauchen nicht falsch zu sein]" (AEA, 25-481).

In a complete theory of physics as a totality, which of course does not yet exist, the field-equations themselves would account for the existence of those physical systems that we use and the behavior of objects used as tools for measurement. The distinction between geometrical and non-geometrical object would disappear, together with the distinction between the geometrical and non-geometrical fields, that would be unified in unique total field: "I am convinced, however, that the distinction between geometrical and other kinds of fields is not logically founded" (Einstein to Lincoln Barnett, 19-06-1948, AEA, 6-58, cit. and tr. in Lehmkuhl, 2013, 10). The latter scenario would however imply some relevant epistemological consequences. "In a consistent field theory there is no real definition of the field", Einstein wrote to Besso in 1950, "*A priori* no bridge to the empirical is given", and the "a comparison with the empirically known can only be expected to come from finding exact solutions of the system in which empirically 'known' structures and their interactions are 'reflected'" (Einstein to Besso, 15-04-1950, in Speziali, 1972, 438–439, tr. Stachel, 1986a, 376).

Thus Einstein's plea for a dynamical explanation of rods and clocks should be understood on the background of a general philosophical question of how a theory is related to to the experimental devices, or as he put it more emphatically, "with Pilate's famous question: 'What is truth?'" (Einstein, 1949b, 676). It is not by chance that Einstein, discussing Hans Reichenbach's 'meaning = verifiability' criterion, found again helpful to describe a different possible stance toward the relations between theory and experience by resorting to an imaginary dialogue between 'Helmholtz-Reichenbach' and 'Poincaré'.

(1) 'Helmholtz' claims that "the empirically given solid body realizes the concept of 'distance'" (Einstein, 1949b, 676–677): geometry is verifiable, its propositions might be true or false. (2) 'Poincaré' objects that "the empirically given bodies are not rigid" (Einstein, 1949b, 677) and consequentially an not be used as "the embodiment of geometric intervals" (Einstein, 1949b, 677): geometry is not verifiable, its proposition are neither true nor false. (3) 'Helmholtz' replies that, one can realize the notion rigid body anyway, by taking "thermal volume-dependence, elasticity, electro- and magneto-striction, etc., into consideration" (Einstein, 1949b, 677). (4) However, in this way—'Poincaré' objects—one has to make use of physical laws that describe the behavior of the rods under this influence. Consequently, "not merely [. . .] geometry but [. . .] the entire system of physical laws" (Einstein, 1949b, 676–677) is to be compared with experience (cf. also Einstein, 1954b, 91).

Einstein seems to repeat here exactly his position of his 1921 lecture (and of several other minor texts that we have mentioned above); however he now more clearly separates the *pars destruens* of Poincaré's argument from the *pars construens*, which he famously attributes to an anonymous 'non-Positivist':

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

There has been a lot of speculation concerning the identity of the non-Positivist. But after all Einstein himself, or at least his later increasing faith in the power of mathematics, seems to fit description. He compared the work of the theorist—no matter "how pure a 'positivist' he may fancy himself"—to that of a "tamed metaphysicist" (Einstein, 1950, 13), who believes that "logically simple is also the real" (Einstein, 1950, 13). If we recall Lanczos' distinction between positivists and metaphysicians that we have mentioned above (see page 42), one can venture to conjecture that Einstein chose the expression non-positivist to avoid the more bold one 'metaphysicist' that he used in other occasions.

However the non-positivist might be, it is at least clear that the question at stake was again whether 'individual statements' of a theory can be directly compared with experience or whether only simplicity and rigidity of the 'theory as whole' are signs of its correspondence to nature (Einstein, 1950, 13). This is the conceptual framework against which the whole discussion between rods and clocks should be understood. In November 1952 (the episode is recounted in Howard, 1990, 13), Paul Oppenheim—a philosopher mostly famous for his work with Gustav Hempel—brought Einstein to visit Rudolf Carnap. According to Carnap recollections, when he remembered him Otto Neurath's famous ship-argument against the positivist, Einstein had a significant reaction: "there is no rock bottom, Neurath's reconstruction of the ship afloat. With that he emphatically agreed" (cit. and tr. in Howard, 1990, 13).

5.3. The Non-Positivist vs. the Positivist. Weyl vs. Pauli once again

In popular writings over the same decades (Weyl, 1932; Weyl, 1934a; Weyl, 1949a) Weyl had defended a very similar epistemological view, in spite of his skepticism toward the project of the unified field theory. In the "Appendix F" of the 1949 English-augmented translation (Weyl, 1949b) of his 1927 philosophical monograph (Weyl, 1927) Weyl articulated even more clearly his position. "The rigid rods and the clocks by which Einstein measures the fundamental quantity ds^2" (Weyl, 1949b, 288), he claims, are assumed to preserve their lengths and periods "in the last instance because charge e and mass m of the composing elementary particles are preserved" (Weyl, 1949b, 288). However, "[t]he systematic theory, however, proceeds in the opposite direction" (Weyl, 1949b, 288). It starts with the ds^2 defined implicitly by the theory and then introduces some a "primitive field quantity" (Weyl, 1949b, 288) to which the Compton wavelength h/mc of the particle stays always in constant proportion. The quantity h/mc furnishes an absolute length standard, which via the spectral lines of atoms, can be used to 'calibrate' the ds^2. In this way "[t]he behavior of rods and clocks comes out as a remote consequence of the fully developed theory" (Weyl, 1949b, 288).

As we have seen at the turn of the 1920s Weyl (and Eddington) had attempted to identify the field quantity in question with the radius of curvature of the world in his theory. Weyl (differently from Eddington) had completely abandoned this approach, forced by the discovery of fundamental length standard in quantum field theory (cf. Weyl, 1934b). However, as he points out, this "does not essentially alter the basic relationship just described" (Weyl, 1949b, 288). The question then and now was to explain "the most fundamental features in the nature of the universe" (Weyl, 1949b, 288; my emphasis), the composition of the material world out "*of one or a few units, existing in a huge number of completely alike* *specimens*" (Weyl, 1949b, 288; my emphasis), to explain how it happens that the "*same particle* with its definite charge and mass [...] occurs in the world in a *large number of copies*" (Weyl, 1949b, 288; my emphasis).

Classical physics could only deal 'preserving quantities', which are highly unstable. Their "initial value may be chosen arbitrarily" (Weyl, 1949b, 288), and since perturbation can never entirely be eliminated, deviations are apt to occur in the course of time. According to Weyl the only explanation of the existence of particles of definite charge and mass was to imagine that elementary particles behave like 'adjusting quantities', which are not arbitrary and not perturbable; there must be some mechanism that "enforces a definite value that is independent of past history and hence reasserts itself after any disturbances and any lapse of time as soon as the old conditions are restored" (Weyl, 1949b, 288). If Mie's program would have been realized, Weyl goes on, then adjustment would have been explained in the framework of classical field physics; the theory would possess only one or at most a small number of static spherically symmetric solutions of definite size. But Mie's program failed. With the development of relativistic quantum mechanics and quantum electrodynamics, the fields was subjected to a 'second quantization' (the classical field variables become quantum operators), "a process by which one passes indeed from one to an indeterminate number of equal particles" (Weyl, 1949b, 289). In this way, however, "equality is accounted for, yet the particular values of charge and mass remain as unexplained as before" (Weyl, 1949b, 289). To solve this problem one would have to derive from the theory that the fine structure constant has a value nearly equal to $1/137$, just like one can derive from Euclidean geometry the value of π.

Of course Weyl had no intention to defend a "theory in which" (Weyl, 1951, 81) he "no longer believe[s]" (Weyl, 1951, 81; cf. also the *addendum* to the reprint of Weyl, 1918b; published in Weyl, 1956), his 1918 unified field theory. However, he still argues that the theory had put general relativity in front of a legitimate epistemological question: "the definition" (Weyl, 1951, 81) of the metric field "with help of rods and clocks" can of course "only be regarded as a temporary connection to the experience" (Weyl, 1951, 81). The theory should be able to account for the very behavior of those material systems, rods and clocks, which we can use to calibrate the measuring units: "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).

It is interesting to notice that the roles in the debate had not changed significantly. In a note added to the 1958 english translation of his Encyclopedia-article (Pauli, 1958), Pauli insisted again that in the original text, in the above mentioned § 65 (see above section 3.4), he "was already at that time very doubtful regarding the possibility of explaining the atomism of matter, and particularly of electric charge, with the help of classical concepts of continuous fields alone" (Pauli, 1958, 255, n. 23). Already at that time he "felt rather strongly the fundamental character of the duality (or, as one says since 1927, complementarity) between *the measured field and the test body used as measuring instrument*" (Pauli, 1958, 255, n. 23; my emphasis). The somehow surprising reference to Bohr's notion of 'complementary' shows that Pauli consciously denied the very possibility of applying of a single conceptual model applicable to the theory. Nature can only be understood by the use of different, mutually exclusive models in different situations.

In an address written on the year of his death—in the occasion of what would have been Einstein's 80th birthday—Pauli seems to seems to attribute to this very problem the difficulties in which Einstein's attempt to solve the problem of matter in the context of classical field theory had encountered. In Einstein's theory "the *duality between the field and its means of measurement*, although latently present in today's quantum theory of fields,[28] is conceptually not clearly expressed" (Pauli, 1959, 245; my emphasis). Precisely for this reason, "[t]he relation of the applicability of the ordinary space-time concept in the small with the properties of the smallest physical objects, the so-called 'elementary particles' is not disclosed" (Pauli, 1959, 245). Einstein's project of a unified field theory was then considered physically dubious, because of the very issue that had bothered the nineteen-year-old Pauli (see above section 2.4). This question seems not to have lost its urgency when Pauli's life was drawn to a close: "Einstein's life", he wrote, "ended with a question [posed] to the science of physics and with a behest for a synthesis to us" (Pauli, 1959, 245).

6. Conclusion

The entire discussion about the role of rods and clocks in relativity theory seems to turn around a very simple issue. If the crucial discovery of relativity was that elapsed-time, just like travelled-distance, is route-dependent, the empirical significance of relativity theory relies on the fact that the units of length and time are not. Both special and general relativity presupposes that there are physical processes in nature that assure that measurements are performed everywhere, at the remotest times and places, in terms of the 'same' units. Following Born, we have tentatively called this assumption 'the principle of the physical identity of the units of measure'. The empirical significance of both relativity theories seems to depend entirely on the validity of such principle, and it was the problem of the validity of such principle that had forced Einstein to call into question the very relationship between a theory and the material devices that serve to its verification.

It is somehow fascinating to realize that by the time of the second flowering of general relativity in the sixties, this very same issues returned in the very same form, raising the very same epistemological concerns. In 1959 John Lighton Synge spoke out a 'plea for chronometry' (Synge, 1959), insisting that clocks

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[28] Bohr and Leon Rosenfeld (Bohr and Rosenfeld, 1933) had shown that the measurability of the components of the electromagnetic field by means of point charges "in the sense of electron theory" (Bohr and Rosenfeld, 1933, 358) acquires new complexities in quantum field theory. The measurement to the field values at points of space-time (which is not an operator in a Hilbert space) would require an infinite amount of energy.

can determine the metric better than measurements made by measuring rods—customarily employed also in recent textbooks (e.g. Møller, 1952)—. Space-like separations can be measured indirectly using radar methods (Bondi, 1964). In this way Synge hoped to "make general relativity more operational than it has been hitherto" (Synge, 1960, 105). Synge attributed to Einstein the claim that physical quantities are defined by the operations used to measure them (Synge, 1958, 7; Synge, 1960, 104–105). According to Synge this means that the 'mathematical observation' (MO) of time (time, standard clock, ticking of standard clock) acquires a physical meaning if can be connected with the 'natural observation' (NO) of time (time atom, emission of wave crests of radiation). Synge does not hide to his readers the difficulties raised by the equation $MO = NO$, but he clearly sufficiently confident that the equation is realized with sufficient accuracy in the case of time measurements. We have the ability to observe virtually standard clocks, since physical entities (such a vibrating atoms, etc) exist in nature in which the intrinsic forces far exceed any accelerating forces we can apply.

Which atoms should be chosen as standard clocks? "The answer is that it does not matter, provided we use consistently the same type of atom (*all atoms of the same type are regarded as identical*) and the same pair of energy levels" (Synge, 1960, 106). In fact, "the only effect of changing from one clock to another is to change the unit of time, the ratio of two units being a universal constant" (Synge, 1960, 107). Synge does not hide the concern that "in the present state of physics it is an impertinence to look too closely into the private life of an atom" (Synge, 1963, 34); however, what Synge called the 'chronometric hypothesis' could at least be regarded as reasonable: "if spectroscopy was a complete chaos (equivalently if spectroscopy did not exist), one would hesitate to speak of a standard clock. But spectroscopy reveals a remarkable order in what in the behaviour of what we may call 'atomic clocks'" (Synge, 1963, 34).

However, Synge's attitude was clearly felt as epistemologically unsatisfying by other relativists. Wolfgang Kundt and Dieter Hoffmann (Kundt and Hoffman, 1962), and Robert Marzke and John A. Wheeler (Marzke and Wheeler, 1964; relying on Marzke, 1959) insisted that it would be preferable to eliminate from the process of measurement of the gravitational field all measuring tools whose working mechanism is not described within general relativity. When we use the red-orange spectral line emitted by the krypton-86 isotope as standard of length (which in 1960 chosen to substitute the Paris platinum-iridium standard), this definition depends on the physical constants which determine the size of that atom, in particular to the fact that they are time-independent (strong equivalence principle). However, the ratio of physical constants, in particular the fine structure constant, may change with time, for instance as a consequence of the expansion of the universe: .

As Marzke and Wheeler pointed put—a space-time theory "in and by itself" should provide "its own means for defining intervals of space and time", without leaning "at all upon the atomic constitution of matter to define a standard of length" (Marzke and Wheeler, 1964, 62). Marzke's method for the measurements of the proper times between events in space-time involves the use of a so-called 'geodesic clock'. The latter consists of two mirrors traveling along parallel paths in space-time with a light beam reflecting back and forth between them. The number of scatterings yields a well-defined measurement of the space-time interval. Marzke and Wheeler hoped that this standard would be "able to supersede the krypton-86 standard meter as well as the platinum-iridium standard meter by a geometrodynamic standard meter" (Marzke and Wheeler, 1964, 62).

This hope was probably misplaced, at least for the fact hat the electromagnetic theory of light is scale invariant, so that geodesic clocks cannot set a global definition of a unit of time. However, the epistemological ideal that inspired such attempts is clear: As James Anderson, who extended explicitly this approach to special relativity (Anderson, 1964), put it, in general a well-behaved physical theory must satisfy a fundamental requirement: "*it must contain a description of all physical systems with which it purports to deal, including those systems employed in the measurement process*" (Anderson, 1967, 139; my emphasis). It is under the spell of this epistemological ideal that several attempts were made to determine the metric structure of space time, using less structures entities.

Mario Castagnino (Castagnino, 1971), and most of all Jürgen Ehlers, Felix Pirani and Alfred Schild (Ehlers, Pirani, and Schild, 1972; following the above mentioned Weyl, 1921b) could show that the metric could be derived using only light rays and free-falling particles. However, the full Riemannian structure of space-time could not be reconstructed in this way without postulating the identity of gravitational time and atomic time (Ehlers, 1973), and consequently breaking the consistency of the whole construction. It was considered philosophically more pleasant to eventually derive this postulate "from a theory that embraces both gravitational and atomic phenomena, rather than to postulate it as an axiom" (Ehlers, Pirani, and Schild, 1972, 65). Thus, the question arose whether for the Ehlers-Pirani-Schild formalism there is an alternative to the recourse to quantum theory in order to achieve its 'Riemannian' completion (Köhler, 1978; Chandra, 1984; Perlick, 1987), or whether it be accepted that quantum theoretic considerations cannot be avoided to close the gap to the usual pseudo-Riemannian geometry adopted in general relativity (e.g. Jürgen Audretsch, 1983; Jurgen Audretsch and Lammerzahl, 1991).

Relativists were again facing the very problems the pioneers of the theory were concerned with: the very same physical problem of establishing a global definition of the units of measure and the very same philosophical issue to establish to what extent this is possible without abandoning the framework of relativity theory. As James Anderson had put it with crystalline clarity concluding a paper written for John Stachel's 70th birthday, this is the fundamental epistemological alternative from which the whole debate about the role of rods and clocks in relativity theory should be understood:

> There are two positions one can take concerning the role of measuring devices in a physical theory. One is that such devices lie outside the province of the them and their properties must be postulated. Such a view is common in quantum mechanics, in which measuring devices are held to be classical devices, and leads to a number of problems associated with the so-called measurement problem. The other position holds that measuring

> devices are physical systems whose behavior must be describable within the framework of the theory of the systems they are designed to measure (Anderson, 2003, 280).

As we have seen, Einstein's waving between the treatment of clocks as unstructured or structured entities should be interpreted as the attempt of positioning himself between these alternatives, historically well represented by Pauli and Weyl.

Einstein's unease towards the 'sin' of treating rods and clocks as simple, unstructured entities rather than as complicated dynamical systems did not intend to address the question around which the contemporary debate is centered: to establish which is the 'cart' and which are the 'horses' between abstract geometrical structure space-time and the physical laws governing material space-time devices. It was rather an attempt to give a balanced answer to a more general philosophical question, which he did not hesitate jokingly to compare Pilate's question: 'what is the truth?'. More humbly, it was the question whether a theory should describe its own means of verification or whether this description should lie outside its domain. Einstein became aware of the fact that testing a theory is a complex process, which might involve preliminary stages that can serve to secure provisionally the empirical support of the theory, but might be even rejected at later stages of development of the theory. Thus only the entire process *as a whole* can be compared with experience in meaningful way. Writing to his lifelong friend Maurice Solovine in 1953, Einstein remarked that the reduction of geometry to behavior of rigid bodies is in principle untenable, since rigid bodies do not exist, but at the same time inevitable from a didactic or heuristic point of view: "Moral: unless one sins against logic one generally gets nowhere; or one cannot construct a house or a bridge without using a scaffold which is really not one of its basic parts" (Einstein to Solovine 28-05-1953, AEA, 21-300, tr. Einstein and Solovine, 1987, 141).

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Abbreviations

AEA	*The Albert Einstein Archives at The Hebrew University of Jerusalem* (n.d.).
CPAE	Albert Einstein (1987-). *The Collected Papers of Albert Einstein*. Ed. by Diana Kormos Buchwald. 13 vols. Princeton: Princeton University Press.

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