Length Matters (I)
The Einstein–Swann Correspondence and the Constructive Approach to the Special Theory of Relativity

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Abstract

In this paper and its sequel I discuss a rarely mentioned correspondence between Einstein and W.F.G. Swann on the constructive approach to the special theory of relativity, in which Einstein points out that the attempts to construct a dynamical explanation of relativistic kinematical effects require postulating a minimal length scale in the level of the dynamics. I use this correspondence to shed light on several issues under dispute in current philosophy of spacetime that were highlighted recently in Harvey Brown's monograph *Physical Relativity*, namely, Einstein's view on the distinction between principle and constructive theories, and the philosophical consequences of pursuing the constructive approach to the special theory of relativity.

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1 Introduction

Considerable attention has been drawn lately to the distinction, attributed to Einstein, between principle and constructive theories, and to the methodological importance it may have to the scientific practice. Viewed as part of the context of discovery, however, this distinction is rarely acknowledged as having any philosophical importance, with the exception of Howard (2004) who urges us to regard it as one of Einstein's most valuable contributions to 20th–Century philosophy of science. In this paper and its sequel I would like to demonstrate this philosophical significance, suggesting that while the principle–constructive distinction furnishes the physicist with an important methodological tool, it also carries a philosophical weight, to the extent that it serves as a demarcation mark in debates that may seem purely epistemological or metaphysical.

Admittedly, while Einstein was not the first to introduce the distinction between principle and constructive theories to theoretical physics, he definitely popularized it when reflecting along his career on the conception of STR. Expressing the novelty of the theory, Einstein ultimately chose the principle view over the constructive view, but his ambivalence with respect to this choice (and his misgivings about what he regarded as its unfortunate implications on the foundations of quantum mechanics) are well documented (Schilpp 1949, Janssen 2000, Brown 2005ab). What is also well documented is the attempt, made by his contemporaries Lorentz and FitzGerald, to think about the kinematical phenomena of electromagnetism in constructive dynamical terms (see, e.g., Janssen 1995). Other physicists who expressed, along with Einstein himself — as some believe, dissenting constructive views of STR are less known in this context. They include Weyl, Pauli, and Eddington in the 1920s, W.F.G. Swann in the 1930s and the 1940s, and L. Janossy, and J.S. Bell in the 1970s.

In a recent monograph entitled *Physical Relativity*, Harvey Brown (2005a) adds himself to this distinguished list of unconventional views on STR. His aim is to advocate what he calls "The Big Principle" of the constructive view: That the universal constraint on the dynamical laws that govern the nature of non–gravitational interactions, namely, their Lorentz–covariance, is the true lesson of STR. On this view, the explanatory arrow in STR between the structure of spacetime and the behavior of rods and clocks is reversed: If one could achieve a dynamical underpinning of this behavior with an ultimate Lorentz–covariant theory of matter, then the "mystery of mysteries", i.e., how material bodies such as rods and clocks supposed to know which inertial frame they are immersed in and hence to contract and dilate accordingly, will be dispelled, and Minkowski spacetime will regain its appropriate status as a "glorious non–entity" (Brown & Pooley 2004).

Brown's controversial view has stirred a lot of discussions in the philosophy of physics community. In these two papers I set to myself the modest goal of examining two presumably contentious issues within it. The first issue is purely historical, and concerns Einstein's view on the constructive approach to STR. Reading Brown (2005a, pp. 113–114, 2005b) one gets the impression that Einstein's ambivalence with respect to his choice in the principle view to STR warrants annexing him to the constructive camp. Here I shall bring ample textual evidence to the contrary, that also sheds new light on the way Einstein saw the dichotomy between principle and constructive theories. The second issue is philosophical in character and regards the ontological and epistemological commitments behind the constructive approach. It arises from a famous claim, made by J. S. Bell (1976/1987, p. 77), and repeated by Brown at the outset of his book (2005a, p. 2), that the constructive approach enforces no commitment to a notion of a preferred frame, i.e., that one can, in Bell's words, adapt the Lorentzian *pedagogy* without accepting the Lorentzian *philosophy*. As we shall see, the current state of affairs in theoretical physics may put some constraints on this neutrality.

In order to achieve this modest goal I shall take my cue from a rarely cited correspondence between Einstein and the physicist W.F.G. Swann, mentioned only briefly in Stachel (2002) and in Brown (2005a). In this correspondence Swann presents Einstein with his constructive approach to STR, wherein rods and clocks are not introduced as primitive building blocks, or as "independent objects", but are taken instead to be material bodies obeying the Lorentz–covariant laws of the quantum theory of matter. Einstein, in response, argues cryptically that any such constructive formulation of STR must, like the quantum theory, contain a fundamental measure of length. My main concern will be to examine how the postulation of a fundamental measure of length (which, according to Einstein, is an inevitable consequence of the constructive approach) bears on the two issues I have set forth to investigate here. To this end I shall offer several interpretations to Einstein's cryptic remark, couching them in historical and theoretical contexts.

The paper is organized as follows. Section (2) offers a rather long but necessary introduction to the distinction between principle and constructive theories, and presents one of the main areas of contention among Einstein's experts that surrounds it, namely, Einstein's ambivalence towards his choice in the principle view of STR. In section (3) I discuss the Einstein–Swann correspondence on the constructive approach to STR, and in sections (4) and (5) I set the record straight with respect to the putative historical consequences of this correspondence on the constructive programme. Turning to philosophy, in section (6) I mention briefly some aspects that arise in current research on quantum gravity that bear on the neutrality of the constructive approach to STR with respect to the question of a preferred frame. These aspects are discussed in more detail in a sequel to this paper (Hagar 2007). The conclusions are spelled out in section (7).

2 The principle–constructive distinction

2.1 Einstein

If 1905 was Einstein's first Annus Mirabilis, 1919 was no doubt his second. By that time he was already five years in Berlin as a salaried fellow of the Prussian Academy, the race for the correct form of the field equations of the General Theory of Relativity (GTR) was over, and his divorce from his first wife was approaching its final stage, allowing him to marry his cousin. Even more important, at that year it was announced in London that measurements of the bending of starlight grazing the Sun during a solar eclipse confirmed the prediction of GTR. The London *Times* ran the headline on November 7th, 1919: "Revolution in science – New theory of the Universe – Newtonian ideas overthrown", and, suddenly, Einstein had become an overnight sensation, the world's first and greatest scientific superstar. Shortly after, the newspaper approached him, requesting a synopsis for the layman of his theories of relativity. Einstein gave his consent, and three weeks later appeared what is considered the most lucid among the short popular presentations of the theories of relativity.

The gist behind his first celebrated contribution to the "secular press" was a distinction Einstein made between two types of scientific theories, namely, the "constructive" and the "principle", that would become crucial for the presentation of the theories of relativity — indeed of the whole of theoretical physics — to the general public in the years that followed, and more important to historians and philosophers of science, that would serve to delineate the two poles between which Einstein's scientific work oscillated. I bring it here in its full length:[1]

> We can distinguish various kinds of theories in physics. Most of them are constructive. They attempt to build up a picture of the more complex phenomena out of the materials of the relatively simple formal scheme from which they start out. Thus the kinetic theory of gases seeks to reduce mechanical, thermal and diffusional processes to the movement of molecules,

[1] I quote the second, third, and fourth paragraphs from (Einstein 1919/1982, pp. 227–232).

> i.e., to build them up of the hypothesis of molecular motion. When we say that we have succeeded in understanding a group of natural processes, we invariably mean that a constructive theory has been found which covers the processes in question.
>
> Along with this most important class of theories there exists a second which I will call 'principle theories'. These employ the analytic, not the synthetic method,. The elements which form their basis and starting point are not hypothetically construed but empirically discovered ones, general characteristics of natural processes, principles that give rise to mathematically formulated criteria which the separate processes or the theoretical representations of them have to satisfy. Thus the science of thermodynamics seeks by analytical means to deduce necessary conditions which separate events have to satisfy, from the universally experienced fact that perpetual motion is impossible.
>
> The advantages of the constructive theory are completeness, adaptability and clearness. Those of the principle theory are logical perfection and security of the foundations. The theory of relativity belongs to the latter class...

What does this distinction between principle and constructive theories amount to? According to Einstein, in a principle theory such as thermodynamics (TD) one starts from empirically observed general properties of phenomena, e.g., the non–existence of perpetual motion machines, in order to infer general applicable results without making any assumptions on hypothetical constituents of the system at hand. Another example of a principle theory in which one employs "the analytic, not the synthetic method" is STR. Its building blocks — that velocity does not matter and that there is no overtaking of light by light in empty space — are 'not hypothetically constructed but empirically discovered'. Statistical mechanics (SM) and its predecessor the kinetic theory of gases, on the other hand, are constructive theories. They begin, says Einstein, with certain hypothetical elements and use these as building blocks in an attempt to construct models of more complex processes. Although ultimate understanding requires a constructive theory, admits Einstein in 1919, often progress in theorizing is impeded by premature attempts at developing constructive theories in the absence of sufficient constraints by means of which to narrow the range of possible constructions. It is the function of principle theories to provide such constraint, and progress is often best achieved by focusing first on the establishment of such principles.

It is hard to overestimate the importance Einstein's experts attribute to the three short paragraphs quoted above. Emphasizing Einstein's famous appreciation of the wide applicability of TD, Martin Klein (1967) sees the distinction between principle and constructive theories as yet another indication of the inspirational power TD had on Einstein's thought, especially in conceiving STR. Michel Janssen (2000) believes that the distinction epitomizes Einstein's ambivalence towards physics–theorizing in general. "Einstein resorted to the 'principle' type of theory", says Janssen, "when he did not have a strong vision of what a satisfactory [constructive – A.H.] model might look like". "Since he saw this type of theorizing essentially as a physics of desperation", concludes Janssen, "his methodological pronouncements later in life promote the 'constructive' approach, which had never gotten him anywhere, rather than the 'principle' approach that had led to all his great successes". Don Howard sees Einstein's distinction as his most original contribution to 20th–Century philosophy of science. "While the distinction first made its way into print in 1919", says Howard (2004), "there is considerable evidence that it played an explicit role in Einstein's thinking much earlier".[2]

Howard argues that in Einstein's hands the distinction between principle and constructive theories became a methodological tool of impressive scope and fertility. This point, while not appreciated as it should be in the philosophy of science community, is nevertheless unproblematic. Einstein's attitude towards to the constructive approach to theoretical physics in general and to STR in particular, is, on the contrary, still under dispute. In order to gain further understanding on the issues at stake, it behooves us to examine the anticipations of this distinction in the 19th–Century literature, James Clerk Maxwell and Henri Poincaré, in particular, being a source from which Einstein might well have drawn while not explicitly acknowledging his debt.

2.2 Maxwell and Poincaré

In the famous opening sentence of his Herbert Spencer lecture *On the method of theoretical physics* delivered in Oxford, Einstein (1982, p. 270) warned his audience not to listen to the words of theoretical physicists, but to fix attention on their deeds instead. Following this advise, one can locate the origins of the distinction between principle and constructive theories in the work of James Clerk Maxwell,

[2]Here Howard refers to Einstein's remark on Boltzmann's entropy principle, $S = k \log W$, which served as the constraint that suggested his own light quanta hypothesis: "Boltzmann's magnificent idea is of significance for theoretical physics ... because it provides a heuristic principle whose range extends beyond the domain of validity of molecular mechanics". (Einstein 1915, p. 262). See also Einstein's letters to Ehernfest and Sommerfeld from 1907 and 1908, respectively, mentioned in (Brown 2005b, p. S85).

who had acknowledged it, at least implicitly in his deeds, while, for example, laying the first foundations for electromagnetism and the kinetic theory of gases in the second half of the 19th–Century. Struggling to construct dynamical models for the ether or for Clausius' and Thompson's newly born theory of heat, Maxwell shifted along his career between the constructive and the principle perspectives. Similarly to Einstein, Maxwell was ambivalent with respect to the constructive approach. On one hand he insisted that:

> When a physical phenomenon can be completely described as a change in the configuration and motion of a material system, the dynamical explanation of that phenomenon is said to be complete. We cannot conceive any further explanation to be either necessary, desirable, or possible ... (Maxwell 1875, in Niven 1890, vol. 2, p. 418)

On the other hand, while working on a mechanical construction of an ether model, he observed that there could be, in principle, an infinite number of possible constructive models which might be proposed to represent the electromagnetic field:

> The problem of determining the mechanism required to establish a given species of connexion between the motion of the parts of the system always admits of an infinite number of solutions... (Maxwell 1873, in Niven 1890, vol. 2, p. 416)

Maxwell's conclusion was that constructive explanations such as his own ether model, described in his paper *On Physical Lines of Force* (1861), could be seen as no more than illustrative, demonstrating the possibility of providing a mechanical explanation to complex phenomena, in this case, electromagnetism (Maxwell 1873, in Niven 1890, vol. 2, p. 417). Referring to this ambivalence when celebrating Maxwell's centennial, Einstein (1931/1982, p. 268), who reviews Maxwell's influence on the evolution of the idea of physical reality, writes:

> Maxwell did, indeed, try to explain, or justify, these equations [Maxwell's equations of the electric and the magnetic fields A.H.] by the intellectual construction of a mechanical model. But he made use of several such constructions at the same time and took none of them really seriously...

The ether model was not the only context where Maxwell "did not take his dynamical constructions seriously". While working on the kinetic theory of gases, Maxwell was ambiguous with respect to the origin of the statistical assumptions needed for the reproduction of thermodynamical description from molecular motion.[3] His admission that constructive models, although serving as possibility

[3]Even today these assumptions still lack mechanical or dynamical justifications (Hagar 2005).

proofs or even as provisional hypotheses, are nevertheless "rough and clumsy compared with the realties of nature" (Maxwell 1873 in Niven 1890, vol. 2, p. 306) was partly motivated by this ambiguity. Although he did not put matters in such general terms as Einstein did, Maxwell was also aware of the tension between his constructive attempts and the principles he was aiming to construct. His famous demon, originally conceived to 'pick a hole' in the second law of TD (Maxwell 1867 in Harman 1995, p. 330), and his mocking of Clausius and Boltzmann who were engaged at that time in sterile attempts to derive this principle purely from the laws of mechanics (Maxwell 1873 in Harman 1995, p. 947) both signify this awareness.

One may even speculate that Einstein's witty remark on the discrepancy between the theoretical physicist's words and deeds might have been motivated by Maxwell's biography. Already at the outset of his career Maxwell suggested that nature may not be analogous to a 'book', envisaged as an ordered unity, but that the appropriate metaphor is a 'magazine', implying a collection of discontinuous parts and a disparity in theorizing:

> Perhaps the 'book', as it has been called, of nature is paged . . . but if it is not a 'book' at all, but a magazine, nothing is more foolish to suppose that one part can throw light on another. (Maxwell 1856 in Harman 1990, p. 382)

Yet, in clear contrast to this statement, he persistently occupied himself with constructing dynamical models for the purpose of unifying various natural phenomena.

Two decades later, the distinction between principle and constructive theories, implicit in Maxwell's work on electromagnetism and on the kinetic theory of gases, would become explicit in the writings of Henri Poincaré, who, as the most valuable link in the chain of "the physics of principles" — a chain that constitutes of Lagrange, Poisson, Fourier, and Cauchy (in France), and of Hamilton, and even Ramsey (in England) — was perhaps its most eloquent proponent (Giedymin 1983, Ch. 2).

According to Poincaré (1905), Physicists such as Laplace — whose celestial mechanics typified what Poincaré called "the physics of central forces" — believe that the aim of physics is to penetrate the mystery of the universe and to provide theories which not only predict observable effects but also postulate some hidden mechanism behind the phenomena. In contrast, physicists such as Lagrange and of Hamilton engage in the conception of mathematical theories based on very general assumptions such as the principle of conservation of energy, of entropy, of least action, etc., in other words on assumptions which yield desired observable predictions without making any explicit reference to hidden mechanisms and yet are reconcilable with many, often mutually incompatible, constructive explanations.

Interestingly, Poincaré had distinguished between what he called "the physics of central forces" and "the physics of principles" earlier, in his 1888–9 lectures on electricity and optics (Giedymin 1983, p. 79) and the context there was no other than the one Einstein mentioned in his commemorative paper, namely, Maxwell's failure to supply a satisfactory mechanical model for electromagnetic phenomena. Maxwell's solution was, recall, to retreat to a different procedure, namely, to show that such mechanical model could exist if and only if the principle of the conservation of energy and the principle of least action hold. Since if there exists one such mechanical model, then there are infinitely many — either postulating discrete unobservable objects or an unobservable continuous medium such as the ether — the choice of one of these models cannot, under these circumstances, be made on experimental grounds. Maxwell, applauded Poincaré, rightly abstained from making a choice.

Maxwell and Poincaré have thus entertained the distinction between theories of principles and constructive models long before Einstein, and both, along with others (e.g., Larmor), emphasized its methodological virtues. Principles serve as guidance for the theoretical physicist in the quest for the construction of dynamical models. In most physical problems, for example, the physicist first determines some form of an energy–function which would describe the recognized dynamical properties of the system and could be tested in further applications, and only then constructs mechanical models that possess such a function.[4] Formalized in terms of principles, the problem is thereby effectively reduced to the dynamical type and what remains are interpretations, explanations and analogies.

Another virtue, popularized by Poincaré conventionalist philosophy, is that when theories are formalized in terms of principles one can raise oneself above the theoretical dispute or rivalries which defy experimental solutions (e.g., between the corpuscular and the wave theories of light) in order either to continue research within a common ground or else to allow free choice of either theoretical frameworks.[5] These pragmatic considerations were strengthened by Poincaré's

[4]Larmor (1900, pp. 213–214) emphasizes a method — already well-established in his time — of enunciating any dynamical problem as a variational one: "... [I]n order to help in the reduction to dynamics of physical theories in which the intimate dynamical machinery is more or less hidden from direct inspection".

[5]It is natural to conjecture that the phrase "to elevate an empirical law to the status of principle" which is common in secondary literature on relativity theory originated from this context.

conviction that there are definite limits to our theoretical knowledge, limits which consist of observational equivalence and formal similarities between rival theories.

The ambivalence of the 19th–Century mathematical physicists with respect to the constructive approach to theoretical physics appears to be well motivated. Philosophers of science may easily locate its reasons in the underdeterminacy of constructive mechanisms by the phenomena (Howard 1998, pp. 154–159). It is thus understandable why mathematicians such as Lagrange and Hamilton, and even Poincaré himself, who were engaged in developing the physics of principles, regarded themselves as replacing mechanistic models. Those who opposed mechanistic philosophy at that time, e.g., Rankine, Otswald, or Duhem, and who were trying to eliminate from science notions such as "atoms" or "ether" saw them as allies, but unlike the energeticists, the motivation of the 19th–Century mathematical physicists who were constructing the physics of principles was not instrumentalist or positivist.

To be sure, they were aiming at a *replacement* programme, but as Giedymin (1983, pp. 80–81) notes, there are at least three reasons why they didn't see themselves as allies to the anti–mechanistic camp: First, the alternative they were following was not based on phenomenological, low–level generalizations, but on free use of sophisticated mathematics which had a wide, almost unlimited, range of applications. Second, their view had little in common with the instrumentalist view of theories as characterized, for example, in Nagel (1961, Chap. 6) — a view that identifies the class of existing objects with the class of observable objects, and that uses a criterion for meaning according to which meaningful terms refer to observable objects. Third, the aim of their replacement programme was not to eliminate unobserved theoretical entities, but rather to provide theories which — while observationally adequate (at least at that time) — would be elevated, by the use of abstract mathematical symbols, above contemporary ontological disputes, so one can further elaborate them no matter which side in the dispute (if any) one favored, and no matter which side (if any) will eventually win the day.[6] In other words, the best way to view the physics of principles, according to the 19th–Century mathematicians, is to regard it as remaining true whatever may be the details of the hidden mechanisms, that is, as *agnostic* with respect to ontological questions. Indeed, Poincaré's discussion on the essential role of equations in a scientific theory in the context of the transition from Fresnel's optics to Maxwell's

[6]Describing the law of conservation of energy, Poincaré (1952, p. 166), for example, claims that since one cannot give a general definition of, or a unique interpretation to, the term *energy* since "...the principle of conservation of energy simply signifies that there is something which remains constant".

electromagnetism can be seen as supporting such a view:[7]

> . . . They [the equations — AH] teach us now, as they did then, that there is such and such a relation between this thing and that; only the something we then called motion, we now call electric current. But these are merely names of the images we substituted for real objects which Nature will hide forever from our eyes. . . (Poincaré 1952, p. 161)

As mentioned above, some regard this type of agnostic physics–theorizing as "desperate" (Janssen 2000). However, thanks to Minkowski, and being a spece-time theory as it is, at least in the case of STR, the physics of principles can be augmented with a an explanation which, while not committed to any mechanism whatsoever, is nevertheless *not* agnostic with respect to the geometrical structure of spacetime. That *this* was Einstein's view, at least in his correspondence with Swann, is one of the claims of this paper, and shall be argued at length in section (5). Prior to doing so, let's end this introductory part with what Brown (2005a) regards as the alternative to the principle view of STR.

2.3 The constructive approach to STR

Several authors (Bell 1992, Brown 2003) have suggested that the dawn of the constructive approach to STR can be traced back to a letter, written in 1889 by G.F. FitzGerald, Professor of Natural and Experimental Philosophy at Trinity College Dublin to the remarkable English auto-deduct, Oliver Heaveside, concerning a result the latter had just obtained in the field of Maxwellian electrodynamics. Some months later FitzGerald exploited the idea he had expressed in that letter, namely, that the distortions suffered by an electric field that surrounds a charged particle traveling through the ether may be applied to a theory of inter–molecular forces, to explain the baffling null result of the Michelson–Morley experiment. In this note appears for the first time a distinct precursor of the FitzGerald–Lorentz contraction — a cornerstone in the kinematic component of STR.

As Brown (2005a, p. 2) puts it, following Einstein's brilliant 1905 work on the electrodynamics of moving bodies, and its geometrization by Minkowski (which proved to be so important to the development of the general theory of relativity),

[7]Note the similarity that this view bears to the contemporary view of structural realism as advocated, e.g., in (Worrall 1989). In future work I shall discuss interesting connections between the physics of principles of the 19th–Century and this modern attempt to solve the realism debate in the philosophy of science.

"it became standard to view the FitzGerald–Lorentz [contraction – AH] hypothesis as the right idea based on the wrong reasoning". Brown strongly doubts that this standard view is correct, and in his monograph, *Physical Relativity* (Brown 2005a) he joins other physicists who expressed, along with Einstein himself — or so Brown claims, dissenting constructive views of STR.[8]

Brown's aim is to advocate what he calls "The Big Principle" of the constructive view: That the universal constraint on the dynamical laws that govern the nature of non–gravitational interactions, namely, their Lorentz–covariance, is the true lesson of STR. On this view, the explanatory arrow in STR between the structure of spacetime and the behavior of rods and clocks is reversed: If one could achieve a dynamical underpinning of this behavior with an ultimate Lorentz–covariant theory of matter, the "mystery of mysteries", i.e., how material bodies such as rods and clocks supposed to know which inertial frame they are immersed in and hence to contract and dilate accordingly, will be dispelled, and Minkowski spacetime will regain its appropriate status as a "glorious non–entity" (Brown & Pooley 2004).

2.3.1 Bell's thread

What is the main epistemological difference between the two approaches? Taking our cue from Brown, who emphasizes that one of his motivations for adapting the constructive approach is the reversal of the explanatory arrow in STR from geometry to dynamics, it is instructive to confront the principle and the constructive views in the context of Bell's thought experiment that appears in the opening paragraphs of his famous *Lorentzian Pedagogy* paper (Bell 1976, pp. 67–68).[9]

Bell considers the following situation (see figure 1): Three small spaceships, A, B, and C, drift freely in a region of space remote from other matter, without rotation and without relative motion, with B and C equidistant from A. On reception of a signal from A the motors of B and C are ignited and they accelerate gently. Let B and C be identical, and have identical acceleration programmes. Then (as reckoned by the observer A) they will have at every moment the same velocity and so remain displaced one from the other by a fixed distance.

Now suppose that one end of a taut thread is attached to the back of B and the

[8] These include Weyl, Pauli, and Eddington in the 1920s, W.F.G. Swann in the 1930s and the 1940s, and L. Janossy, and J.S. Bell in the 1970s.

[9] Bell mentions that credit to this thought experiment is due to Dewan and Beran (1959). The influential role of this paper on the development of Brown's view is evident and explicitly acknowledged in Brown & Pooley (2001) and Brown (2005a).

other end to the front of C, assuming that the thread doesn't affect the motion of the spaceships. According to STR, the thread must Lorentz–contract with respect to A because it has relative velocity with respect to A. However, since the spaceships maintain a constant distance apart with respect to A, the thread (which we have assumed to be taut from the start) cannot contract; therefore a stress must form until for high enough velocities the thread finally reaches its elastic limit and breaks.

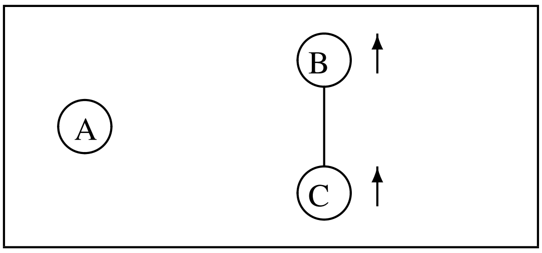

Figure 1

Bell (1976, p. 68) mentions that the knee–jerk reaction of one of his colleagues as well as the consensus in CERN's theory division was that the thread will *not* break. Further reflection, however, reveals that despite the views that deprive Lorentz–contraction of any reality STR predicts that (1) the thread *will* break, and (2) *all* the observers in this set–up, namely, A, B, and C, will agree on (1). Setting aside the interesting sociological issues that the reactions to this thought experiment reveal, let us examine the two possible *explanations* one can give for facts (1) and (2).

The standard explanation to which the principle view of STR subscribes hinges on two basic postulates, namely, the relativity of simultaneity and the principle of point–coincidence (i.e., that all observers agree on whether two events happen at the same point in space *and* in time, see, e.g., Mermin 1968, p. 31). On this view, the two postulates can be regarded as explaining the breaking of the rope since B, for example, sees C drifting further and further behind (and, conversely, C sees B drifting further and further ahead) so that the given piece of thread can no longer span this distance.[10] Yet the two postulates are nothing but the observation that the geometrical structure of spacetime is Minkowskian, i.e., that the

[10]One should imagine two clocks that are situated on the two points where the thread is hooked to B and C. If these clocks are synchronized when B and C are at rest relative to A, then when B and C start moving with equal constant velocity v relative to A in the direction that is depicted in figure 1, the clock at C will be *ahead* of the clock in B by lv/C^2 where l is the distance between B and C.

absolute, invariant distance measure in spacetime between two events is the four dimensional Minkowski metric, and that our spatial and temporal measurements of these events are nothing but covariant projections of this invariant measure on particular Lorentz frames. In sum, in order to explain the phenomenon of the breaking of the thread, the principle view simply cites the geometrical structure of spacetime, and nothing more.

How would the constructive approach explain the phenomenon of the breaking of the thread? One can look at things from A's point of view and regard the spaceships and the thread as material objects, obeying dynamical laws. Once set in motion relative to A, the material of the spaceships, and of the thread, will Lorentz–contract: A sufficiently strong thread would pull the spaceships together and impose FitzGerald contraction on the combined system. But if the spaceships are too massive to be appreciably accelerated by the fragile thread, the latter has to break when the velocities become sufficiently great.

The epistemological difference in the explanations adapted by the principle and constructive views is evident. The first explanation is geometrical, structural, and non–causal; the second is dynamical and causal. Brown, apparently, believes that the first is mysterious:

> What is required if the so–called spacetime interpretation is to win over this dynamical approach is that it offers a genuine explanation of universal Lorentz covariance. This is what is being disputed. Talk of Lorentz covariance 'reflecting the structure of spacetime posited by the theory' and of 'tracing the invariance to a common origin' needs to be fleshed out if we are to be given a genuine explanation here, something akin to the explanation of inertia in general relativity. Otherwise we simply have yet another analogue of Moliŕe's dormative virtue.. . . (Brown 2005a, p. 143)

> From our perspective, of course, the direction of the explanation goes the other way around. It is the Lorentz covariance of the laws that underwrites the fact that the geometry of spacetime is Minkowskian. (Brown & Pooley 2004, p. 84)

Brown's complaint, in effect, is that Minkowski spacetime fails to supply a *constructive* (i.e., causal–dynamical) explanation to relativistic effects such as breaking of the thread.

Now, this claim is true, but the attentive reader can appreciate that it is also vacuously so: After all, nowhere in the principle view one can find the claim that Minkowski spacetime *causes* the thread to break. Recall Poincaré's lesson: The principle view is *not* engaged in postulating some hidden mechanism behind observed phenomena. Rather, the explanation of the breaking of the thread in Bell's thought experiment that this view subscribes to is very similar to the geometrical explanation that appears, e.g., in (Putnam 1975, pp. 295–296), where, if one wants to explain why a round peg cannot fit through a square hole, one points to geometric features of the peg and the board rather than solving the equations for the detailed motion of all the atoms in the peg and the board.

Brown seems to believe that the constructive view is explanatorily superior to the principle view, but it is only by restricting explanations to *causal–dynamical* explanations that one can claim explanatory superiority of the constructive view over the principle view. Unfortunately, it is not clear that such a restriction could be justified independently of one's support in the constructive view, and, moreover, it is not clear that, if one restricts oneself to the relevant level of description, the constructive explanation is really an explanation. One is reminded of Putnam's famous dictum: The explanation of an explanation is not an explanation since even if one could actually write down those solutions to the equations for the detailed motion of all the atoms in the peg and the board, such a "micro–explanation" would include a lot of details that are irrelevant to the question, and it would fail to provide any meaningful kind of understanding. Indeed, returning, again, to Maxwell and Poincaré, it is more appropriate to view constructive explanations as consistency proofs, mere demonstrations that casual–dynamical models are *possible* for the phenomenon at hand.[11]

Be that as it may, rather than delving into a debate on the alleged superiority of one explanatory view over the other,[12] I shall set this matter aside and turn to examining the two issues I have set forth to investigate in this paper. The first issue is historical in character and concerns the question of Einstein's view on the constructive approach to STR. Brown believes that Einstein's ambivalence with respect to his choice in the principle view warrants annexing him to the constructive camp (Brown 2005a, pp. 13–14, 2005b). In the following two sections I shall offer some reasons to doubt this belief. My examination of the second issue,

[11]This view of constructive explanations as consistency proofs is even advocated by W.F.G. Swann (1940, p. 276), another proponent of the constructive approach to whom the following sections are devoted. Swann, in his inimitable style, compares the relation between constructive explanations and their respective phenomenological principles to the logical justification that "our old grandmothers remedies to our aliments as children" receive in terms of the fundamentals of bacteriology, physiology and chemistry.

[12]Indeed, such a debate threatens to deteriorate since, paraphrasing the debate between Brown & Pooley (2004) and Balashov & Janssen (2003), one's man horse is the another's cart. I attend to this issue elsewhere (Hagar, in preparation).

although it stems from the same historical context of the former, is more philosophical. It has to do with a claim made by John S. Bell, restated in (Brown & Pooley 2001) and (Brown 2005a, p. 2 & pp. 5–7), that one can adapt the constructive approach to STR — the so called Lorentzian pedagogy — *without* committing to Lorentzian philosophy. This philosophy, in Bell's words, is the one according to which "there is indeed a state of *real* rest, defined by the aether, even though the laws of physics conspire to prevent us identifying it experimentally" (1976, p. 77). As we shall see, in light of the current state of affairs in theoretical physics, as a matter of fact such a neutrality might be difficult to maintain.

3 The Swann–Einstein correspondence

In this section I shall discuss a historical anecdote, mentioned briefly in Brown (2005a, pp. 119–120), that regards a short correspondence Einstein had in early 1942 with the physicist W.F.G. Swann concerning the constructive approach to STR. Apart from drawing some interesting conclusions on Brown's own project from this anecdote, I use this presentation to set the historical record straight with respect to the correspondence itself since it appears to have been misinterpreted not only by Brown, but also by Stachel, whom he cites.

3.1 Background

W. F. G. Swann (1884–1962) was born in England, he was educated at Brighton Technical College, the Royal College of Science, University College, Kings College and the City Guilds of London Institute. Swann came to the US in 1913 as head of the Physical Division of the Department of Terrestrial Magnetism at the Carnegie Institute in Washington. Later he was Professor of Physics at the University of Minnesota, the University of Chicago and Yale, where he became Director of the Sloane Laboratory. He was appointed the Director of the Bartol Research Foundation in 1927.[13]

[13] A man of many talents, W.F.G. Swann was an accomplished cellist, founder of the Swarthmore Symphony Orchestra, a former assistant conductor of the Main Line Orchestra and former director of the Philadelphia Academy of Music. By the time of his appointment in Bartol, Swann had already distinguished himself as an excellent teacher, an outstanding researcher, and an emerging leader of the scientific community. Although Swann is perhaps best known for his experimental and theoretical efforts in the area of cosmic ray physics, his research interests touched on many other disciplines such as condensed matter physics, relativity, and charged particle acceleration. In his capacity as a professor he is perhaps best known as the advisor of E. O. Lawrence who sub-

Swann advocates his constructive view on STR as early as 1912 in two papers with lengthy titles he publishes in the *Philosophical Magazine* (Swann 1912ab). He returns to this view years later in a series of papers published during the 1930s and 1940s in *Reviews of Modern Physics* (and in which he cites no one else but himself). It is this view which he also repeats in his correspondence with Einstein in 1942 and in his letter to Eddington that preceded it.

3.1.1 Swann's constructive approach to STR

Here is Swann in a representative quote from (Swann 1930, pp. 261–263):

> If we strip from the theory [STR — AH] all the concepts incidental to its historical development; the fundamental outstanding dogma which remains is that the laws of nature — the differential equations — shall remain invariant under the transformation (1)–(4) [the Lorentz transformations — AH]. *Once we have written down some proposed laws*, the test of this conclusion is a matter of pen and paper, and not of experiment. ... I wish simply to state that insofar as its working aspects are concerned the theory of relativity is only of value provided that it can successfully demand invariance of laws when expressed in terms of measurements as we make them. ... On the other hand, starting with laws expressed initially in some definite set of coordinates, it is to be distinctly understood that mathematical invariance of those laws under the Lorentzian transformation by no means implies that the coordinates for the transformed system would have any relation to actual measures of the observer traveling with a velocity v associated with transformation to that system. It is true that the principle of the restricted relativity owed its formulation to a belief that the coordinates associated with the various systems corresponded to the actual measures; but, once formulated, the working content of the theory, involving as it does the mathematical invariance of the laws, is independent of this hypothesis.

Up to 1941 Swann's idea that the fundamental tenet of STR is the Lorentz covariance of the laws of physics was exemplified solely with electrodynamics. In 1941 he augmented his view with the claim that "relativity itself would provide no explanation of the [Lorentz–FitzGerald — AH] contraction were the story not

sequently was awarded the Nobel Prize for developing the cyclotron. Lawrence followed Swann from Minnesota, to Chicago, and then to Yale where he received his Ph.D. Altogether Swann had over 250 publications including a popular science book *The Architecture of the Universe*. In 1967 the International Astronomical Union honored Swann when it gave his name to a crater on the lunar surface at 52 north latitude and 112 east longitude.

capable of amplification by additional arguments based fundamentally upon the existence in nature of some such theory as the quantum theory" (Swann 1941b, p. 197). In a footnote to this statement Swann mentions that already in his 1912 papers he had made the case for this claim when showing that the mere invariance of the electromagnetic equations, with the electrons regarded as singularities in them, was insufficient to explain the Lorentz contraction (Swann 1912b, pp. 93–94). A relativistically invariant force equation was necessary in addition to the invariance of the field equation, but as time progressed it became more and more evident that "no obvious force equation following the lines of classical electrodynamics could be expected to provide the story of atomic and intermolecular forces in such a manner as to determine, ultimately, the form and equilibrium of a material body" (Swann 1941b, p. 197, fn. 4).

Swann's intuition for the quantum nature of the cohesive forces in matter is mentioned already in (Swann 1941a), and, as we have seen, the motivation for it already appears in 1912, but this intuition is spelled out in full for the first time only in (Swann 1941b, p. 201), where he discusses the physical changes that a material rod will suffer when set in motion (I extensively quote Swann here since his correspondence with Einstein relies heavily on this passage):

> When I start the rod in motion, all sorts of acoustical vibrations are set up. Of course, these will die down in time, but while I might be, perhaps unjustifiably, content if they should die down so as to leave the rod at its original length as measured in S [when the rod is at rest — AH], I am at loss to know how the rod decides that it must settle down to a new length determined by the FitzGerald–Lorentz contraction. ... It seems that quantum theory, if relativistically invariant in from, possesses the power to give the necessary answer. Consider the rod before the motion was imparted to it. What determines its form and stability? According to the quantum theory, these are determined by its being in a "ground state". Now the discussion given above ... tells us that if the equations are invariant and we have in S, one solution for, let us say, the ψ function, satisfying the usual conditions of continuity, etc., then associated with this solution we have an infinite number of other solutions obtainable from it by a Lorentz transformation, and *these are all possible states in the system* S. ... The ground state for our rod moving in a velocity v is the state obtainable by a Lorentzian transformation from the ground state of the rod before the motion was imparted.

When discussing this passage, Brown (2005a, p. 121), notes that in it Swann has forcefully demonstrated the power of the constructive approach STR. First, the relativity principle is the consequence of the Lorentz covariance of the quantum dynamics, rather than the other way round. Second, the universality of the behavior of rods and clocks (what Brown calls "the mystery of mysteries") emerges as a consequence of the dynamical argument, as long as matter of any constitution is assumed in principle to obey quantum theory.[14]

Detecting an opportunity to propagate his view to a larger readership, Swann repeats the above words almost verbatim in a letter he sent to *Nature* on September 23, 1941, which was also the pretext for his correspondence with Einstein on the subject.

3.1.2 Swann's letter to *Nature*

In the second half of 1941 *Nature* hosted an exchange between the mathematical physicist J.H. Jeans and Sir Arthur Eddington. The former had just re–read Eddington's (1939) *The Philosophy of Physical Science* and was struck by Eddington's views on the *a priori* character of the fundamental laws of nature. Jeans reports that he had read Eddington "with great admiration, but also with grave doubts as to whether his philosophical position is not wholly unsound" (Jeans 1941, p. 140). The focus of the heated exchange that ensued between the two distinguished physicists on the pages of *Nature* (and that attracted the attention of others, such as Herbert Dingle) was the status of the light postulate of STR and the null result of the Michelson–Morley experiment. It is here where Swann enters the stage, and in a letter to *Nature*, written in September but published only in December that year (Swann 1941c, p. 692), he spells out his own view on the role of the null result of the Michelson–Morley experiment in the foundations of STR.

Swann's view on this role is rather odd–sounding: He claims that there would be meaning to STR even if the Lorentz–FitzGerald contraction were not to hold, and the result of the Michelson–Morley experiment were non–null. What Swann meant in this claim is that it is a purely mathematical fact that Maxwell equations are Lorentz covariant. This fact, however, does not imply in itself that the transformed variables appearing in the Lorentz transformations refer to physical quantities actually measured by an inertial observer moving at the appropriate velocity v relative to the original frame. After all, STR, on Swann's view, while securing the Lorentz covariance of the equations, does not tell us how rods and

[14]Several years later in a pedagogical paper published in the American Journal of Physics, Swann (1960a, pp. 59–61) emphasizes this point when he explicitly constructs an "electron–dial" clock in his discussion on the twins paradox.

clocks behave in motion.[15]

Brown (2005a, p. 120) admits that here Swann may have overstated his view. Indeed, these remarks by Swann are baffling precisely because on his view, the Lorentz contraction is a result of the Lorentz covariance of the quantum theory of matter. But if the covariance of *all* the dynamical laws of nature is what follows from STR, as Swann claims, then the Lorentz contraction *must* hold if STR is to hold any physical meaning.[16]

Eddington responded to Swann in a rather interesting note, in which he traces the solution to the Swann's "mystery of mysteries" (i.e., "how a rod decides its extension when it is given a different motion" (Eddington 1941, p. 692)) to the law of gravitation. The connection to quantum theory, according to Eddington, is that it supplies us with a common standard for a measure of length, because "it is only in quantum theory that a method has been developed of describing material structure by pure numbers . . . Thus appeal must be made to quantum theory for the definition of the interval ds, which is the starting-point of relativity theory" (Eddington 1941, p. 693).

3.2 Swann's letter to Einstein

Swann, it appears, was not impressed with Eddington's reply, and a month after his letter to *Nature* was published, he wrote to Einstein.[17] In addition to mentioning his *Nature* letter, Swann enclosed one of his earlier *Review of Modern Physics* article on the constructive view to STR from 1941, and asked for Einstein's opinion on this view:[18]

> I thought you might be interested in the enclosed in view of its relation of quantum theory to relativity. I gave a digest of it in *Nature*, December 6, and there is a comment by Eddington in the same issue. I find it very difficult to ascertain whether he agrees or disagrees. It seems that he agrees but does not like to.

[15]Swann goes on and restates his idea that if a co–moving observer adopts coordinates that match the moving rod (and clock) associated with a solution to the dynamical equations generated by the Lorentz symmetry group from the solution of these equation that describes a rod at rest, then the quantum description of the moving rod (and clock) by the co–moving observer is exactly the same as the description of the stationary rod by an observer associated with its rest frame.

[16]We shall return to this point in the sequel to this paper (Hagar 2007).

[17]Swann's documented relation with Einstein had started a year earlier, when he tried to get an academic position to an acquaintance of Einstein, Felix Ehrenhaft.

[18]As can be seen from the page numbers to which Swann refers in his reaction to Einstein's reply (see below), the enclosed *Rev .Mod. Phys.* paper was Swann's (1941b).

> As I see it, he wishes to deny the credit of the solution of the paradox to the quantum theory and give it to something more deep–seated, of which the quantum theory is an outcome. With this, of course, everyone must agree in principle, but the situation seems to me a little like one in which someone should give credit to a person A for a solution of a certain paradox, while somebody else claimed that it was A's ancestors to whom the credit should be given because it was they who were responsible for the existence of A.
>
> Do not feel obligated to reply to this letter unless there is something you really wish to say about it, as I am only sending you the paper for your general interest (Swann 1942a).[19]

The paradox, recall, was "the mystery of mysteries", and the way Swann thought he had resolved it was by invoking the constructive approach to STR and a Lorentz covariant quantum theory of matter. Einstein's response arrived after three weeks, and as we shall see, contained some very interesting, albeit cryptic, remarks on the constructive view that Swann was advocating.

3.3 Einstein's reply

3.3.1 The German Version

Einstein's response from January 24, 1942 to Swann's letter and to his *Rev. Mod. Phys.* article (and maybe even to Swann's letter to *Nature*) was written in German and reads as follows:

> Ich habe erst jetzt die mir freundlich übersandte Arbeit ansehen können. Mir scehint deren Inhalt war nicht unrichtig, doch in gewissem Sinne irreführend.
>
> In der speziellen Relativitäts–Theorie werden (idealisierte, aber doch im Prinzip als realisierbar aufgefasste) Masstäbe und Uhren als selbständige physikalische Objekte behandelt, die — als verknüpft mit den Koordinaten der Theorie — in die Aussagen der Theorie mit eingehen. Dabei ist zunächst über die strukturellen Naturgesetze nichts weiter ausgesagt, als dass sie mit Bezug auf so definierte Koordinatensysteme Lorentz–invariant sein sollen.
>
> Es ist zunächst bewusst darauf verzichtet, Masstäbe und Uhren unter Zugrundelegung von Struktur–Gesetzen als Lösungen zu behandeln. Dies ist

[19] All quotes appearing here, including Einstein's letter, are verbatim, and are reprinted from the original letters that can be found in Einstein's archive in the Hebrew University in Jerusalem, and in the W.F.G. Swann Archive at the American Philosophical Society, Philadelphia, PA. I thank Charles Greifenstein from the APS Library for his kind help in accessing the relevant letters.

darum wohlbegründet, weil die (prinzipielle) Existenz solcher als Masstäbe für Koordinaten dienlicher Objekte vom Standpunkt unserer Erfahrungen besser begründet erscheint als irgendwelche besonderen Strukturgesetze, z.B. die Maxwell'schen Gleichungen.

Fasst man die Sache so auf, so hat der Michelsonversuch sehr wohl mit der speziellen Relativitäts–Theorie zu tun, sobald man das Prinzip von der Konstanz der Lichtgeschwindigkeit oder (darüber hinaus) die Maxwell'schen Gleichungen hinzunimmt.

Wenn man aber Masstäbe und Uhren nicht als selbständige Objekte in die Theorie einführen will, so muss man eine strukturelle Theorie haben, in welcher eine Länge fundamental eingeht, die dann zur Existenz von Lösungen führt, in denen jene Länge bestimmend eingeht, sodass es nicht mehr eine kontinuierliche Folge ähnlicher Lösungen gibt. Dies ist zwar bei der heutigen Quantentheorie der Fall, hat aber nichts mit deren charakteristichen Zügen zu tun. Jede Theorie, welche eine universelle Länge in ihrem Fundament hat und auf Grund dieses Umstandes qualitativ ausgezeichnete Lösungen von bestimmter Ausdehnung liefert, würde inbezug auf die hier ins Auge gefasste Frage dasselbe leisten. (Einstein 1942)

3.3.2 The English translation

Translated to English, Einstein's letter reads as follows:[20]

> Only now I have been able to look at the work that you so kindly sent to me. It seems to me that its content was not incorrect, but still in a certain sense misleading.
>
> In special theory of relativity measuring rods and clocks (idealized, but in principle conceived as realizable) are treated as independent physical objects, which, linked as they are to the coordinates of the theory, will enter into the propositions of the theory. At first there is nothing stated about the structural laws of nature other than the fact that they should be Lorentz–invariant with reference to coordinate systems so defined.
>
> Measuring rods and clocks are consciously not treated as solutions under the basis of structural laws [this sentence is a little ambiguous in the original]. This is well justified because from the point of view of our experiences, the (in principle) existence of those objects that can serve as measures for

[20] I thank Jutta Shickore and Sandy Gliboff (HPSC, IUB) for their help in translating Einstein's letter into English.

> coordinates appears better justified than any particular structural laws, e.g. Maxwell's equations.
>
> If one looks at the issue this way, the Michelson experiment does indeed have something to do with special theory of relativity, as soon as one adds the principle of the constancy of the velocity of light or (furthermore) Maxwell's equations.
>
> But if one does NOT [underlined in the original] introduce rods and clocks as independent objects into the theory, one has to have a structural theory in which a length is fundamental, which then leads to the existence of solutions in which that length plays a determinant [constitutive] role, so that a continuous sequence of similar solutions no longer exists. This is the case in today's quantum theory but has nothing to do with its characteristic features. Any theory that has a universal length in its foundation, and because of this produces qualitatively distinct solutions of a certain extension, would do the same with regard to the question under examination here.

Before we set forth to decipher this response and to analyze its implications on some of Brown's claims, let us end this anecdotal chain of events with Swann's reaction to Einstein's reply.

3.4 Swann's reaction

Swann seems to have asked for a translation of Einstein's letter, and this translation, which apparently was done by an amateur,[21] is enclosed along with his correspondence with Einstein in Swann's archives. One can only lament on this infelicity since Einstein's original German version of the letter is, to say the least, non–transparent, and as we shall see, an unqualified translation could easily (and actually did) lead to misunderstanding of Einstein's entire point.

Swann's reaction to Einstein's letter came a little more than a fortnight later, and it marks the end of his documented discussions with Einstein's on the constructive approach to STR.[22] In his letter Swann (1942b) admits that the point he was making "did not depend specifically upon all of those features of the quantum

[21] The translation Swann got looks awkward to a native German speaker, and shows lack of knowledge of the subject matter.

[22] It appears that Swann maintained contact with Einstein and even visited him in 1950 in Princeton, to discuss a public talk he (Swann) was preparing at the Franklin Institute. No record of this meeting exists, apart from a letter Swann sent to Einstein after the meeting took place, to which he attached his planned talk, in order to thank Einstein and to allow him to veto any part of the talk that regarded Swann's reconstruction of Einstein's work on the theories of relativity.

theory which are of interest in the atomic structure", and he agrees with Einstein on the necessity of the constructive theory defining a length. Nevertheless, Swann claims that there are two additional reasons why one should look at quantum theory.

First, the theory displays a unique ability to supply a (relativistically invariant) measure of length (here Swann gives Bohr's hydrogen atom model as an example where the fundamental length would have been the radius of the first electronic orbit). Second, the theory allows us, using this measure of length, to determine what will be the ground state of the rod:

> It is this power to fix a length which is a special aspect of the determination of structural form and that is why, in speaking of the rod, I give the quantum theory, on page 201 [Swann 1941b — AH], the credit of determining that it shall be in a *ground state* [underlined by Swann — AH]. It seems to me that it is this act of the quantum theory which is significant and which is another aspect of its power to fix a length. (Swann 1942b)

Swann ends his letter with an apologetic tone:

> Please do not feel that it is necessary to reply to this letter. Of course, it is always a pleasure to hear from you on these matters, but I do not think there is any very great divergence of view point in this instance.

from which it is clear that Swann had interpreted Einstein's response as sympathetic to his (constructive) explanation of the Lorentz–FitzGerald contraction with a Lorentz covariant quantum theory of matter!

In the next section I shall argue that Swann completely misunderstood the gist behind Einstein's response, and, moreover, that he is not alone in his misunderstanding. In effect, Einstein's letter, which was mentioned only twice in the literature on the foundations of STR, was in both cases either misrepresented or misinterpreted.

4 Reconstruction

Admittedly, Einstein's letter to Swann is not one of his famous letters. Nor is it one of his most transparent. In effect, while mentioned only twice in the vast literature on the foundations and history of STR, this letter was misinterpreted on both occasions. In this section we shall display these misinterpretations, and then set the record straight. This task is of crucial importance since Einstein's letter contains an interesting warning to advocates of the constructive approach to STR, which only lately, with the development of quantum gravity, has retained its full meaning.

4.1 Lost in translation

4.1.1 Stachel's de-contextualization

In his recent *Einstein from 'B' to 'Z'*, John Stachel (2002) devotes a chapter to "Einstein and the Quantum". Einstein's letter to Swann appears here for the first time in a rather misplaced context. Stachel brings the letter as evidence to the claim that Einstein (in his attempts to explore the possibility that a field theory based upon a continuous manifold, the principle of general covariance and partial differential equations, could provide an explanation to quantum phenomena) sometimes indicated that a fundamental length might be needed to explain the existence of stable structure in such a field theory. He then (mis)quotes only the *last* paragraph of the letter (Stachel 2002, p. 394).

But clearly, by now the reader can appreciate that Einstein's letter to Swann, while it may be classified as pertaining to Einstein's views on the relations between relativity theories and quantum mechanics, and while, as we shall see, is connected to Einstein's attempts to explain quantum phenomena with a generally covariant continuous field theory, is *not* in any way meant to indicate that a fundamental length is needed to explain the existence of stable structures in such a field theory, as Stachel asserts.

Next, while understandable from an editorial perspective, the omission of the first part of the letter (and with it the omission of the real context it was written in and the attitude towards the constructive approach it displayed) is nevertheless problematic since it appears to have misled Brown to claim that Einstein *agreed* with Swann that a dynamical explanation of the Lorentz–FitzGerald contraction is wanting, and that quantum theory is indeed the missing ingredient in supplying such an explanation.

4.1.2 Brown's (mis)interpretation

Brown (2005a, Ch. 7), devoted as it is to the dissenting, unconventional, voices on relativity, is a wonderful starting point for any future research on the constructive approach to STR. Brown's inclusion of Einstein in this chapter, however, is somewhat misleading. The problem is that reading Brown one gets the impression that Einstein was supportive of the constructive view to STR. The innocent reader is led, for example, to believe (as Swann does) that Einstein agrees with Swann on the need for dynamical explanation for the Lorentz–FitzGerald contraction, and on the role of quantum theory in supplying such an explanation. Yet the evidence for this "agreement" is nothing else but the paragraph (mis)quoted by Stachel:

> It is known that Swann corresponded with Einstein on the foundations of SR; in Stachel (1986) p. 378 [this part appears also in Stachel 2002, p. 394] – AH], part of a 1942 letter from Einstein to Swann is cited in which he discusses the possibility of a constructive formulation of the theory wherein rods and clocks are not introduced as 'independent objects'. Einstein argues in this letter that any such theory must, like the quantum theory, contain an absolute scale of length. It would be interesting to know more about this correspondence. (Brown 2005a, p. 120, fn. 19)

But knowing what we know now on this correspondence it seems that Einstein, rather than supporting Swann, was not at all sympathetic to his ideas when he replied in 1942.

4.2 A more accurate interpretation of Einstein's response

4.2.1 Reading Einstein step by step

Let us look more carefully at Einstein's response to Swann. The first thing to note is that from the very beginning, Einstein, in his famous style, is criticizing Swann without saying so explicitly when he writes in the opening sentence:

> . . . It seems to me that the content [of Swann work — AH] was *not incorrect but still in a certain way misleading*. [my italics — AH]

I cannot refrain from making an analogy between Einstein's attitude to Swann's work and his reaction to Reichenbach's *Relativity and A Priori Knowledge* (1920), where Einstein, when asked whether he considers true what Reichenbach had asserted, mischievously answered "I can only answer with Pilate's famous question: 'what is truth?"' (Schilpp 1949, p. 676).

The second paragraph presents the principle view of STR, in which measuring rods and clocks are treated as idealized (but in principle realizable) *primitives*, i.e., independent, unanalyzed physical objects. On this view nothing is said about the dynamical laws of nature other than the fact that they should be Lorentz–invariant with reference to coordinate systems defined by the primitive rods and clocks. Note that here Einstein restates the epistemological priority, under the principle view, of the geometrical symmetries of spacetime over the symmetries of the dynamical laws. To paraphrase Brown again, it is quite clear here which is the cart and which are the horses.

The next paragraph justifies this principle view: Measuring rods and clocks are "consciously" (i.e., on purpose) not treated as solutions to the dynamical laws (i.e., solutions to the equations of motion) — as Swann would have it — and this view is well justified since on the basis of our experience (e.g., the null result of the Michelson–Morley experiment – see below — or the empirical content of the relativity principle), the in principle existence of these primitives as measures of coordinates (i.e., as representing the geometry of spacetime) seems to be better justified than any given dynamical theory of matter that one may come up with. This last sentence reminds us of the agnosticism of the principle view with respect to the actual dynamical model for observed phenomena.

However, in Swann's amateurish translation the first sentence of this paragraph was translated as saying "We at first consciously desist from treating measures and clocks as solutions by taking structural laws as basis". This was apparently interpreted to imply that instead of taking rods and clocks as primitives, as the former paragraph suggests, one should take the dynamical laws as a basis. Clearly this is not what Einstein meant here, as can be seen from the German version and from the place of this paragraph in the letter (Einstein turns to the constructive view only two paragraphs later).

Next comes another blow to Swann's view: If one looks at the issue this way, the Michelson experiment does indeed have something to do with special theory of relativity, as soon as one adds the principle of the constancy of the velocity of light or (furthermore) Maxwell's equations.

It is only in the last paragraph where Einstein finally turns to the constructive view. Unfortunately, this is the most obscure paragraph in the letter, and any minor changes in its translation can result in totally different interpretations.

Let us start with the first sentence of this paragraph. It is a conditional sentence, with a short antecedent and a long and winding consequent. The short antecedent is, in paraphrase, preparing the stage for what, in Einstein's view, happens when one does *not* wish to take measuring rods and clocks as primitives, i.e., when one insists, contra all what was said before in the letter, to take the opposite view to the principle view, namely the constructive view. The long and winding consequent, again in paraphrase, is a logical chain that follows from the constructive view. First, the dynamical theory that one utilized in order to construct the kinematical effects (and to explain the geometry of spacetime) must establish a fundamental measure of length. Second, this (here "this" is ambiguous — either this fundamental measure of length, or the fact that the dynamical theory has such a measure length) leads to the existence of solutions (of the dynamical equations) in which length plays a constitutive role. Third, since this fundamental length appears in the solutions to the equations of motion, a continuous sequence of similar solutions no longer exists (note that the negation "no longer exists" can be equally interpreted as quantifying over "continuous", or over "similar", or over both).

The second sentence of the last paragraph is quite straightforward and says that the dynamical theory's having a fundamental measure of length is indeed the case with today's quantum theory but has nothing to do with its characteristic features. The final sentence then says that *any* theory which has a universal length in its foundations and because of this circumstance yields qualitatively distinguished or distinct solutions of a certain extent would do the same with respect to the question under examination here.

4.2.2 Interpretation

We can divide Einstein's response to Swann into four distinct parts. The first is the opening sentence that sets the tone: Swann's view on the role of the Michelson–Morley experiment, and on the role quantum theory plays in dynamically explaining the Lorentz–FitzGerald contraction is, according to Einstein, misleading. The second is the description of the standard, principle, view of STR, where rods and clocks are taken as primitives, and the explanation of the Lorentz–contraction is a geometrical one. The third is the admission that one can reject the principle view and adapt the constructive view instead, and the warning that such a move entails several non–trivial consequences, at least one of them being discontinuity or discreteness. The fourth and final part is the observation that there is nothing special in quantum theory as such that makes it a preferable candidate for the constructive approach.

The first conclusion from this reconstruction that comes to mind is that, contrary to what Swann wanted to believe, and quite opposite to what Brown claims, in his letter to Swann Einstein was not at all expressing any sympathy to the constructive view. This may be a warning sign against the broader claim Brown is making throughout his project, namely, that Einstein's ambivalence with respect to the principle view warrants annexing him to the constructive camp. I shall examine this claim in detail in the following section (5). The second conclusion more philosophical. His letter to Swann gives us a rare opportunity to discuss what Einstein saw as an inevitable consequence of the constructive approach to STR. The problem is that this letter is open to many interpretations. We shall defer the analysis of these interpretations to section (6).

5 Einstein and the constructive approach to STR

In this section I shall attempt to raise doubts with respect to one of Brown's claims in *Physical Relativity* (2005a), namely, that Einstein himself, although unappreciative of the developments in relativistic quantum theory, was a supporter of the constructive approach to STR. Admittedly, the truth of this claim is not in any sense essential to the success of Brown's project. However, it does carry a certain historical significance.

My own view, contrary to Brown, is that Einstein — although ambivalent with respect to his celebrated choice in the principle view in 1905 — was nevertheless unimpressed *in general* with the constructive approach to STR, and in particular with attempts such as Swann's to harness the quantum theory of matter to dynamically explain kinematical relativistic effects such as the Lorentz–FitzGerald contraction. Indeed, if, as Brown (2005b, p. S87) claims, Einstein was so *in favor* of the constructive approach to STR and saw the principle view of 1905 as a choice of despair (after realizing in his light quanta article that Maxwell's theory was not the final word on the structure of matter), then why, years later, after a relativistic quantum theory of matter has become available, he was still reluctant to acknowledge its implications on his choice?

Brown (2005a, p. 114) side–steps this embarrassing question and suggests that Einstein's "long–standing distrust" and hostility towards quantum theory prevented him from recognizing the progress in the theory and its implication on the formulation of STR. However, as we can see from the last paragraph of his letter to Swann and from other writings I shall quote below, Einstein was troubled with a *general* feature of the constructive approach to STR, and not with quantum theory *per se*. This general feature was the fact that any such constructive theory of the constitution of matter with which one would try to explain the geometrical structure of spacetime, must include, according to Einstein, a fundamental measure of length. One way of interpreting this feature is to say that any constructive approach to STR must depart from the continuum. Naturally, since Einstein was involved most of his life with search for a unified *field* theory, discontinuity and discreteness were, for him, something to worry about.

5.1 The myth of the physics of despair

A recurrent theme in Brown's project is the claim that Einstein expressed, throughout the years from 1907 at least until 1949, a certain "unease" with respect to his choice in the principle view to STR:

> When Einstein formulated his 1905 treatment of relativistic kinematics, the template in his mind was thermodynamics. This was because a more desirable 'constructive' account of the behaviour of moving rods and clocks, based on the detailed physics governing their microscopic constitution, was unavailable. The price to be paid was appreciated by Einstein and a handful of others since 1905. (Brown 2005b, p. S85)

Brown then brings several references where Einstein is expressing 'misgivings' about the principle view of STR he has adapted in his celebrated 1905 paper.[23] The point Brown is trying to make here is that Einstein's choice when adapting the principle view was a choice of despair. Since several months before the 1905 STR paper Einstein has written another revolutionary paper claiming that electromagnetic radiation has a granular structure, the assumption, prevalent among his contemporaries, that Maxwellian electrodynamics is strictly true, had lost his trust. If Maxwell's equations were thought by Einstein as incompatible with the existence of the photon, then there was no sense, says Brown (2005b, p. S87), in trying to write down a constructive, dynamical, theory for relativistic kinematical effects on the basis of classical electrodynamics as the latter could not be regarded as the complete theory of the constitution of matter.

Einstein mentions in several occasions that the example he had in his mind when retreating to the principle view of STR was TD, where, although no assumptions on the constitution of matter are made, few phenomenological and restrictive principles suffice to predict the behavior of bulk matter. Einstein, says Brown (2005b, p. S89), would have preferred a constructive account of the relativistic effects of length contraction and time dilation, but in 1905 the elements of such an account were unavailable.

Yet if this is the case, then the the "despair" of electrodynamics could not have been the reason why Einstein retreated to the principle view. For as we have seen, such a retreat from the constructive approach to the physics of principles is motivated, according to the 19th–Century mathematical physicists, when one has in one's disposal *too many* constructive models to a certain class of phenomena. But in Einstein's case, says Brown, not even *one* model was available.

Einstein indeed admits that strictly speaking, measuring rods and clocks should have been represented as solutions to the basic equations and not as primitives, but then he adds in the spirit of his response to Swann that:

[23] These include Einstein's (1907) letter to Ehrenfest, Einstein's (1908) letter to Sommerfeld, Einstein's (1919) letter to the London *Times*, Einstein's (1921) famous paper on *Geometry and Experience*, and several quotes from Einstein's reflections in his *Autobiographical Notes* (Schilpp 1949).

> it was clear from the beginning that the postulates of the theory [STR — AH] are not strong enough to deduce from them sufficiently complete equations for physical events sufficiently free from arbitrariness, in order to base upon such a foundation a theory of measuring rods and clocks. (Schilpp 1949, p. 59)

So it was not the breakdown of Maxwellian electrodynamics which lead Einstein to the principle view, but rather his belief in the the arbitrariness of any constructive approach whatsoever.[24] At most, Einstein can be interpreted here as neutral with respect to the constructive approach, but it is quite clear that instead of looking for dynamical laws to explain the "mystery of mysteries", Einstein preferred Minkowski's four–vector formalism:

> Before Minkowski's investigation it was necessary to carry out a Lorentz–transformation on a law in order to test its invariance under such transformations; he, on the other hand, succeeded in introducing a formalism such that the mathematical form of the law itself guarantees its invariance under the Lorentz–transformations . . . He showed the Lorentz–transformations are nothing but a rotation of the coordinate system in the four–dimensional space. (Schilpp 1949, p. 59)

It thus appears that Einstein, agreeing as he was with Swann and Brown, and all the other 'constructivists', that the fundamental physical content of STR is that the laws of physics *in general* are invariant with respect to the Lorentz–transformation (Schilpp 1949, p. 57), took the geometrical structure of spacetime as the only available explanation for this lesson since any other dynamical explanation that one may come up with would be arbitrary.

Consequently, according to Einstein the principle view of STR can be augmented with a an explanation which, while not committed to any mechanism whatsoever, is nevertheless *not* agnostic with respect to the geometrical structure of spacetime since the theory with all its observable consequences can also be *constructed synthetically* based on the exceedingly elementary hypothesis that the underlying structure of space and time is Minkowskian. In particular, it this is this structure that allows one to explain the kinematical effects without reverting to any (arbitrary) dynamical laws — laws that would have to be, of course, Lorentz–covariant.

When matter are put in these terms (and as his correspondence with Swann demonstrates), it is not at all clear that Einstein would have preferred a fully constructive formulation of STR of the sort Swann was advocating. Agreed, Einstein

[24] See also Howard (Howard 1998, especially pp. 154–159).

often remarked that the concept of the rigid rod, while justified by our approximated experience and necessary in the process of reducing geometry into physics, is nevertheless arbitrary (Schilpp 1949, p. 55, and Einstein 1921/1982, pp. 237–238), and he was also aware of what he called "the latent inconsistency" in STR wherein rods and clocks posses a special status as unanalyzed primitives and as such differ from all other physical variables (Schilpp 1949, pp. 59–61). But the remedy Einstein had for this latent inconsistency was not dynamical. Rather, it was geometrical. For instead of turning geometrical explanations into dynamical ones, Einstein, at least in his later work on the general theory of relativity and on the unified field theory, and even in his views on STR (as can be seen from his comment on Minkowski's spacetime above) was trying to turn dynamical explanations into geometrical ones.

Brown (2005a, p. 114) conjectures that Einstein's long–standing distrust of the quantum theory might have led him to ignore the implications of its progress on foundations of STR, but Einstein's letter to Swann and even a paragraph which appears just below the one Brown quotes as evidence for Einstein's support in the constructive approach reveal the real reason why Einstein disliked not only the quantum dynamical explanation of the Lorentz–contraction, but *any* other dynamical explanation thereof:

> It therefore appears unavoidable that physical reality must be described in terms of continuous functions in space. The material point, therefore, can hardly be conceived any more as the basic concept of the theory. (Schilpp 1949, p. 61)

5.2 Discontinuity

It is customary to attribute to Einstein a "rigid adherence to classical theory" (Schilpp 1949, p. 675) in his attitude to the quantum theory. Most commentators interpret this rigidity as manifest in Einstein's view of quantum theory as incomplete (e.g., Bohr in Schillp 1949, p. 235), or in his reluctance to abandon the notion of separability (Howard 1985), or even in his prejudice for causality (Fine 1986, pp. 100–103). But there is one feature which Einstein himself saw as germane to his objection to the quantum theory and which received less attention. This feature is continuity:

> The opinion that continuous fields are to be viewed as the only acceptable basic concepts, which must also [be assumed to] underlie the theory of material particles, soon won out. Now this conception became, so to speak, "classical;", but a proper, and in principle complete, *theory* has not grown

> out of it. . . . Consequently there is, strictly speaking, today no such thing as a classical field–theory; one can, therefore, also not rigidly adhere to it. Nevertheless, field theory does exist as a program: "Continuous functions in the four–dimensional [continuum] as basic concepts of the theory." Rigid adherence to this program can be rightfully be asserted of me. (Schilpp 1949, p. 675)

In this passage Einstein directly responds to the "friendly accusation" made by Pauli:

> The theoretical determination of the fine structure constant is certainly the most important of the unsolved problems of modern physics. We believe that any regression to the ideas of classical physics (as, for instance, to the use of the classical field concept) cannot bring us nearer to this goal. (Pauli in Schilpp 1949, p. 158)

It is well known that Einstein carried a decades–long search for a classical field theory whose singularity–free solutions could reproduce the atomistic structure of matter and radiation. What is less known, however, is that he had regarded the dichotomy between the continuum and the atomistic views of nature as *equivalent* to the dichotomy between the principle and the constructive views of theories:

> The electrons (as points) would be the ultimate entities in such a system (building blocks). *Are there* indeed such building blocks? . . . With the continuum viewpoint one is better off in this respect, because one doesn't have to prescribe elementary building blocks from the beginning. Further, the old question of the vacuum! (Einstein to Walter Dällenbach, November 1916, in Stachel 2002, p. 395)

Furthermore, Einstein, in his letter to the *NYT* from 1929 famously remarked that "People slowly accustomed themselves to the idea that the physical states of space itself were the final physical reality", explicitly linking geometrical explanations with the concept of the field:

> The general problem is: Which are the simplest formal structures that can be attributed to a four-dimensional continuum and which are the simplest laws that may be conceived to govern these structures? We then look for the mathematical expression of the physical fields in these formal structures and for the field laws of physics — already known to a certain approximation from earlier researches — in the simplest laws governing this structure. The conceptions which are used in this connection can be explained just as well in a two-dimensional continuum (a surface) as in the four-dimensional continuum of space and time. (Einstein 1929)

I believe that Einstein's reluctance to abandon the continuum view of nature is the reason behind his rejection of the constructive approach to STR that Swann was advocating in his letter. This is why he warns Swann that any such constructive theory (with the fundamental measure of length it postulates) will lead to breakdown of continuity. Einstein was aware, of course, that his research program to which he "rigidly adhered" might not be successful.[25] But what is interesting, and what lends altogether more support to my claim that Einstein — ambivalent as he was with respect to his choice in the principle view — did *not* favor a dynamical approach to STR, is the fact that when considering the alternative to the continuum view, rather than dynamical theories, Einstein envisioned "a purely algebraic physics":

> The alternative continuum–discontinuum seems to me to be a real alternative; i.e., there is no compromise. By discontinuum theory I understand one in which there are no differential quotients. *In such a theory space and time cannot occur* [my emphasis], but only numbers and number–fields and rules for the formation of such on the basis of algebraic rules with exclusion of limiting processes. Which way will prove itself, only success can teach us. (Einstein to H.S. Joachim, August 1954, in Stachel 2002, p. 396)

Since in such a discrete framework "space and time cannot occur", it is clear that Einstein would have been reluctant to endorse Swann's constructive approach to STR, or any other constructive approach which, according to him, must entail, like quantum theory, a fundamental measure of length.

6 Length matters

In the last section I have offered what I believe to be a plausible resolution of the debate on Einstein's view on the constructive approach to STR. As always is the case with Einstein, his insight was far more outreaching than his contemporaries. His views on STR were thus couched in a much broader context, which involved not only the quantum theory, but also his general theory of relativity (GTR). Alas, Einstein died in 1955, well before quantum gravity became a respectable science. But his prophetic remarks on the tension between quantum theory and GTR are worth mentioning here:

> The theory of gravitation showed me that the non–linearity of these equations [the field equations — AH] results in the fact that this theory yields

[25] See, e.g., his letters to Langevin, Kondo, Besso, Lichnerowicz, Bohm and Joachim, quoted in Stachel (2002, pp. 395–396).

> interactions among structures (localized things) at all. But the theoretical search for non–linear equations is hopeless (because of too great variety of possibilities), if one does not use the general principle of relativity (invariance under general continuous co–ordinate transformations). In the mean time, however, it does not seem possible to formulate this principle, if one seeks to deviate from the above [continuum — AH] program. (Schilpp 1949, pp. 675–676)

> Contemporary physicists do not see that it is hopeless to take a theory that is based on the independent rigid space (Lorentz–invariance) and later hope to make it general relativistic (in some natural way). (Einstein to Laue, September 1950, in Stachel 2002, p. 393)

Apparently, Einstein sensed a tension between the general covariance of GTR and the background dependence of the quantum theory (even in its relativistic formulation). It is this tension that warrants a full examination of the last paragraph in his letter to Swann, where his warning as to the loss of "continuous sequence of similar solutions" that results from the fundamental measure of length the constructive view requires can be interpreted as a warning against the breakdown of symmetry that this approach might entail.

6.1 Ambiguities

Einstein's long and winding conditional sentence, contained in the last paragraph of his letter to Swann, may be interpreted in many ways. First, Einstein is saying that a constructive approach to STR (i.e., a dynamical explanation to the Lorentz–FitzGerald contraction) requires that the dynamical theory will contain a *fundamental* measure of length. But the word *fundamental* can mean here at least two different things, e.g., *universal*, or *minimal*. Second, Einstein is saying that when a fundamental measure of length is introduced at the level of the dynamical equations, a continuous sequence of similar solutions to these equations no longer exists. Here the negation "no longer exists" can quantify over "continuous", over "similar", or over both. In the first case, this means that the constructive approach leads inevitably to discreteness and discontinuity. In the second and the third cases, this means that the constructive approach leads inevitably to breaking of symmetry.

Let us start by examining the possible interpretation of the word "fundamental" (in German: fundamental). Einstein, in fact, repeats the locution "fundamental measure of length" also in the last sentence of that paragraph, but there he uses the word "universal" (in German: universelle). In physics, the word "fundamental" usually signifies the lowest, i.e., minimal element. But while a minimal length is always universal, the converse does not hold. Nevertheless, his reply to Einstein suggests that Swann had understood the word "fundamental" as implying "minimal", as can be seen from the example he gives for such a minimal length, namely the the radius of the first electronic orbit in Bohr's Hydrogen atom model, and the fact that such a length is related in quantum theory to the *ground* state of the atom.

These considerations and the discussion in section (5.2) lead me to believe that it is safe to interpret Einstein here as saying that the introduction of fundamental measure of length leads to discontinuity, or discreteness. As a result, the word "fundamental" should be interpreted here as "minimal".

So for Einstein it was an inevitable fact that the constructive approach to STR (in which one is trying to explain Length contraction *dynamically* and not *geometrically*) must depart from the continuum since it must include it its basis a minimal length scale that would then (universally) appear in the solutions of the dynamical equations. But what are the consequences of this inevitable fact? To answer this question we should look again at the last paragraph of Einstein's letter to Swann.

Einstein is saying there that (1) a minimal measure of length, since it appears in the solutions to the dynamical equations of the constructive theory, leads to a situation in which "continuous sequence of similar solutions no longer exists", and that (2) "[a]ny theory that has a universal length in its foundation, and because of this produces qualitatively distinct solutions of a certain extension would do the same with regard to the question under examination here". One can easily claim, as we have done in the previous section, that here Einstein simply restates his view that a dynamical explanation to the geometrical structure of spacetime must depart from the continuum. This interpretation, while ignoring (2), regards the negation "no longer exists" in (1) as quantifying only over the word "continuous".

But what if we include in the scope of the negation in (1) the entire locution "continuous sequence of *similar* solutions" and choose *not* to ignore (2) and in particular not to ignore the locution "*qualitatively distinct* solutions of a certain extension"[26] in it? On this, admittedly invested, reading of Einstein, the consequence of discreteness is breaking of the continuous symmetries of spacetime, namely, breaking of Lorentz invariance.

While Einstein's letter to Swann certainly allows for such an invested reading, I do not wish to speculate on Einstein's actual view. Indeed, although in a hind-

[26]Emphasis mine in both quotes.

sight his remarks may seem prophetic, I doubt that Einstein really foresaw the current state of affairs in the domain of quantum gravity (see below). For the purpose of this paper, however, it is sufficient to establish (as we have done in section (5)) the more neutral reading of the paragraph, according to which Einstein was unsympathetic to the constructive approach to STR because he believed it demolishes the continuum view of nature to which he "rigidly adhered" (Schilpp 1949, p. 675).

But one can still examine such an invested reading of Einstein's letter to Swann that sees the breakdown of Lorentz invariance as an inevitable consequence of the constructive approach, especially in light of Brown's (and Bell's) claim about the neutrality of this approach on this subject. In effect, this reading resonates well with contemporary research in the domain of quantum gravity, where the question of the compatibility between a discrete spacetime and Lorentz invariance has recently become an issue of major theoretical and experimental interest.

6.2 Minimal length in quantum gravity

One can identify two possible conditionals in Einstein's letter. The first says that any constructive approach to STR must depart from the continuum and employ a notion of a minimal length (call this conditional LENGTH). The second says that any theory that employs a minimal length must break the continuous Lorentz symmetry (call this conditional BREAK). In section (5) we have established that Einstein believed that LENGTH hold, and that his bias for the continuum positioned him in the *opposite* camp to the constructive approach to STR. Whether or not Einstein held BREAK to be true is an open historical question.

The interesting *philosophical* (and mathematical) question, however, is whether LENGTH and BREAK *are* true. The stakes here are rather high, especially in the context of the attempts to unify quantum theory and GTR. Indeed, as Brown himself admits (Brown 2005a, p. 151, p. 171, pp. 176–177), there exists a tension between the dynamical approach to STR and the structure of GTR. Recall that the local validity of STR is derived from what Brown calls (2005a, pp. 169–172) the Strong Equivalence Principle (SEP). This tension between the dynamical approach and GTR is manifest in two of the following three possibilities: (i) SEP is fundamentally true; (ii) SEP is only phenomenology true (i.e., approximately, in a certain limit) but its violations are undetectable; and (iii) SEP is not only fundamentally false but also phenomenologically false, and its violations may be detected in certain scenarios. Clearly, that LENGTH may be true is inconsequential here, but if BREAK were true, then option (i) would be false, and Brown's "Big principle" would cease to be fundamental and would become only approximately true (i.e., in cases where gravitational effects can be neglected). Note also that Brown's detachment from the epistemological interpretation of STR constrains him to deny (ii).[27]

While I admit I couldn't find any *a priori* argument that can establish LENGTH or BREAK, the remarkable (contingent?) fact is that the current theoretical landscape in quantum gravity has vindicated Einstein's belief in LENGTH: A large number of convincing semiclassical considerations indicate that in a quantum theory of gravity the Planck length $L_p \equiv \sqrt{\hbar G/c^3} \sim 1.6{\cdot}10^{-35}m$ should play the role of minimal observable length (Garay 1995). This is indeed the case in most, if not all, tentative quantum theories of gravity, e.g., string theory (Smolin 2001, pp. 179–193), canonical quantum gravity (Butterfield and Isham 2001) and loop quantum gravity (Rovelli 2004, pp. 249–259).

Whether or not the current state of affairs in theoretical and experimental high energy physics vindicates BREAK, however, is still an open question — perhaps the most fascinating question in the quantum gravity research program. For a detailed examination of this question and its consequence on Brown's approach the reader is referred to a sequel to this paper (Hagar 2007).

7 Concluding remarks

Defending them with textual evidence, in this paper I have made several historical observations:

- Einstein's choice in the principle view on STR does not signify a choice of despair, but rather exemplifies a conscious bias towards the physics of the continuum.
- Einstein did *not* support the constructive approach to STR, as can be seen from his letter to Swann. Instead, he preferred the geometrical explanation of the kinematical effects as fostered by Minkowski's spacetime structure.

[27] Notably, Bell, at least at some points along his career (e.g., Bell 1986, 48–50), supported Bohmian mechanics — a theory that violates STR but also incorporates a dynamical mechanism that prevents us from detecting this violation, and thus employs exactly the type of Lorentzian philosophy from which Brown is detaching himself. Hagar (2007) picks up this thread and exposes a similar maneuver in the quantum gravity domain which appears to saddle Brown in an interesting and unexpected dilemma.

- In effect, Einstein regarded the principle/constructive distinction as equivalent to the distinction between the continuum and the discrete views of nature, and "rigidly adhered" (Schilpp 1949, p. 676) to the former.

Additional support to these observations can be found in Einstein's views on the separability principle. It is well known that Einstein regarded this principle as constitutive to any physical theory since it provides a clear cut criterion for individuation: one can draw the lines between the parts of the universe anywhere one wants — "there are "joints" everywhere" (Einstein 1948, cited in Howard 1993, p. 239). Field theory does this in the most extreme possible fashion, adds Howard, by regarding every infinitesimal region of spacetime as a separate system characterized by its own, separate state. Howard then notes that loss of separability, brought by quantum mechanics, also signified the departure from the spatiotemporal description of nature. The analysis presented here allows one to include Einstein's reluctance to abandon the continuum within the same realistic research program which Howard (1993) describes — a research program that prevented him from supporting to the constructive approach to STR.

It is a remarkable fact that although there is no *a priori* argument for it, Einstein's belief that the constructive approach, i.e., the attempt to explain geometry with dynamical laws, will lead to breakdown of continuity, is vindicated by the current state of affairs in quantum gravity. Whether the constructive approach will lead to breakdown of Lorentz symmetry is still an open question. Naturally, the competing theories of quantum gravity have yet to supply us with definite predictions that can elevate the research in this domain from pure mathematics to actual science.[28] But even under this proviso, and thanks to Einstein who had prophetically opened up these questions in his letter to Swann, the analysis presented here puts interesting constraints on the advocates of the constructive approach to STR: In their theoretical choices within the domain of quantum gravity they would have to take into account that length matters.[29]

[28]For a progress in this directions see Amelino Camelia (2002).

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