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EINSTEIN AND MACH'S PRINCIPLE

INTRODUCTION

Einstein's attempt to realize Machian ideas in the construction of general relativity was undoubtedly a very major stimulus to the creation of that theory. Indeed, the very name of the theory derives from Einstein's conviction that a theory which does justice to Mach's critique of Newton's notion of absolute space must be generally relativistic, or covariant with respect to the most extensive possible transformations of the spacetime coordinates.

The extent to which general relativity is actually Machian is, however, the subject of great controversy. During the last six months, I have been examining closely all of Einstein's papers that concern the special and general theory of relativity together with a substantial proportion of his correspondence related to relativity. There were several things that I wished to establish: 1) What precisely was the defect (or defects) in the Newtonian scheme that Einstein sought to rectify in his general theory of relativity? 2) How did Einstein propose to rectify the perceived defect(s)? 3) What relation does Einstein's work on his Machian ideas bear to the other ideas and work of his predecessors and contemporaries on the problem of absolute and relative motion? 4) Finally, to what extent did general relativity solve that great and ancient problem of the connection between and status of absolute and relative motion?

In this paper, which addresses the first three issues and gives my main conclusions (which are being presented in more detail together with my attempt at an answer to the fourth question in a forthcoming book (Barbour, in preparation)), I begin by reviewing the most important contributions to the discussion of absolute and relative motion made by Einstein's predecessors and contemporaries. As we shall see, this work identified certain key problems and went some way to providing the solutions to them. In particular, in 1902 Poincaré (1902; 1905, 75–78 and 118) provided a very valuable criterion for when a theory could be said to be Machian. Moreover, Mach (1883, 1960), Hofmann (1904), and Reissner (1914, 1915) made definite proposals of non-relativistic models of particle mechanics that meet this criterion. The examination of Einstein's entire relativity opus shows that this work made virtually no impact on him. Moreover, there is rather strong evidence which indicates a surprising lack of awareness on Einstein's part of the central problem with which the absolute-relative debate is concerned—*the problem of defining velocity*, i.e., change of position (and, more generally, *change* of any kind). For reasons that can be at least partly under-

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stood, Einstein saw this as a relatively trivial matter and regarded *acceleration* as more problematic.

In fact, Einstein associated with Mach's name two specific problems.

The first may be called the **absolute-space problem**, but it could equally well be called the problem of the *distinguished frames of reference*. Einstein initially presented it as the great mystery of why there seem to exist distinguished frames of reference for the expression of the laws of nature, though later he often spoke of the unacceptability of there being a thing (absolute space) that could influence the behavior of matter without itself being affected by matter.

The second may be called the **inertial-mass problem**. This problem was first mentioned explicitly by Einstein in 1912, when he asserted that Mach had sought to explain the *inertial mass* of bodies through a kind of interaction with all the masses of the universe.

In the years up to the definitive formulation of general relativity in 1915 and a little beyond, Einstein repeatedly mentioned these two problems. However, in 1918, following a critique by Kretschmann (Kretschmann 1917), Einstein (Einstein 1918a) said that he had not hitherto distinguished properly between these two problems (and between the means by which he proposed to resolve them). He then gave a formal definition of what he called *Mach's Principle*, which took the form of the requirement that all the local inertial properties of matter should be completely determined by the distribution of mass-energy throughout the universe. He said that this was "a generalization of Mach's requirement that inertia should be derived from an interaction of bodies." At the same time, Einstein gave a definition of the relativity principle that took from it all the specific empirical content it had previously seemed to possess in Einstein's work and transformed it into a very general necessary condition on the very possibility of stating any laws of nature: "The laws of nature are merely statements about spacetime coincidences; they therefore find their only natural expression in generally covariant equations."

Towards the end of his life, Einstein admitted (not very publicly but explicitly in a letter to Felix Pirani)[1] that his 1918 formulation of Mach's Principle made no sense mathematically and from the physical point of view had been made obsolete by the development of physical notions that had displaced material bodies from the pre-eminence they had possessed in Newtonian theory. However, to the end of his life he retained the 1918 formulation of the relativity principle, which he admitted carried little real physical content. However, he asserted that in conjunction with a requirement of simplicity it possessed great heuristic value, namely that, in a choice between rival theories, preference should be given to those theories that, when expressed in generally covariant form, took a simple and harmonious form.

This faith in *simplicity* as a criterion for selecting physical theories is extremely characteristic of Einstein and gives expression to his deep faith in the ultimate rationality of physics. It is, however, a notoriously slippery criterion. It is also a fact that

1 Einstein to Felix Pirani, 1954 (EA 17-447).

when, in the years up to and including 1916, Einstein said that a satisfactory theory of gravity and inertia must be generally covariant he undoubtedly thought that this requirement had a deep physical significance going far beyond the bland 1918 formulation of the relativity principle.

Mach made the comment that the creators of great theories are seldom the best people to present those theories in a logically concise and consistent form. In this book devoted to alternative strategies that could have been adopted (and in some cases were) to the development of relativity theory, I hope that the following attempt to establish what Einstein was trying to do, actually did, and might have done will help to cast light on the extremely tangled story of the creation of one of the wonders of theoretical physics: the general theory of relativity. In particular, I hope this paper will complement the articles by Jürgen Renn and John Norton[2] (both of which I found very useful in my own work) by looking at Einstein's work closely from the perspective of the specific problem of absolute *vs* relative motion. John Norton has done a splendid technical and conceptual job in comparing Einstein's approach with the more conventional 'Lorentz-invariant field theoretical' approach (to use Norton's useful anachronism) that virtually all his contemporaries adopted to the finding of a relativistic field theory of gravitation. Jürgen Renn, for his part, has emphasized the vital importance of Einstein's more wide-ranging approach and the inclusion of epistemological problems from the foundations of mechanics in the set of issues to be resolved in a satisfactory theory of gravitation. He brings out the value of Einstein's philosophical and integrative outlook. Examination of Einstein's work from the specific absolute *vs* relative perspective brings to light some further issues and aspects of Einstein's work that are not so readily revealed in their approaches.

I hope and believe that nearly all the articles in this book will have not only historical and philosophical interest but also serve a useful purpose for current research. It is widely agreed that the greatest current problem that has to be solved in theoretical physics is that of the relationship between quantum theory and the general theory of relativity. It is my conviction (Barbour 1994, 1995, in preparation) that general relativity is deeply Machian in a sense that unfortunately Einstein never managed to pinpoint accurately and that precisely this very Machian nature of general relativity is the main cause of the difficulties that stand in the way of its quantization. I therefore hope that the present article will have not only historical relevance but also help to clarify some central issues of current research.

In this article, it will not be possible to give a comprehensive account. I aim merely to identify some of the most important issues and ask the reader to consult my forthcoming monograph for a more detailed account. See also the *Notes Added in Proof* at the end of this article.

2 See *The Third Way to General Relativity* and *Einstein, Nordström, and the Early Demise of Scalar, Lorentz Covariant Theories of Gravitation* (both in this volume).

1. THE ORIGIN AND EARLY HISTORY OF THE ABSOLUTE *VS* RELATIVE DEBATE

The whole absolute *vs* relative debate arose from Descartes's claim in his *Principles of Philosophy* (1644) that *motion is relative* (Barbour 1989). Descartes argued that position can only be defined relative to definite reference bodies. Since there is evidently no criterion for choosing certain reference bodies in preference to others, Descartes argued that there can be no unique definition of motion—a given body has as many different motions as there are reference bodies (which, in general, will, of course, be moving relative to each other) with which it can be compared.

Despite this rather cogent argument, Descartes then proceeded, in a manifest *non sequitur*, to formulate definite laws of motion, the first two of which were identical in their content to the law that Newton subsequently adopted as his first law: Any body free of disturbing forces will either remain at rest or move in a straight line with uniform speed. It is evident that such a law presupposes a definite frame of reference—a reference space—and an independent time (an external clock) if it is to make any sense. About this mysterious reference space Descartes said not a word.

We know from Newton's tract *De Gravitatione* (Hall and Hall 1962), written around 1670 but published only in 1962, that Newton was intensely aware of the flagrant contradiction between Descartes's espousal of relativism and the vortex theory, on the one hand, and his anticipation and formulation of the law of inertia, on the other. In a world in which all matter is in ceaseless relative motion (as it is in accordance with Cartesian vortex theory or the atomistic theories so prevalent in the 17th century), Cartesian relativism seems to make it utterly impossible to define a definite motion; in particular, it would appear to be impossible to say that any given body is moving in a straight line. Commenting sarcastically on Descartes's law, Newton said: "That the absurdity of this position may be disclosed in full measure, I say that thence it follows that a moving body has no determinate velocity and no definite line in which it moves." This may truly be called the *fundamental problem of motion*: If all motion is relative and everything in the universe is in motion, how can one ever set up a determinate theory of motion?

The entire story of the absolute *vs* relative debate flows from this dilemma that Newton posed so clearly in around 1670. For completeness, one should also add the temporal part of the story: Motion can never be measured by *time* in the abstract but only by a definite comparison motion. For scientific purposes, the comparison motion was for millennia the rotation of the Earth, though more recently a global network of atomic clocks has been introduced as the official standard of time. Thus, statements in physics involving time are really statements about physical clocks, for which a theory based on first principles is needed (given the fundamental importance of time).

Having formed the deep conviction that no sensible mathematically well-defined dynamics could be based upon Cartesian relativism, Newton insisted on the introduction of a rigidly fixed absolute space and a uniformly flowing external absolute time as the kinematic framework for the definition of motion. However, he was still very conscious of the cogency of Descartes's relativism and in the famous Scholium in the

Principia on absolute and relative motion admitted freely the need to show how absolute motions, which cannot be observed directly ("because the parts of that immovable space, in which those motions are performed, do by no means come under the observation of the senses"), could be deduced from the observed relative motions. This task may be appropriately called the *Scholium problem*: Given observed relative motions, find the corresponding absolute motions. Although Newton actually claimed at the end of the Scholium that he wrote the *Principia* specifically in order to show how that problem is to be solved, he never spelled out the solution explicitly and in the Scholium merely advanced some first qualitative arguments designed to show that absolute space must exist. Even less effort was made to demonstrate the existence of absolute time.

Despite eloquent criticism of the notions of absolute space and time by Newton's contemporaries Huygens, Leibniz, and Berkeley, the absolute *vs* relative problem remained effectively in a state of limbo for very nearly 200 years until it was taken up again by the mathematician Carl Neumann in 1870 (Neumann 1870) and by Ernst Mach in 1872 (Mach 1872, 25; 1911) at the end of an extended essay on the conservation of energy and then again in his famous book on mechanics in 1883 (Mach 1883, 1960). Parallel but less influential work was done in Britain (Scotland to be precise) by William Thomson (later Lord Kelvin) and Tait (Thomson and Tait 1867, §§208ff.; Tait 1883) and also Lord Kelvin's brother James Thomson (Thomson 1883). The interventions of Neumann and Mach brought two issues to the fore.

The first was essentially the Scholium problem: under the assumption that Newton's scheme is in essence correct, how can one make correct epistemological sense of his notions of absolute space and time? Important and significant contributions to the resolution of this problem were made by Neumann (Neumann 1870), Tait (in an unfortunately little noted elegant piece of work (Tait 1883)), Ludwig Lange (Lange 1884, 1885, 1886), the logician Frege (Frege 1891), and above all Poincaré (Poincaré 1898 and 1902; 1905, 75–78 and 118). This work will be considered in Sec. 3.

The second issue brought to the fore was Mach's proposal, made already in 1872 and then repeated (though not quite so clearly or unambiguously as one might wish) in his 1883 *Mechanik* and all its subsequent editions, to the effect that Newton's mechanics might actually be *physically incorrect* and should be replaced by a dynamics of a different form in which only relative separations of bodies occur. The physical cogency of this proposal was made much more impressive by Mach's ability to counter Newton's bucket argument from the undoubted existence of centrifugal force to the need for an absolute space to explain it. Mach observed that the distant masses of the universe rather than some absolute space could be the ultimate origin of the centrifugal forces and that if this were the case local material bodies, such as the wall of Newton's bucket, could be expected to have only a minuscule and unobservable effect.

2. DIRECT ATTEMPTS TO IMPLEMENT MACH'S PROPOSAL AND THEIR LACK OF IMPACT ON EINSTEIN

Although they have attracted very little notice, attempts at a direct implementation of Mach's proposal were made throughout the twentieth century. The first such attempts were made early enough for them to have influenced Einstein in his work on general relativity. In this section, this work and its very marginal impact on Einstein will be considered.

A proposal for a new, non-Newtonian mechanics was already advanced by Mach, in a very tentative and mathematically rather unsatisfactory form, in the *Mechanik* in 1883.[3] His ideas were advanced in several interesting ways by the Friedlaender brothers in a rather obscure booklet published in 1896 (Friedlaender and Friedlaender 1896). In a simple and beautiful example,[4] Benedict Friedlaender showed how distant rotating masses (the 'stars' as seen from someone rotating with Newton's bucket) could very well generate centrifugal forces away from the axis of rotation and thus make absolute space unnecessary. In his contribution to this volume, Renn discusses the various interesting points and also anticipations of Einstein's later work that can be found in the Friedlaenders' booklet.

A rather general way of generating (nonrelativistic) relational theories of the kind envisaged by Mach was found by a certain Wenzel Hofmann of Vienna, who in 1904 (Hofmann 1904) published an even more obscure booklet[5] than the Friedlaenders' which would surely have been lost forever had it not been for fleeting references to it by Mach in the 5th and 6th editions of the *Mechanik* and by Einstein in 1913 (Einstein 1913a). In modern terms, the essence of Hofmann's proposal was to replace the Newtonian kinetic energy T, which occurs in the Lagrange function $T - V$ of the classical mechanics of n point particles and consists of a sum over individual masses of the form

$$1/2\sum m_i\dot{\boldsymbol{r}}_i \cdot \dot{\boldsymbol{r}}_i, i = 1, ..., n, \tag{1}$$

where m_i is the mass of particle i, $\boldsymbol{r}_i$ is its position vector in absolute space, and the dot denotes the time derivative, by a sum over all pairs of the n particles of the form

$$\sum_{i<j} m_i m_j f(r_{ij})\dot{r}_{ij}^2, \tag{2}$$

where r_{ij} is the (Euclidean) separation of particles i and j, $f(r_{ij})$ is some function of this separation, and the dot has the same meaning as in (1).

Hofmann was able to show qualitatively that in a realistic cosmological model, in which there are many stars distributed more or less uniformly over a large area,

3 See (Mach 1960, §VI.7, 286–7) and the discussion of this section by Norton (who questions whether it is a proposal for a new mechanics) and myself in (Barbour and Pfister 1995).

4 Translated in part in (Barbour and Pfister 1995).

5 Mach's proposal reduced essentially to the special case $f = 1$ of Hofmann's general proposal (2).

masses such as those in the solar system would behave in accordance with laws that approximated quite well Newton's laws but in an effective space determined explicitly by the matter distribution in the universe.

Hofmann's idea has since been independently rediscovered many times. The first person to do that was Reissner in 1914 and 1915 (Reissner 1914, 1915), when he chose the particular form $1/r_{ij}$ for $f(r_{ij})$ in (2). This choice is physically plausible and has some remarkably interesting consequences as was shown in part by Reissner himself and also Schrödinger (Schrödinger 1925) in a very beautiful paper at least partly inspired by Reissner's work.

More recently, Bertotti and I (Barbour and Bertotti 1977, 1982) considered a very general framework for constructing relational theories of this kind, including a relational treatment of time. The basic idea is taken straight from Mach. One assumes that dynamics must be formulated for the universe as a whole[6] and, in a variational formulation, insists that only the relative quantities r_{ij} and their rates of change may appear on the Lagrangian that describes the dynamics of the universe. Time is treated relationally by insisting that all changes are measured, not by comparison with some abstract external time t but always by comparison with other actual changes in the universe. This has the effect that Newton's abstract time is replaced by an appropriate average of the totality of changes in the universe.

It turns out that within this large class of possible Machian theories there exist at least two distinct subclasses. One is essentially the class discovered by Hofmann, but it has the disadvantage that it leads to an effective inertial mass that is anisotropic in the presence of nearby accumulations of mass. Schrödinger, in particular, was well aware of this anisotropy and knew that it could lead to an experimental refutation of such theories. He attempted to investigate the effect of the Galaxy and found it to be just below the then existing observational accuracy. He was however using a much too low value for the mass of the Galaxy, and modern data rule out such a theory completely. Such theories are therefore of interest mainly as examples of what Machian theories might look like. In contrast, in the theories of the second class, which Bertotti and I base on a notion called the intrinsic derivative (or *best matching*), mass anisotropy is completely absent. Indeed, one can construct intrinsic models of Machian mechanics that in their locally (but not globally) observable consequences are completely indistinguishable from Newtonian mechanics. I shall return to this briefly at the end of the next section.

The fact that the basic idea of relational mechanics was rediscovered many times[7] indicates that it is a very natural and direct way of realizing Mach's ideas and thereby eliminating absolute motions (and with them absolute space and time) from the foundations of physics. Given Einstein's passionate desire to implement Mach's ideas, it

6 This is implicit in the proposal of Mach and is made explicit by the appearance of the crucial summation in Hofmann's expression (2).

7 Apart from Hofmann, Reissner, and Schrödinger in the early part of this century, at least five other people besides Bertotti and myself hit on the same basic idea in the period 1960–1990, as noted in the articles by myself and Assis in (Barbour and Pfister 1995).

has always seemed to me most surprising that the basic idea—the insistence that only relative quantities should appear in the laws of nature—never seems to have been considered seriously by Einstein. All of Einstein's work on relativity—from 1905 right through to his death in 1955—has a quite different 'flavour.' In fact, it is quite difficult to find evidence that Einstein was even aware of the possibility.

Unless more evidence comes to light in the as yet unpublished correspondence, the only really clear statement of Einstein which does show that he was aware of what might be done along these lines comes from a paper published in 1918 (Einstein 1918b) with the title "Dialogue on objections to the theory of relativity," which includes the following:

> We want to distinguish more clearly between quantities that belong to a physical system as such (are independent of the choice of the coordinate system) and quantities that depend on the coordinate system. Ones initial reaction would be to require that physics should introduce in its laws only the quantities of the first kind. However, it has been found that this approach cannot be realized in practice, as the development of classical mechanics has already clearly shown. One could, for example, think—and this was actually attempted—of introducing in the laws of classical mechanics only the distances of material points from each other instead of coordinates; *a priori* one could expect that in this manner the aim of the theory of relativity should be most readily achieved. However, the scientific development has not confirmed this conjecture. It cannot dispense with coordinate systems and must therefore make use in the coordinates of quantities that cannot be regarded as the results of definable measurements

In the absence of definite references, it is impossible to know for sure whose work Einstein had in mind with his "this was actually attempted" but it is plausible to suppose that he was referring to Mach's original proposal of 1883, Hofmann's 1904 booklet, which he had mentioned briefly in 1913 (Einstein 1913a), describing it as "ingenious," and also perhaps Reissner's two papers.[8] It must also be said that, if he was thinking of the work of Hofmann and Reissner, Einstein had clearly failed to grasp what had been achieved in that work. Both authors had in fact succeeded in finding a genuine alternative to Newtonian inertia governed by absolute space. Moreover, the alleged difficulty to which Einstein refers, that of dispensing with coordinate systems, is simply nonexistent. Both Hofmann and Reissner *did* dispense with coordinate systems in the formulation of their proposed law and worked directly with "only the distances of material points from each other instead of coordinates."

Since these last cited words of Einstein do perfectly encapsulate what Mach had advocated, and since also Einstein repeatedly expressed the greatest admiration for Mach's critique of Newtonian mechanics, his remarks in 1918 present something of a

8 No correspondence from Einstein to Reissner survives. There is one letter from Reissner to Einstein in the Einstein Archives. It dates from 1915 but concerns Reissner's work on general relativity. Reissner makes no mention of his Machian papers. In September 1925, Einstein (Einstein to Schrödinger, September 26, 1925 (EA 22-003) thanked Schrödinger for sending him a copy of his 1925 paper on the relativity principle. Einstein merely said it was "interesting." Had the work of Hofmann and Reissner truly made any impact on him, one might have expected Einstein to point out to Schrödinger that his work had been anticipated by them.

puzzle, as I noted a little earlier: Why did Einstein take so little interest in a serious and direct attempt to implement Mach's proposal? To this query one may add the observation that Einstein's frequent references to Mach in his papers in the period 1912 to 1923 seldom reflect accurately what Mach actually said and sometimes even represent a serious distortion. The most serious distortion concerns a straight confusion between two quite distinct meanings of the word *inertia*. It is worth saying something about this.

Both in Mach's time and now, the word inertia meant two things: first, as expressed in Newton's first law, the law of inertia, namely the tendency of a body to continue in rest or in uniform motion in a straight line unless acted upon by some force; second, the quantitative measure of resistance to acceleration as expressed by the presence of m, the *inertial mass*, in Newton's second law $\boldsymbol{F} = m\boldsymbol{a}$. Mach (Mach 1872, 25; 1883) pointed out that Newton had failed to give a meaningful definition of inertial mass and proceeded to supply one himself. He believed that his definition removed all difficulty surrounding the use of the concept of inertial mass in Newtonian dynamics. In contrast, he felt that Newton's formulation of the law of inertia was very seriously deficient and probably incapable of being given adequate expression without some actual change in its physical content. Mach insisted that genuine content must be given to expressions like "uniform motion in a straight line": uniform with respect to what and straight with respect to what? He considered it absolutely impermissible to invoke invisible time and space to answer these questions, and his discussion of these issues takes us straight back to the problems with which Newton grappled in *De Gravitatione*.

Very careful examination of *all* of Einstein's numerous comments on issues related to Mach have led me to a very surprising conclusion. Einstein *never once* even mentioned this problem—the fundamental problem of motion—at the heart of dynamics. He seems to have been more or less completely blind to its existence. He very often used the word inertia but never once made the distinction between the two meanings of it. When he was most explicit about Mach and inertia, he incorrectly attributed to Mach the idea that the *inertial mass* should arise in some manner from a kind of interaction of all the bodies in the universe (Einstein 1912, 1917). Now it is true that the m_i's that appear in Hofmann's proposal (2) are best interpreted as inertial *charges*. In the theory to which (2) and other similar proposals give rise, one then obtains effective *inertial masses*, which are indeed determined by interaction with all the bodies in the universe. This was clearly demonstrated by both Reissner and Schrödinger, but it was already qualitatively clear to Hofmann.

Einstein may very well have had a correct intuitive appreciation that some such effect could come out of a Machian theory of motion, but his repeated assertions that this was what Mach had called for are unfortunate on several counts: 1) They are historically inaccurate. 2) The effect arises in a certain class of Machian theories—the class considered by Hofmann, Reissner and Schrödinger—but not in another, which Bertotti and I discovered (Barbour and Bertotti 1982). This second class of theories is impeccably Machian and actually includes general relativity as a special and remarkably interesting example (Barbour 1995, see also the *Notes Added in Proof*). 3) Einstein's concentration on the inertial mass deflects attention away from the true and profound problem that underlies the absolute *vs* relative debate: How are time and motion to be defined?

This is the fundamental question that, very surprisingly, Einstein never addressed directly. In the final section of this paper, I shall try to establish why this was so. However, before then, in the following section, I want to complete the review of the work of Einstein's predecessors and contemporaries. As noted earlier, the critique of Neumann and Mach raised two issues: 1) Can Newtonian theory be recast in an epistemologically satisfactory manner without change of its essential physical content? 2) Can Newtonian theory be replaced by a physically different theory based on Machian ideas?

This section has essentially considered the answer to the second question. In the next section, we shall consider the answer to the first.

3. THE EPISTEMOLOGICAL WORK OF NEUMANN, LANGE, AND POINCARÉ AND ITS IMPACT ON EINSTEIN

In his habilitation lecture of 1870, Neumann posed a general problem and provided a partial solution to a small part of it. The general problem was this: As formulated by Newton, the laws of mechanics simply cannot be tested because absolute space and time are invisible and inaccessible to experimentalists. The question then was: Is it nevertheless possible to make epistemological sense of Newton's laws by identifying operational surrogates of absolute space and time?

To begin to make progress in this direction, Neumann assumed that particles moving freely of all forces (force-free particles) exist and could be identified as such and that also by some means absolute space (or a suitable surrogate of it) could be observed directly. If the second assumption is satisfied, one can then observe the motion of some chosen force-free particle. Neumann pointed out that, in the absence of an external clock, it is meaningless to say that such a particle is moving uniformly (though, if absolute space has been 'made visible', one can verify that it is moving in a straight line). However, what one can do is observe further force-free particles and see how they behave relative to the original particle, which is taken as a reference body. One can use the distance traversed by this reference body as a measure of time (inertial clock) and see if, relative to this inertial clock, a second force-free body moves uniformly. In this way, Neumann was able to give genuine operational content to the part of Newton's first law which asserts the uniformity of the motion of a force-free body. However, Neumann admitted that he was unable to solve the problem of making absolute space 'visible.'

This problem was taken up by the youthful Ludwig Lange (he was only 21) in 1884. He proceeded very much in the spirit of Neumann and assumed the existence of force-free particles that could be identified as such. His basic idea was to use *three* such particles to define a spatial frame of reference. Just as in the case of Neumann's inertial clock, for which it is meaningless to say that the clock itself is moving uniformly, Lange noted that it would be meaningless to say that his three reference bodies are moving rectilinearly. Instead, they *define* a frame of reference, with respect to which one can then verify that other bodies are moving rectilinearly. Moreover, using any one of the three chosen reference bodies as a Neumann inertial clock, one can simultaneously verify that further bodies are moving uniformly as well as rectilinearly.

Lange's actual construction of the spatial frame of reference using three force-free bodies is in fact rather awkward and clumsy, so I shall not attempt to describe it here, especially since I shall shortly describe a much neater construction due to Tait (Tait 1883). However, it is worth emphasizing the crucial point of the construction, which Lange was the first to recognize clearly and for which he deserves great credit. It will be recalled that Newton criticized Cartesian relativism because it made the motion of a considered body dependent on the choice of the reference bodies used to determine its motion. Since the choice of reference bodies is entirely arbitrary, it would appear that motion itself cannot be defined in any unique way. However, the situation is radically altered if one insists that the reference bodies—no matter which are chosen—*are themselves moving in accordance with Newton's laws*. This is the crucial stipulation that takes the seemingly fatal arbitrariness out of a relational definition of motion. Once this basic fact has been recognized, precise definitions merely reduce to a working out of details.

One severe problem with the Neumann-Lange approach—Lange never succeeded in overcoming it—was that of recognizing when bodies are free of forces. The construction depends crucially on the existence of unambiguously identifiable force-free bodies. This raises *two* problems: 1) How can one tell if a body is free of forces? 2) What can one do if nature fails to provide *any* force-free bodies? In fact, this is exactly the case with gravity, to which all bodies are subject. These serious difficulties were pointed out clearly by the logician Frege (Frege 1891) in an otherwise positive review of Lange's work. Frege correctly emphasized that the axioms of dynamics form a closed system and can only be tested in their totality. Since forces are an integral part of dynamics, their existence must be taken into account in the foundations of any method used to determine the distinguished frames of reference that play such an important role in Newtonian dynamics.

As it happens, the requirement that Frege raised was (in its essentials) met in three studies that unfortunately received very little attention. The first was actually the work of Tait in 1883 that I already mentioned. The other two were published in 1898 and 1902 by Poincaré.

Tait did not solve the problem of finding the dynamical frame of reference in full generality in the case when no force-free bodies are available. He did, however, give a solution to the problem for purely inertial motion that yields the Newtonian frames of reference given purely relative data and simultaneously confirms that all the considered bodies are actually free of all forces.

Tait solved the following problem, which had been posed by James Thomson (Thomson 1883). Suppose that at certain unknown instants of time we are given all the relative separations r_{ij} between a set of n point particles. Thus, we are, as it were, given 'snapshots' of the relative configurations of the particles. Using these snapshots and nothing else, can we verify if there exist a frame of reference and a measure of time, both of which must be deduced from the snapshots, in which all the particles are moving in accordance with Newton's first law?

To solve this problem, Tait supposed that the answer is yes. I shall consider the solution he gave for the case of three particles, since it fully illustrates the underlying principle. If all the particles are moving in accordance with Newton's first law, then one can certainly always choose the frame of reference in such a way that one of the particles is permanently at rest at the origin of the frame. If we exclude the special cases in which there are collisions of the particles, then if we consider some second particle there must exist a time at which it passes the first one at a distance a of closest approach. We can then choose x and y axes of the frame of reference in such a way that at $t = 0$ this second particle is at the point $(a, 0, 0)$ and at time t is at the point $(a, t, 0)$. Thus, we choose the unit of time such that particle 2 has unit velocity. It becomes a Neumann inertial clock. The spatiotemporal framework is then uniquely defined (up to reflections). At $t = 0$, the third particle will have some initial position (x_3, y_3, z_3) and initial velocity $(\dot{x}_3, \dot{y}_3, \dot{z}_3)$ Thus, this three-body problem will have *seven* essential unknowns. The problem of inertial motion is more or less trivial and one can find an analytical solution for the observable separations r_{ij} in terms of these seven unknowns. Given observed values of r_{ij}, these can be compared with the analytical solution and the seven unknowns determined.

As Tait noted, the most interesting point concerns the number of snapshots needed to find the seven unknowns. Each snapshot yields three independent data—the three sides of the triangle—but each snapshot is taken at an unknown time, so that only *two* useful data are supplied with each. It is thus clear that to determine the spatiotemporal framework and test whether all three particles are moving inertially in accordance with Newton's first law one needs at least four snapshots, since they give eight data, from which the seven unknowns can be determined and one verification made of the conjecture. Each extra snapshot yields a further two verifications.

Several important points emerge from Tait's analysis. First, contrary to a very widespread opinion engendered by Lange's work, three particles are already sufficient to establish the spatiotemporal framework and to test whether Newton's first law is satisfied. Lange, and many of his followers, believed three particles were needed to define the framework and that only a fourth would permit a nontrivial verification of Newton's law. Second, attention should be drawn to the central importance of the complete configurations of the three particles, which, in a sense, *define* the instants of time, and to the fact that both time and the spatial reference frame are best and mostly effectively determined together from the raw observational data—the relative separations. Third, knowledge of the spatial frame of reference is a vital prerequisite for determination of all quantities of primary concern in dynamics, above all time, which in the Tait procedure is read off from distance traversed in the spatial inertial frame of reference, and velocity and momentum, both of which can only be found once the complete spatiotemporal framework has been determined.

Two further points should be made here. In analytical mechanics, great emphasis is placed on the possibility of representing dynamics in completely arbitrary frames of reference. However, this does not alter the fact that somehow or other the primary dynamical quantities such as momentum and energy must be found in an inertial spatiotemporal framework. It is only then that a transformation to an arbitrary framework can be performed. Many people, even experts, are quite unaware of this fact. The second remark concerns the definition of a clock. It is widely believed that the essential basis of a clock is a strictly periodic process, the 'ticks' of which measure time. This belief is wrong on *two* scores. First, the Neumann-Lange-Tait procedure shows that linear distance traversed in an inertial frame of reference by a force-free particle is a perfectly good measure of time. Thus, a periodic process is not needed. Second, the inertial frame of reference and distance traversed in it are (in mechanics at least) always the ultimate source of a scientifically meaningful definition of time. Ironically, a pendulum clock, the rate of which depends upon the strength of the gravitational field in which it is set up, is not really a good clock, since its rate is not exclusively determined by its local inertial frame of reference. Thus, a pendulum clock goes faster near sea level than on the top of a mountain, but (as Einstein's general theory of relativity established) clocks that measure proper time go slower at sea level. This highlights the salient point: A clock, to function properly, must 'lock onto' or 'tap' processes directly and exclusively governed by the local inertial frame of reference.

We still have to consider the realistic general case in which no force-free particles are available at all. How is the inertial spatiotemporal framework to be determined in that case? As preparation to the answer to this question, it is worth noting that in the case of Tait's problem in the general case of n point particles, the number of unknowns to be determined is $1 + 6(n-2) = 6n-11$ (giving our 7 for $n = 3$). On the other hand, each snapshot of n particles yields $3n-6$ independent mutual separations or $3n-7$ useful bits of information (since the time of the snapshot is unknown). Thus, two snapshots can only yield $6n-14$ data, while $6n-11$ are needed to determine the inertial spatiotemporal framework and, from it, the dynamically relevant quantities. Two snapshots are therefore never enough information but, if n is large, three are comfortably more than enough. The reason why two snapshots always fail to yield enough information is that, in Newtonian terms, they contain no data at all on the change of the *orientation* of the system of n particles as a whole in absolute space.

This fundamental fact was made the point of departure of a very interesting analysis of the problem of absolute *vs* relative motion made by Poincaré in his *La Science et l'Hypothèse* in 1902 (Poincaré 1902; 1905, 75–78 and 118). Before considering this, it is worth mentioning that unfortunately Poincaré never, so far as I know, published a single comprehensive study of the problem of determining the complete spatiotemporal framework of dynamics from observable relative quantities. He considered the temporal and spatial problems separately (the former in his "*Mèsure* *du temps*" in (Poincaré 1898) and the latter in 1902). Both studies were rather qualitative in nature, and both attracted much less attention than they might otherwise have done on account of the creation in 1905 of the special theory of relativity. This then attracted most of the serious attention of scientists concerned with foundational problems and also introduced a host of new issues. This was unfortunate, since a solid authoritative study by Poincaré, of which he was undoubtedly capable, would have become an important landmark in the absolute *vs* relative debate. As it is, his work has very largely passed unnoticed (in part, at least, because Einstein did not notice it, as we shall see).

In his *La Science et l'Hypothèse*, Poincaré asked what if anything was 'wrong' with Newton's use of absolute rather than relative quantities in the foundations of dynamics. Instead of asking the epistemological question—how do we find the absolute quantities given the relative quantities?—Poincaré posed a very interesting question, which was this: If, in the case of the n- body problem of celestial dynamics, one has access to only *relational* initial data (which will be the mutual separations r_{ij} of bodies and their various derivatives $\dot{r}_{ij}, \ddot{r}_{ij}, \ldots,$ with respect to the time t (Poincaré assumed t known for the purposes of his discussion)), what *initial data* must be specified if one is to be able to predict the observable future evolution of the system uniquely? Since the ability to predict the future is the acid test of dynamical theory, Poincaré's question could not be better designed to cast much needed light on the role of absolute and relative quantities in dynamics.

Poincaré then noted that if, like the relationists, one believed the relative quantities were truly fundamental and all that counted, one might then suppose that (given known masses of the bodies and under the assumption that they were moving in accordance with Newton's laws, including the law of universal gravitation) knowledge of the r_{ij} at one instant together with the rates of change of these r_{ij}, i.e., the $\dot{r}_{ij}$'s, would be sufficient to determine the future uniquely. However, he then drew attention to the fact with which we are already familiar from Tait's analysis of the inertial case, namely, that even in that simplest of cases two snapshots are not sufficient to determine the absolute quantities, which, as Poincaré pointed out, are needed to make dynamical calculations. (The initial-value problem of celestial mechanics is well posed if, in addition to the masses and specification of the law of interaction, one is given initial positions and initial velocities in *absolute space*.) The situation is no different if interactions occur. In Poincaré's view, this failure of the initial-value problem if one is given only relative quantities is the clearest indication that dynamics involves something more than just relations of bodies among themselves—and that 'something more' is what Newton called absolute space.

It is important to realize, as Poincaré was careful to emphasize, that it is perfectly possible to express the entire content of Newtonian mechanics in purely relational terms. However, the resulting equations, unlike Newton's equations, which contain at the highest *second* derivatives with respect to the time, must contain at least some *third* derivatives. Although he did not explicitly mention him by name, Poincaré almost certainly had in mind here Lagrange's famous study of the three-body problem of celestial mechanics made in 1772 (Lagrange 1772). Lagrange (1772) had assumed the validity of Newton's equations in absolute space and, in an outstanding piece of work, had then proceeded to find equations that govern the variation in time of the *sides of the triangle* formed by the three particles, i.e., precisely the r_{ij}'s for this problem. Lagrange had found three equations, each containing the r_{ij}'s and their derivatives symmetrically and all containing first, second, and *third* derivatives of the r_{ij} with respect to the time. He was also able to show that two of the equations could be integrated once, giving two equations of the form

$$F_1(r_{ij}, \dot{r}_{ij}, \ddot{r}_{ij}) = E, \tag{3}$$

$$F_2(r_{ij}, \dot{r}_{ij}, \ddot{r}_{ij}) = M^2, \tag{4}$$

where E is the total energy of the system and M^2 is the square of the total angular momentum of the system (both in the center-of-mass system). These equations show very graphically that whereas the fundamental dynamical quantities such as energy and angular momentum are functions of the coordinates and their *first* time derivatives *in absolute space*, the expressions for the same quantities in relative quantities also necessarily contain the *second* derivatives.

Poincaré considered this a decidedly mysterious and unsatisfying feature of Newtonian mechanics and felt that it was the only thing one could fault in the Newtonian scheme. He felt, repugnant though this state of affairs was to a philosophically minded person, that one still had to accept it as a fact. He was however prepared to speculate as to how things might be in an ideal world, and this led him to a very interesting speculation as to the form that the relativity principle might have taken.

He noted that the ordinary Galilean relativity principle of classical mechanics had very interesting consequences for the initial data that had to be specified in mechanics. An n-particle system requires formally the specification of $3n$ initial positions and $3n$ initial velocities in absolute space. However, because of the fundamental symmetries of classical mechanics, it is sufficient to specify these quantities with respect to the center of mass of the system. This reduces the number of data that need to be given by 6. In addition, the initial orientation of the system in absolute space has no physical significance, so three more data are redundant. However, essentially that is as far as the reduction to relative quantities can go. It remains crucially important to know at the initial instant how the orientation of the system as a whole in absolute space is changing. This cannot be obtained from purely relative quantities and is the reason why third derivatives of the r_{ij} occur in one of Lagrange's equations.

Such considerations then led Poincaré to comment that "for the mind to be fully satisfied" the law of relativity would have to be formulated in such a way that the initial-value problem of dynamics would hold for a *completely relational* specification of the initial data. One should not be left with the curious absolute-relative mixture just described.

This analysis and suggestion of Poincaré are both extremely valuable. They show that the problem with Newtonian dynamics is not that it cannot be cast into relational form—Lagrange's work is the clearest demonstration of the incorrectness of that belief (which is actually quite widely held).[9] The problem is that when Newtonian theory is recast in a relational (or generally covariant) form it turns out to be *less predictive* than one would like it to be. In addition, Poincaré's analysis also shows what a Machian theory, expressed solely in relative quantities as Mach required, must achieve if it is to represent any improvement on Newtonian theory: It must be able to predict the future uniquely given only r_{ij} and $\dot{r}_{ij}$ at an initial instant. Mach's critique of Newtonian mechanics was unfortunately couched in rather vague terms and the same goes for his proposal for a relational alternative. Poincaré's analysis provides a most welcome clarification and sharpening of the issues involved.

It should be mentioned that all the Machian models of the Hofmann-Reissner-Schrödinger type together with the alternative (intrinsic) type considered by Bertotti and myself meet the requirement of the relativity principle in the stronger form as formulated by Poincaré. It is also the case that the special set of *Newtonian* solutions of an n-body universe for which the total angular momentum in the center-of-mass system vanishes are described by equations of a form different from those that hold in the general case. In this special case, the constants E and M^2 disappear from the right-hand sides of Eqs. (3) and (4) and the third derivative also disappears from Lagrange's third equation. Therefore, the corresponding set of equations for this special case satisfy Poincaré's requirement. Indeed, it is a very interesting fact that when Newton's equations are expressed in a generally covariant form (as Lagrange in effect did, using quantities completely independent of all coordinate systems), the complete set of possible solutions breaks up into distinct classes corresponding to the general case with both $E, M^2 \neq 0$ and the various special cases with either one or both of E and M^2 equal to zero. The most interesting special case

$$E = 0, M^2 = 0 \tag{5}$$

arises very naturally from the intrinsic Machian dynamics developed by Bertotti and myself and referred to in the previous section.

In fact, such a situation was foreseen to quite an extent by Poincaré, who pointed out that, when one is considering the complete universe, it is appropriate to consider

9 Lagrange's work does in fact represent the complete solution (for the three-body case) of the problem that Newton posed in the Scholium: Given relative observations, how can one find the absolute quantities? First, Lagrange found equations that govern the evolution of the sides of the triangle. Second, he showed how, once these equations for the sides of the triangle had been solved, one could find the position of the triangle in absolute space (the position of its center of mass and—a much greater problem—its orientation) by quadrature (i.e., by straightforward integration of functions known from the solution of the problem for the sides). A good account of all this is given by Dziobek (Dziobek 1888, 1892). It is somewhat ironic that Lagrange was evidently much more interested in practical problems of celestial mechanics than Newton's Scholium problem and did his work at a time when absolute space had ceased to be a problematic issue. Its importance for the Scholium problem was not noted and escaped Neumann, Lange, and Mach. It is truly a great pity that Poincaré did not flesh out his very perceptive remarks in *La Science et l'Hypothèse* and draw explicit attention to Lagrange's work and its bearing on the Scholium problem.

these various different cases as actually corresponding to fundamentally different dynamical laws of the universe. An important point to note is that if an n-body universe as a whole does satisfy the condition (5) isolated subsystems of it can still perfectly well have nonvanishing values of their energy and angular momentum. They would then appear to be governed by perfectly standard Newtonian dynamics, even though the universe as a whole is governed by a more powerful and more predictive dynamics. This is the reason why Bertotti and I were able to recover Newtonian behavior exactly for local observations. It may also be mentioned that the formalism of intrinsic dynamics is completely general and is not restricted to nonrelativistic mechanics. Unlike the Hofmann-Reissner-Schrödinger approach, it can readily be applied to field theory and even to dynamic geometry. Indeed, it turns out that general relativity is itself of the general type of intrinsic theories, and this is the reason why Bertotti and I have concluded that it is actually perfectly Machian (Barbour 1995).

Let me now go back to Poincaré's earlier paper of 1898 on the topic of time. This paper has received significantly more attention than the analysis of the absolute *vs* relative question in *La Science et l'Hypothèse*, but its Machian implications have nevertheless been completely missed.

Poincaré noted that in recent years there had been considerable discussion of the problem of measuring time. What does it mean to say that a second today has the same *duration* as a second tomorrow? What criterion is to be used to choose the unit of time and identify clocks? Poincaré noted that these questions had become especially topical and acute for the astronomers, who had been finding anomalies in the observed motion of the Moon, one possible explanation of which could be irregularities in the rotation rate of the Earth. (This has since been confirmed. It is due to tidal effects of the Moon.) Since for millennia the rotation of the Earth had constituted the sole reliable clock for use in astronomy, this placed the astronomers in a serious quandary.

Poincaré then proceeded to outline the solution to which the astronomers were moving. Their point of departure was that Newtonian theory was in fact correct, namely, that there did exist a frame of reference and time for which Newton's laws were correct. The entire problem consisted of finding the invisible frame of reference and time from things that could actually be observed. The only material on which they could work was the motions of the bodies making up the solar system. Fortunately, this could, on account of the immense distance of the stars, be treated as an effectively isolated dynamical system. However, in contrast to the gedanken experiments considered by Lange and Tait, the bodies of the solar system were certainly not free of forces, since they all interacted with one another through universal gravitation. The astronomers were therefore confronted with the task that Frege a few years earlier had said needed to be solved by Lange.

The solution proposed by the astronomers, and endorsed in principle by Poincaré, was to seek a frame of reference and time in such a way that the observed motions did indeed accord with Newton's laws when referred to the obtained frame of reference and time. This is a rather obvious generalization of the method initiated by Neumann, Lange, and Tait, but, of course, entailed much greater mathematical difficulties on account of the need to take into account interactions. Fortunately for the astronomers, they did not have to start completely from scratch, since excellent approximations to the conjectured Newtonian frame of reference and time already existed.

A very significant difference of this astronomical procedure from the Tait-Lange procedure is that in the latter time and the frame of reference can in principle be found from just *three* bodies, but the astronomical procedure entails consideration of *all* the dynamically significant bodies in the solar system. If accuracy adequate for astronomical purposes is to be achieved, it is in principle necessary to take into account even relatively small asteroids. This means that effectively the only clock available to the astronomers is the complete solar system.

About forty years after Poincaré wrote his 1898 paper, the astronomers did indeed go over to such a definition of time (which by then had to take into account small relativistic corrections as well). It was initially called *Newtonian* time, but is now known as *ephemeris time* (Clemence 1957). A rather beautiful feature of ephemeris time is that it is actually a weighted average of all the dynamically significant motions of the bodies in the solar system in its center-of-mass inertial frame. Were the solar system to consist of a system of point particles, the expression for the infinitesimal increment of ephemeris time would be given as follows. Let the position of particle $i, i = 1, \ldots, N$, at one instant of time be given by x_i and at a slightly later instant by $x + dx_i$, the positions being measured in the inertial frame of reference. Then the increment dt of ephemeris time is given by

$$dt = \frac{k\sqrt{\sum m_i dx_i \cdot dx_i}}{\sqrt{E - V}},$$

where E is the total energy of the system, V is the instantaneous potential energy, and k is a constant.

Note also that but for the fortunate fact that the solar system is almost perfectly isolated an accurate determination of time would require the summation in the above expression to be extended to the complete universe. Ultimately, the only reliable clock is the complete universe!

I have gone into this detail about ephemeris time (the theory of which was outlined rather more sketchily by Poincaré in his 1898 paper) because, first, it rectifies the shortcoming of Newton's treatment in the Scholium, and, second, it has passed almost without notice for over a century. This remarkable state of affairs has arisen because a quite different aspect of time—the problem of defining simultaneity at spatially separated points—came to dominate discussions once Einstein had created the special theory of relativity.

As it happens, Poincaré also mentioned this problem of simultaneity in his 1898 paper and noted that in some respects it was a more immediate problem than that of defining duration but that hitherto it had hardly been noted. It is on account of this remarkable early anticipation of the key problem of special relativity that Poincaré's

1898 paper is mentioned relatively often today, but I am not aware of any discussion of the duration problem even though it is certainly very fundamental.

The reason for this lack of notice is, I suspect, to be traced to the immense influence of Einstein, and this is an appropriate point at which to consider how his own work on special and, more particularly, general relativity relates to the topics discussed in this and the previous section. At the end of the previous section, I noted that Einstein seems to have had not much accurate knowledge of the work done by Hofmann and Reissner and to have taken little interest in it. The same comment is true of the epistemological work reported in this section. So far as I can judge from his published papers and the correspondence I have examined, *none* of the work described in this section made any significant impact on him. In the remainder of this section, I shall substantiate this claim; in the following section, I shall try to establish why Einstein seems to have been remarkable insensitive to what might be called the classical issues in the absolute *vs* relative debate.

Let me start with the topic last discussed—the definition of duration and a clock. To the best of my knowledge, this question was never once discussed by Einstein (in striking contrast to his numerous discussions of the definition of simultaneity). Throughout his entire work on relativity, Einstein simply assumed, as a phenomenological fact, that clocks (like rods to measure distance) exist and can be used to measure the fundamental interval ds of relativity theory.

Already in the 1920s (Einstein 1923) and then again in the *Autobiographical Notes* (Einstein 1949) written towards the end of his life, Einstein noted that his consistently phenomenological treatment of rods and clocks, which made it necessary to introduce them formally as separate entities in the framework of his theory, was a logical defect of the theory that ought to be eliminated. Rods and clocks should be constructed explicitly from the truly fundamental physical quantities in the theory—preferably fields alone, but, if particles could not be eliminated as fundamental entities, then from fields and particles together.

From the way Einstein wrote about this, I get the strong impression that he did not think anything particularly interesting would come out of this exercise. However, I think it can be argued that he was actually insensitive to a fundamental issue. This is reflected in the fact that he invariably described a clock as being realized through some strictly periodic process. However, this immediately begs the question that Neumann set out to answer with his inertial clock: How can one say of a single motion that it is uniform? I have not seen anything in Einstein's writings which shows an awareness of the fact that a measure of time can be extracted only from the totality of the motions within a dynamically isolated system and that, if it is to give true readings, a clock must somehow 'lock onto' and reflect the inertial spatiotemporal framework. I shall return to this.

A similar rather perfunctory attitude characterizes Einstein's references to the determination of inertial frames of reference. In his published papers, he never once referred to the procedures of Lange or Tait or drew attention to the difficulties that Newton 250 years earlier had already recognized so clearly. Generally, he simply says that an inertial frame of reference is one in which a force-free particle moves rectilinearly and uniformly, giving no indication at all how such a frame of reference is to be found. Very characteristic of his approach is the following passage written in the early 1920s (Einstein 1923):

> In classical mechanics, an inertial system and time are best determined together by means of a suitable formulation of the law of inertia: It is possible to establish a time and give the coordinate system a state of motion (inertial system) such that relative to it material points not subject to the action of forces do not undergo acceleration.

A little later, Einstein noted that such a definition had a logical weakness "since we have no criterion to establish whether a material point is free of forces or not; therefore the concept of an 'inertial system' remains to a certain degree problematic" This passage (with its incorrect conclusion) suggests to me that Einstein never gave much serious thought to the issue of the determination of inertial frames of reference.

Confirmation that this is the case can be found in some remarkably interesting late correspondence between Einstein and his old friend Max von Laue. Among the leading relativists, von Laue is the only one who mentions the work of Lange. In 1948 (von Laue 1948), he wrote an appreciation of Lange and his work, in which he stated: "Ludwig Lange progressed so far in the solution of the problem of the physical frame of reference, which Copernicus, Kepler, and Newton did not completely solve, that only Einstein's theory of relativity added something new." In 1951, he published a new edition of his book on the theory of relativity (von Laue 1955), which opens with the definition of the inertial time scale and inertial system as given by Lange, calling it a great achievement. Not surprisingly, he sent Einstein a copy of the new edition. In response,[10] Einstein commented:

> I was surprised that you find Lange's treatment of the inertial system significant. It merely says that there exists a coordinate system (with time) in which 'uninfluenced' material points move rectilinearly and uniformly. This is Newton's '*absolute* space.' It is not absolute because no transformations exist that conserve the law of inertia but because it must be prescribed in order to give the concept of acceleration a clear meaning.

In the same letter, Einstein remarked: "Provided one considers action-at-a-distance forces that decrease with r sufficiently rapidly, the word 'uninfluenced' has a direct meaning." This comment implies, like the one made in the 1920s, that inertial frames of reference can only be determined if force-free bodies are available. As we have noted earlier, this is simply not true, though unfortunately the correct state of affairs had never been clearly stated in the literature (see footnote 9). However, I am convinced that had Einstein really made a serious effect to find out the truth he would certainly have succeeded. What we must try to establish (in the next section) is why he was insensitive to the issue.

To conclude this section, it is worth mentioning a connection between Einstein's lack of concern about the definition of the inertial frame of reference and his belief

10 Einstein to Max von Laue, 17 January 1952 (EA 16–168).

that a Machian theory of motion should provide some kind of cosmic derivation of inertial mass (rather than a cosmic derivation of the law of inertia). It is a very striking fact that the expressions 'relativity of position' and especially 'relativity of velocity' (the truly fundamental problem of the absolute *vs* relative question) hardly ever occur in Einstein's writings, whereas he frequently mentions the relativity of acceleration. In fact, almost the only case in which relativity of velocity occurs is in the following passage (Einstein 1913b), in which Einstein is discussing his first attempt at a general theory of relativity undertaken with Grossmann in 1913:

> The theory sketched here overcomes an epistemological defect that attaches not only to the original theory of relativity, but also to Galilean mechanics, and that was especially stressed by E. Mach. It is obvious that one cannot ascribe an absolute meaning to the concept of acceleration of a material point, no more so than one can ascribe it to the concept of velocity. Acceleration can only be defined as relative acceleration of a point with respect to other bodies. This circumstance makes it seem senseless to simply ascribe to a body a resistance to an acceleration (inertial resistance of the body in the sense of classical mechanics); instead, it will have to be demanded that the occurrence of an inertial resistance be linked to the relative acceleration of the body under consideration with respect to other bodies. It must be demanded that the inertial resistance of a body could be increased by having unaccelerated inertial masses arranged in its vicinity; and this increase of the inertial resistance must disappear again if these masses accelerate along with the body.

Einstein then proceeds to claim that the 1913 theory does indeed contain an effect of the desired kind.

The above passage is remarkable on two scores. First, there is the already noted incorrect claim that Mach was concerned about the definition of inertial resistance. Second, Einstein states that both velocity and acceleration are relative and presents this as a major problem. However, he never once in his papers attempted to show how general relativity attacked the fundamental kinematic problem of the relativity of velocity. The idea that inertial resistance, like acceleration, must be relative, is expressed very prominently in Einstein's writings from 1912 through to about 1922. However, Einstein never once attempted to show how such an idea (and still less the even more fundamental relativity of motion alluded to above) was implemented in the basic kinematic and dynamic structure of the theory he was constructing.

This is in very striking contrast to the epistemological work of Neumann, Tait, Lange, and Poincaré and the manifestly relational proposals of Hofmann and Reissner. All of these authors attacked the relativity of motion head on. What are the reasons for Einstein's conspicuous failure to follow their example?

4. EINSTEIN'S PRIORITIES WHEN CREATING GENERAL RELATIVITY

Let me now attempt to begin to answer the question with which the previous section ended by considering the evidence that can be gleaned from Einstein's early papers and correspondence. It is quite clear that by the time he had left school and commenced university studies Einstein had set himself a supremely ambitious task. He was going to attack and make an extremely serious attempt to solve the great topical problems of physics. In later years, he may have liked to cultivate the image of a somewhat indolent student, but a very different picture emerges from his correspondence. There were certain fundamental issues that he followed avidly, above all anything related to Maxwellian field theory and also anything that could provide evidence for the existence of atoms. These were the burning topics of the time, and he followed them closely.

It seems to me that with regard to the absolute *vs* relative debate, the situation was somewhat different. There is no doubt that it was a topic of genuine widespread interest; Poincaré's inclusion of it in *La Science et l'Hypothèse* is clear evidence of that. However, it was a topic with relatively few (but by no means none at all) opportunities for decisive experimental tests;[11] both Mach and Poincaré tended to treat the topic in a rather passive manner, drawing attention to problems but without proposing an energetic programme for their resolution. For an ambitious young man like Einstein, with a strong awareness of the importance of experiment and clearly determined to make a name for himself as quickly as he could, the problems of electromagnetism and atomism must surely have appeared to offer far better prospects. This could well explain why Einstein's imagination was clearly caught, through his reading of Mach's *Mechanik* around 1898 (CPAE 1), by the great issue of absolute space without this leading him on to a more detailed consideration of the details. Whatever the reason, in the period 1898 to 1905 (and, indeed, up to the end of his life) Einstein had the opportunity to go into the details and really come to grips with the central problems of defining time, clocks, and motion. He did not or, at least, not directly (except, of course, with regard to simultaneity).

There are, I believe, at least three clearly identifiable reasons for Einstein's *indirect* attack on the problem of absolute space. All three are important and interrelated and already played a decisive role in his creation of the *special* theory of relativity.

The first, and surely the most important, is that the principle of Galilean relativity suggested to Einstein an indirect but extremely effective way of making absolute space redundant in physics. He saw the success of special relativity as an important first step in that direction and then attempted, with great consistency, to generalize the relativity principle to the maximum extent possible. He believed that this would make absolute space completely redundant as a concept in physics.

The second reason for Einstein's indirect strategy is to be found in the phenomenological concept of the rigid body and the important work done by Helmholtz on the empirical foundations of geometry. The phenomenological rigid body played a vitally important role in both special and general relativity but, as we shall see, made it extremely difficult to address directly the relativity of motion in any obviously Machian manner.

The third reason for Einstein's indirect approach may seem somewhat surprising at the first glance—it was Planck's discovery of the quantum of action in 1900. We shall see that this discovery greatly diminished Einstein's confidence in the possibil-

11 For a discussion of early experiments, see (Norton 1995).

ity of finding quickly any explicit and detailed dynamical equations that could be taken to describe the behavior of particles and fields at the fundamental microscopic level. Instead, he consciously sought general principles such as those established in phenomenological thermodynamics by means of which he could obtain constraints on the behavior of matter. This strengthened his faith in the value of the relativity principle and his indirect approach to implementation of Mach's ideas. It also persuaded him that it would be useless to attempt to construct a microscopic theory of rods and clocks.

Let me now expand on these three points in more detail.

It seems to me entirely possible that an overall strategy for eliminating absolute space from physics started to take shape in Einstein's mind very soon after he had read Mach's *Mechanik* around 1898. The basic idea arose from consideration of a problem that Mach had not considered at all: electrodynamics. Much of the later development of relativity theory is clearly prefigured in a comment of Einstein to his future wife in a letter written in August 1899 (CPAE 1):

> I am more and more convinced that the electrodynamics of moving bodies, as presented today, is not correct, and that it should be possible to present it in a simpler way. The introduction of the term "aether" into the theories of electricity led to the notion of a medium of whose motion one can speak without being able, I believe, to associate a physical meaning with this statement.

This train of thought then led on to the clear formulation in 1905 of the relativity principle, in accordance with which uniform motion relative to the supposed aether is completely undetectable. As Einstein (Einstein 1905) famously remarked, this then meant that "the introduction of a 'luminiferous aether' will prove to be superfluous". Moreover, by the end of the 19th century, the aether had more or less come to be identified with absolute space, a rigid substrate that besides being the carrier of electromagnetic excitations also served as the ultimate standard of rest for all bodies in the universe. In his famous 1895 paper on electrodynamics with which Einstein was certainly familiar, Lorentz said of the aether (Lorentz 1895, 4): "When for brevity I say that the aether is at rest this means merely that no part of this medium is displaced relative to any other part and that all observable motions of the heavenly bodies are relative motions with respect to the aether."[12]

Having banished the aether from the foundations of physics, Einstein felt that he had made an important first step on the way to the complete elimination of the notion of absolute space. Einstein felt that a thing could only be said to exist if it had observable effects. The 1905 relativity principle showed that *uniform* motion relative to the putative aether (or absolute space) had no observable consequences. If the relativity principle could be extended further, to all accelerated motions, then all residual arguments for the existence of absolute space would be eliminated. Einstein's 1933 Gibson lecture (Einstein 1933) suggests rather strongly that this train of thought had

12 It is worth noting that this is a remarkably naive concept of motion compared with the subtlety of Lange's construction.

taken shape in Einstein's mind already by 1905, but that at that stage he was unable to take the idea any further. It was only in autumn 1907 (Einstein 1907) that the potential of what he later called the equivalence principle struck him; for it suggested that the restricted principle of relativity could be extended from uniform motions to *uniformly accelerated* motions as well. This then opened up the prospect of extension of the relativity principle even further—to all motions whatsoever.

This logic is spelled out very clearly in the Gibson lecture, from which the following quotation is taken:

> After the special theory of relativity had shown the equivalence for formulating the laws of nature of all so-called inertial systems (1905) the question of whether a more general equivalence of coordinate systems existed was an obvious one. In other words, if one can only attach a relative meaning to the concept of velocity, should one nevertheless maintain the concept of acceleration as an absolute one? From the purely kinematic point of view the relativity of any and every sort of motion was indubitable; from the physical point of view, however, the inertial system seemed to have a special importance which made the use of other moving systems of coordinates appear artificial.
>
> I was, of course, familiar with Mach's idea that inertia might not represent a resistance to acceleration as such, so much as a resistance to acceleration relative to the mass of all the other bodies in the world. This idea fascinated me; but it did not provide a basis for a new theory.

Note how Einstein insists that the idea of a more general equivalence of coordinate systems "was an obvious one". It certainly was not so to his contemporaries. If there is one aspect of Einstein's work on gravitation that most clearly distinguished him from them all, it was his insistence on the need to generalize the relativity principle and on the equivalence principle as the means to do so. All of the truly original steps which eventually led Einstein to the general theory of relativity sprang from this conviction. It is certainly the case that Mach's vehement opposition to Newton's absolute space as a nonexistent monstrosity was completely shared by Einstein and served as the main stimulus to the creation of general relativity.

However, it is important to note that the two men disliked absolute space for rather different reasons. Mach tended very much to concentrate on the things in the world that could be directly observed—bodies—and on the relationships between them, which were expressed in the first place by the mutual separations between them. This gut instinct is expressed very clearly in Mach's famous comment (Mach 1960): "The world is not *twice* given, with an earth at rest and an earth in motion, but only *once*, with its *relative* motions, along determinable." Given Einstein's great enthusiasm for Mach, it is remarkably difficult to find evidence which shows unambiguously that Einstein understood what Mach really wanted to do: base mechanics solely on the relative separations of bodies. As I have already noted, many of Einstein's remarks about Mach actually represent a distortion of the older man's thought. The passage from 1918 quoted earlier is a clear expression of what Mach wanted to do ("introducing in the laws of classical mechanics only distances of material points from each other"), but there is no direct attribution to Mach. The solitary direct attribution I have found is in a very late letter to Pirani,[13] in which Einstein says [my translation]:

> There is much talk of Mach's Principle. It is, however, not easy to associate a clear notion with it. Mach's stimulus was this. It is unbearable [unerträglich] that space (or the inertial system) influences all ponderable things by determining the inertial behaviour without the ponderable things exerting a determining reaction back on space. Mach rediscovered what Leibniz and Huygens had correctly faulted in Newton's theory. He sought to eliminate this evil by attempting to abolish space and replace it by the relative inertia of the ponderable bodies with respect to each other. Space should be replaced by the distances between the bodies taken in pairs (with these distances as independent concepts). This evidently did not work, quite apart from the fact that the time with its absolute nature remained.

Several comments can be made about this opening paragraph of Einstein's letter.

First, there is Einstein's admission that it is not easy to associate a clear notion with Mach's Principle. This, however, is what the criterion of Poincaré considered earlier does do.

Second, the idea that something should not be able to influence another thing without suffering a back reaction on itself is not, so far as I know, to be found anywhere in Mach's writings. It is, however, an idea that Einstein himself frequently advanced from around 1914, mainly I think as a result of his work on Nordström's theory, in which the propagation of light is governed by an absolute structure and is not subject to the influence of gravitation. For example, in 1914 (Einstein 1914) he wrote: "It seems to me unbelievable that the course of any process (e.g., that of the propagation of light in a vacuum) could be conceived of as independent of all other events in the world."

Next, it should be noted that even the account of what Mach proposed is not strictly correct, since Mach did not propose to eliminate the relations of Euclidean space and regard the distances between bodies as completely independent. It should be noted that for n bodies in Euclidean space there are $n(n-1)/2$ mutual separations, of which only $3n-6$ are independent (for $n \geq 3$). In his proposal for a new law of inertia, Mach did not include any suggestion that this basic fact of three-dimensional Euclidean geometry should be relaxed. However, in a very early paper he had speculated (Mach 1872, 25; 1911, 51–53) that such a relaxation might occur in the interior of atoms and play an important role in the formation of spectral lines. He later explicitly withdrew (Mach 1911, 94) this theoretical speculation, which hinged on a putative representation of atoms and molecules in Euclidean spaces of more than three dimensions. Einstein read Mach's booklet on the *Conservation of Energy*, where the idea is discussed, in 1909,[14] so it is possible that in his old age he muddled it up with Mach's proposals for inertia.

Finally, we note in Einstein's "this evidently did not work" an echo of the comment in 1918 that the proposal to found mechanics solely on relative separations had not proved feasible. However, the papers of Hofmann, Reissner, and Schrödinger had shown the approach to be perfectly feasible. With the possible exception of Reiss-

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13 Einstein to Felix Pirani, 1954 (EA 17–447).

14 As we know from a letter Einstein wrote to Mach in August 1909 (CPAE 5E, Doc. 174).

ner's work, Einstein had read these papers. It therefore seems that they made very little impression on him; he was certainly confused about their content, since all demonstrated the Machian approach *was* feasible. Had Einstein from the beginning shared Mach's gut instinct that only relative separations count and that the central problem was to reflect this in the foundations of mechanics, then surely he might have been expected to have taken more notice of what had been achieved.

The fact that he did not suggests that Einstein's objection to absolute space had a somewhat different psychological origin. For this conclusion, there is much evidence. Rather than consider objects in space, Einstein was evidently wont to contemplate the notion of empty space by itself. Evidence for this can be found, for example, in (Einstein 1921). Given the perfect uniformity of space, Einstein then found it an affront to the principle of sufficient reason that such a featureless thing should contain within it *distinguished frames of reference* for the formulation of the laws of nature. Numerous arguments on such lines can be found in Einstein's papers from 1913 up to the *Autobiographical Notes*. They always invoke the point mentioned in the 1933 Gibson lecture—that "from the purely kinematic point of view the relativity of any and every sort of motion [in space] was undubitable." Thus, the only way to create a theory perfectly in accord with the principle of sufficient reason was through generalization of the relativity principle to the absolutely greatest extent possible.

The difference between Mach and Einstein can be summarized very simply: Mach wanted to eliminate coordinate systems entirely, Einstein wanted to show that all coordinate systems were equally valid. Given the tremendous success of the special theory of relativity, which established the equivalence of all coordinate systems in uniform motion relative to each other, and the promise offered by the equivalence principle for extension to accelerated motion, it is very easy to see why Einstein became so totally committed to his approach and took virtually no notice of the alternative.

The question of whether one (and then which one) or both of these two approaches are valid is very complex. It is a subject that I cannot follow further in this paper, in which I have set myself the more modest task of identifying some characteristic differences between the approaches of Mach (and his contemporaries) and Einstein, finding the reasons for Einstein's choices, and placing his work in the perspective of other work on the absolute *vs* relative debate. Let me just say that, in my opinion, Mach's approach (augmented by Poincaré's analysis) is deeper and more consistent than Einstein's but that nevertheless Einstein's theory, when properly analyzed as a dynamical theory, does perfectly implement Mach's ideas. However, the reason for this has more to do with deep intrinsic properties of the absolute differential calculus, which Einstein took over 'ready made' from the mathematicians, than with Einstein's covariance arguments. All this will be spelled out in my forthcoming monograph (Barbour, in preparation). See also my *Notes Added in Proof* at the end of this article.

Because it ties in very well with Einstein's conception of space that we have just been considering, let me now turn to the role played by the rigid body and Helmholtz's work on the empirical foundation of geometry (Helmholtz 1868) in Einstein's development of both special and general relativity. Helmholtz's study was made about a decade after Riemann's famous habilitation lecture of 1854 (Riemann 1867). Initially Helmholtz was unaware of Riemann's work, which was not published until 1867, and found that he had largely rediscovered already known results. However, there was one respect in which Helmholtz went significantly beyond Riemann. This concerned the hypothesis that Riemann had made for the form of the line element.

Riemann had assumed, more or less on grounds of simplicity and to match Pythagoras's theorem, that the fundamental line element ds of his generalized geometry should be the square of a *quadratic* form in the coordinate differences. He noted, however, that *a priori* one could not rule out, say, taking the fourth root of a quartic form. In contrast, Helmholtz considered the empirical realization of geometry by rigid bodies and congruence relations between them. If such bodies are to be brought to congruence, they must satisfy certain conditions of mobility and remain congruent in different positions and different orientations. Their congruence must also be independent of the paths by which they are brought to congruence. Helmholtz was able to show that if these conditions are to be met then the quadratic form of the line element adopted by Riemann as a simplicity hypothesis is indeed uniquely distinguished. This established a very beautiful connection between empirical geometry based on physical measuring rods and a particular mathematical formalism. Helmholtz concluded his important paper with the following words:

> the whole possibility of the system of our space measurements ... depends on the existence of natural bodies that correspond sufficiently closely to the concept of rigid bodies that we have set up. The fact that congruence is independent of position, of the direction of the objects brought to congruence, and of the way in which they have been brought to each other—that is the basis of the measurability of space.

The influence of Helmholtz's study is manifest throughout Einstein's entire relativity opus. In a newspaper article published in 1926, Einstein (Einstein 1926) described the practical geometry of the experimental physicist in which "rigid bodies with marks made on them realize, provided certain precautions are taken, the geometrical concept of interval" and said [my italics]:

> Then the geometrical "interval" corresponds to a definite object of nature, and thus all the propositions of geometry acquire the nature of assertions about real bodies. This point of view was particularly clearly expressed by Helmholtz; *one may add that without this viewpoint it would have been practically impossible to arrive at the theory of relativity.*

The Helmholtzian conception was crucial for two reasons in particular: It provided a definite framework in which Einstein could comprehend length contraction and simultaneously gave a method for *position determination*. It also gave Einstein a way of 'making space visible' that perhaps made him less concerned than Mach about the problems of position determination. Taken together, these factors led Einstein to a method of position determination that appears to be decidedly un-Machian.

Indeed, a complete method of position coordination appeared already in the famous Kinematical Part of his 1905 paper (Einstein 1905). Einstein opens that section as follows: "Let us take a system of coordinates in which the equations of Newtonian mechanics hold good." Thus, he simply *presupposes* the outcome of a Tait-Lange procedure for finding an inertial frame of reference. He then continues:

> If a material point is at rest relatively to this system of coordinates, its position can be defined relatively thereto by the employment of rigid standards of measurement and the methods of Euclidean geometry, and can be expressed in Cartesian coordinates.

The standards of measurement (Helmholtz's rigid bodies) serve two supremely important purposes. First, they can be imagined to "fill" the whole space of the inertial system; since one can also suppose that the bodies carry marks permanently scratched on them, other bodies can be unambiguously located by means of these marks. Space has been made visible. Then, second, comes the really great convenience of such rigid bodies—intervals defined by the marks on them satisfy the congruence conditions required in Helmholtz's phenomenological foundation of geometry. Thus, the coordinates can be associated with the marks on the rigid bodies in such a way that they simultaneously *give physical distances directly.*

Convenient as all this is, it still does not contain anything that goes beyond Helmholtz's scheme. However, the scheme turned out to be wonderfully adapted to the exigencies of relativity theory and length contraction. Here the important thing is the underlying conception Einstein had of what might be called the true physical nature of rigid bodies (and therefore of measuring rods). He certainly did not think of them as ultimate elements incapable of further explication. On the contrary, Einstein was a convinced atomist and he conceived of a measuring rod as being made up of a definite number of atoms governed by quite definite laws of nature. Provided external circumstances (pressure, temperature, etc.) remained the same, such a system of atoms could be expected to 'settle into' a unique equilibrium configuration. Two such systems constituted by identical collections of atoms would settle into the same configuration and therefore be congruent to each other. Thus, Helmholtz's phenomenological foundation of geometry would have a theoretical underpinning in atomism and the laws governing it.

A vital part in this overall picture was played by the notion of an inertial system coupled with Einstein's formulation of the (restricted) relativity principle, in accordance with which the laws of physics must have the identical form in all inertial systems obtained from each other by a uniform translational motion. Coupled with Einstein's (long tacit but later explicit (Einstein 1923)) atomistic conception, the relativity principle ensured that the identical phenomenological Helmholtzian geometry must be realized in each inertial system. However, it left open the connection between the geometries (and chronometry, which I have not considered here) in different inertial systems. The Helmholtzian scheme had *just* enough flexibility to allow and accommodate those marvellous *bombes surprises* of relativity: length contraction and time dilation. Moreover, the underlying atomistic conception meant that one could still talk about 'the same measuring rod' in two different inertial systems. One merely had to suppose two rods constituted of the same atoms and subject to the same external conditions. Then in their respective inertial systems they would settle into identical configurations, and comparison of these configurations between inertial systems would become epistemologically valid. One would be talking about the 'same things.'

Another extraordinary convenience of this whole conception was that it could be generalized almost unchanged to the framework of the general theory of relativity. It was merely necessary to repeat the entire exercise, not globally, but 'in the small.' Very important here was the presumed existence of local approximations to inertial systems in the neighborhood of every point of spacetime; for the distinguished 'equilibrium' configurations into which rods could be assumed to 'settle' exist only in an inertial system.

Einstein's attitude to his phenomenological treatment of rods and clocks is summarized very clearly in his *Autobiographical Notes*:[15]

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

Before commenting on this passage and its bearing on the central issues of the absolute *vs* relative question, let us consider the third factor that I identified as shaping Einstein's overall strategy in the creation of both relativity theories: the quantum. In the above passage, Einstein merely says "it was clear from the very beginning that the postulates of the theory are not strong enough to deduce ... a theory of measuring rods and clocks." However, while this statement is obviously true it is at the same time something of an inversion of the actual historical development. The fact is that Einstein *deliberately*, already in the period 1904/5, chose to develop his ideas on the basis of very general postulates. His reasons for doing so are very well known and were explained by Einstein himself in the *Autobiographical Notes*.

The truth is that Planck's discovery of the quantum of action in 1900 made a tremendous impression on Einstein and quickly persuaded him that some very strange things indeed must be happening at the microscopic level. In particular, he was certain that Maxwell's equations (for which he had the very greatest respect) could not be valid in their totality for microscopic phenomena. This comes out graphically in a letter that Einstein wrote to von Laue in January 1951:[16]

15 He had already made very similar comments in (Einstein 1923).

16 Einstein to Max von Laue, January 1951 (EA 16-154).

> If one goes through your collection of the verifications of the special theory of relativity, one gets the impression that Maxwell's theory is secure enough to be grasped. But in 1905 I already knew for certain that it yields false fluctuations of the radiation pressure and thus an incorrect Brownian motion of a mirror in a Planck radiation cavity.

It is well known that Einstein's complete conviction that Maxwell's theory could be at best partly right was a decisive factor in his selection, as a postulate in the foundations of his special theory of relativity, of the one minimal part of Maxwellian theory in which he did retain confidence: the light propagation postulate.

More generally, it led him at the same time to formulate consciously the idea of a theory based on principles that wide experience had demonstrated had universal validity. The classic paradigms of such principles were the denials of the possibility of construction of perpetual motion machines of the first and second kind, which played such a fruitful role in the phenomenological thermodynamics that had been created around the middle of the 19th century. The great strength of such theories was that they enabled one to make many important predictions without attempting to find a detailed theory at a fundamental microscopic level. Such a theory Einstein called a *constructive* theory, in contrast to a theory based on principles. In a very clear account of the distinction between the two kinds of theory that he included in a piece that he wrote for *The Times* (Einstein 1919), Einstein said that the theory of relativity was one of the latter kind, which he called *fundamental*.

Let me now conclude by considering some of the consequences of these three aspects of Einstein's overall strategy—the programme to eliminate absolute space by generalization of the relativity principle, the use of Helmholtzian rigid bodies to define position, and the eschewal of constructive theories. Both together and separately, they had the consequence that virtually all the issues that one might have expected to feature prominently in a frontal attack on the absolute *vs* relative question—and that was certainly a very major part of Einstein's undertaking—were actually missing as explicit elements in Einstein's work. It is almost a case of Hamlet without the Prince of Denmark.

One of the biggest problems with Einstein's approach is that distinguished frames of reference figured crucially in his work, but he never explicitly considered their status and origin. For example, in his work on special relativity he would invariably start by assuming the existence of inertial frames of reference and then postulate the existence of laws of nature that had to be expressed relative to these frames of reference. Once this step had been taken, the relativity principle could come into play—the laws of nature must take the same form in all frames of reference related by Lorentz transformations. It is however legitimate to ask what determines the frames of reference: Are there laws of nature that determine them? The whole logic of Einstein's approach made it impossible for him to pose, let alone answer, this question, since the frames of reference had to be there before he could formulate the laws of nature. Einstein bequeathed us an unresolved vicious circle at the heart of his theory.

It should not be thought that the transition to general relativity, in which all frames of reference are purportedly allowed, eliminates this problem. The fact is that the *local* existence of distinguished frames of reference (locally inertial frames) is an absolute prerequisite of the theory, since it is only when such frames exist that Einstein's phenomenological treatment of rods and clocks can be taken over from special relativity. But Einstein never once seems to have considered seriously how the local frames of reference and his rods and clocks could arise from first principles.

Of course, we know that he was at least partly aware of this issue, since he more than once said that one should elaborate a theory of rods and clocks. However, I suspect that Einstein did not quite appreciate the true nature of the problem, which is already evident from Neumann's theory of the inertial clock. This showed clearly that there is a *twofold* problem. First, one must have access to an inertial system; second, one must track some physical object or process whose behavior stands in a known one-to-one relationship to the framework defined by the inertial system. In the simplest case of Neumann's inertial clock, this is done directly by the motion of a force-free particle. Of these two problems, the first is by far the most difficult; indeed, the second problem is effectively solved together with the first, the very posing of which is a decidedly subtle matter (as the long and painful elaboration of the problem demonstrates).

If we now examine Einstein's relatively terse comments about rods and clocks, rather strong evidence emerges that his understanding of the issue never really advanced beyond the level of Neumann's inertial clock defined in a *known* inertial system. For example, in the same *Autobiographical Notes* from which the earlier quotation about rods and clocks was taken, Einstein refers to a clock as an 'in itself determined periodic process realized by a system of sufficiently small spatial extension' and then shortly afterwards comments:

> The presupposition of the existence (in principle) of (ideal, viz, perfect) measuring rods and clocks is not independent of each other; since a light signal, which is reflected back and forth between the ends of a rigid rod, constitutes an ideal clock, provided that the postulate of the constancy of the light-velocity in vacuum does not lead to contradictions.

From this it is clear that Einstein already presupposed knowledge of the positions of objects constituting his model clocks in an inertial frame of reference—for if the rigid rod is not moving strictly inertially, Einstein's ideal light clock is useless. All the evidence I have examined is consistent with my conclusion that Einstein never grasped this fact and that he did not properly understand the problem posed by determination of the inertial frames of reference. His disparaging remarks to von Laue about Lange's work are strong support for this view.

This is not deny the correctness of Einstein's supposition that the quantum problems made it premature to try and make truly realistic microscopic models of rods and clocks. But what Einstein had in mind was the theory of such things in *a known inertial frame of reference*, whereas the more fundamental problem concerned the origin of the frame. And to address that problem he did not really need such advanced physics. The quantum bogey gave Einstein a valid excuse for not constructing microscopic theories of actual physical clocks, but may have misled him by seeming to locate the problem in the wrong place.

Mention should also be made here of the rather vague way in which Einstein formulated the relativity principle. He invariably simply said that the laws of nature, the form of which he deliberately left vague, must take the same form in all frames of reference. He never attempted anything like the subtly refined formulation proposed by Poincaré based on identification of the kind of initial data needed to predict the future uniquely. A strength of Poincaré's approach is that it avoids the vicious circle inherent in Einstein's approach whereby the status and origin of the necessary distinguished frames (the local existence of which is still needed in general relativity despite the general covariance of that theory) is left open. In Poincaré's approach, the question of the distinguished frames of reference does not arise, since he formulated the initial-value problem deliberately in such a way that they do not enter into it at all. For some reason or other, Einstein never seems to have thought of general relativity as a dynamical theory and Poincaré's comments seem to have made no impression on him.

Finally, we must consider the effect of Einstein's Helmholtzian method of position determination. It was this above all that precluded any directly Machian implementation of relativity of position and velocity after the manner of Hofmann and Reissner. For them, like Mach, position and velocity were determined by the set of distances to all other bodies in the universe. But Einstein was completely and irrevocably tied to local position determination by means of Helmholtzian rigid bodies that 'filled' the space of inertial systems, which Mach insisted must be understood as arising from relations to other matter in the universe, whereas Einstein simply took them as given. Thus, Einstein's technique was doubly un-Machian. In the 1918 paper quoted in Sec. 2, Einstein said that "the historical development" had shown that it was not possible to "dispense with coordinate systems." For 'historical development' we must here understand the foundations of Einstein's own work: coordination by Helmholtzian rigid bodies and relativity transformations between such coordinates. Note also that in the passage cited earlier from his 1913 paper Einstein said:

> It is obvious that one cannot ascribe an absolute meaning to the concept of acceleration of a material point, no more so than one can ascribe it to the concept of velocity. Acceleration can only be defined as relative acceleration of a point with respect to other bodies.

In this passage, Einstein is quite clearly saying that position and velocity are defined relative to other bodies in exactly the same sense as did Mach (and all the other relationists). Yet he did not give any indication how that requirement was implemented in his own theory. He merely pinned his hopes on a *resistance* to acceleration induced by distant matter. These hopes came to nothing—and meanwhile the Machian issues were never directly addressed.

However, general relativity is an immensely rich and sophisticated theory, and the same can be said of the veritable odyssey of its discovery by Einstein. One can find ironies, serendipity, and utter brilliance throughout the entire saga. Just because Einstein's chosen route to the creation of general relativity did not directly address certain central issues of the absolute *vs* relative debate, this does not necessarily mean that his wonderful theory fails to solve them. After all, Newton posed some critical problems in the Scholium at the time he wrote the *Principia*, but some two hundred years passed before they were more or less completely resolved within the framework of Newtonian theory. As already indicated, I am convinced that the problems considered in this paper, which has considered alternative issues that Einstein might have addressed (and his contemporaries, above all Poincaré, did address), are actually resolved within the heart of general relativity by virtue of the exquisite mathematics on which it is based. However, that is quite a long story too and will have to be considered elsewhere (Barbour, in preparation).

NOTES ADDED IN PROOF

Since writing this article, I have continued to research and write about Mach's principle and related issues. This activity has generated much material that relates to the topics discussed in this article and envisaged for (Barbour, in preparation). First, there is my book *The End of Time* (Barbour 1999a), which considers the quantum cosmological implications of a relational treatment of time and motion. Second, the three review papers (Barbour 1997, 1999b, 2001) complement the present paper. Third, collaboration with Edward Anderson, Brendan Foster, Bryan Kelleher and Niall Ó Murchadha has resulted in the publication of about ten research papers, of which I mention (Barbour, Foster and Ó Murchadha 2002, Anderson and Barbour 2002, Barbour 2003a, Anderson, Barbour, Foster and Ó Murchadha 2003, Barbour 2003b).

These research papers take very much further the approach to Mach's principle initiated Hofmann, Reissner and Schrödinger as modified in (Barbour and Bertotti 1982) through the use of best matching (Sec. 2) to avoid anisotropy of inertial mass. The relational treatment of time, implemented through reparametrization invariance, also plays a central role. If one assumes that space is Riemannian and that all interactions are local, the two principles of *best matching* and *reparametrization invariance* lead almost uniquely to Einstein's general theory of relativity. Quite unexpectedly, they also enforce the existence of a universal lightcone and gauge theory. One starts with the notion of Riemannian space (*not* pseudo-Riemannian spacetime) and three-dimensional fields (scalar, spinor and vector) defined on it. Then implementation of the Machian principles of the relativity of motion (through best matching) and time (through reparametrization) creates a four-dimensional spacetime with all the basic features of modern physics. I believe that my claim that general relativity is perfectly Machian (as regards the relativity of time and motion) is strongly vindicated.[17]

Another issue that we have investigated is relativity of scale (Barbour 2003a, Anderson, Barbour, Foster and Ó Murchadha 2003). The same intuitive convictions that lead one to require relativity of time and motion suggest that physics ought to be scale invariant too. In the two cited papers, we have extended the notion of best matching to scale transformations. We have shown that general relativity is almost scale invariant but not quite perfectly so. Specifically, in the case of a spatially closed universe one can change the (spatial) scale arbitrarily at all points. However, this must be done subject to the solitary requirement that these local transformations do

17 See, especially (Barbour, Foster and Ó Murchadha 2002).

not change the spatial volume of the universe. This remarkably weak restriction is, in fact, what permits expansion of the universe to be a meaningful concept. Modern cosmology depends crucially on this single residual 'Machian defect.' Work on this topic and our general approach, which we call the 3-space approach, is continuing. My personal feeling is that we are close to a definitive formulation of the principles and main conclusions of the 3-space approach. I may at last be in the position to complete (Barbour, in preparation)! In fact, I put aside work on it because I felt that it would not be complete without a proper Machian treatment of the relativity of scale.

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REFERENCES

Anderson, Edward and Julian B. Barbour. 2002. "Interacting Vector Fields in Relativity without Relativity." *Classical and Quantum Gravity* 19: 3249–3261 (gr-qc 0201092).

Anderson, Edward, Julian B. Barbour, Brendan Z. Foster, and Niall Ó Murchadha. 2003. "Scale-Invariant Gravity: Geometrodynamics." *Classical and Quantum Gravity* 20: 3217–3248 (gr-qc 0211022).

Barbour, Julian B. 1989. *Absolute or Relative Motion,* Vol. 1. *The Discovery of Dynamics*. Cambridge: Cambridge University Press. Reprinted 2001 as the paperback *The Discovery of Dynamics*. New York: Oxford University Press.

———. 1994. "The Timelessness of Quantum Gravity. I. The Evidence from the Classical Theory. II. The Appearance of Dynamics in Static Configurations." *Classical and Quantum Gravity* 11: 2853–2897.

———. 1995. "General relativity as a Perfectly Machian Theory." In (Barbour and Pfister 1995).

———. 1997. "Nows Are All We Need." In H. Atmanspacher and E. Ruhnau (eds.), *Time, Temporality, Now*. Berlin: Springer-Verlag.

———. 1999a. *The End of Time*. London: Weidenfeld & Nicholson: New York: Oxford University Press (2000).

———. 1999b. "The Development of Machian Themes in the Twentieth Century." In J. Butterfield (ed.), *The Arguments of Time*. Oxford: Oxford University Press.

———. 2001. "On General Covariance and Best Matching." In C. Callender and N. Huggett (eds.), *Physics Meets Philosophy at the Planck Scale*. Cambridge: Cambridge University Press.

———. 2003a. "Scale-Invariant Gravity: Particle Dynamics." *Classical and Quantum Gravity* 20: 1543-1570 (gr-qc 0211021).

———. 2003b. "Dynamics of Pure Shape, Relativity and the Problem of Time." In H.-T. Elze (ed.), *Decoherence and Entropy in Complex Systems. Lecture Notes in Physics*, Vol. 663. Berlin: Springer-Verlag (gr-qc 0308089).

———. (in preparation). *Absolute or Relative Motion?* Vol. 2: *The Frame of the World*. New York: Oxford University Press.

Barbour, Julian B. and Bruno Bertotti. 1977. "Gravity and Inertia in A Machian Framework." *Nuovo Cimento* 38B: 1–27.

———. 1982. "Mach's Principle and the Structure of Dynamical Theories." *Proceedings of the Royal Society of London,* Series A 382: 295–306.

Barbour, Julian B., Brendan Z. Foster and Niall Ó Murchadha. 2002. "Relativity without Relativity." *Classical and Quantum Gravity* 19: 3217–3248 (gr-qc 0012089).

Barbour, Julian B. and Herbert Pfister (eds.). 1995. *Mach's Principle: From Newton's Bucket to Quantum Gravity. Einstein Studies,* Vol. 6. Boston: Birkhäuser.

Clemence, G.M. 1957. "Astronomical Time." *Reviews of Modern Physics* 29, 2–8.

CPAE 1: John Stachel, David C. Cassidy, Robert Schulmann, and Jürgen Renn (eds.), *The Collected Papers of Albert Einstein*. Vol. 1. *The Early Years, 1879–1902*. Princeton: Princeton University Press, 1987.

CPAE 2. 1989. John Stachel, David C. Cassidy, Jürgen Renn, and Robert Schulmann (eds.), *The Collected Papers of Albert Einstein*. Vol. 2. *The Swiss Years: Writings, 1900–1909*. Princeton: Princeton University Press.

CPAE 4. 1995. Martin J. Klein, A. J. Kox, Jürgen Renn, and Robert Schulmann (eds.), *The Collected Papers of Albert Einstein*. Vol. 4. *The Swiss Years: Writings, 1912–1914*. Princeton: Princeton University Press.

CPAE 5E: *The Collected Papers of Albert Einstein*. Vol. 5. *The Swiss Years: Correspondence, 1902–1914*. English edition translated by Anna Beck, consultant Don Howard. Princeton: Princeton University Press, 1995.

CPAE 6. 1996. A. J. Kox, Martin J. Klein, and Robert Schulmann (eds.), *The Collected Papers of Albert Einstein*. Vol. 6. *The Berlin Years: Writings, 1914–1917*. Princeton: Princeton University Press.

CPAE 7. 2002. Michel Janssen, Robert Schulmann, József Illy, Christoph Lehner, and Diana Kormos Buchwald (eds.), *The Collected Papers of Albert Einstein*. Vol. 7. *The Berlin Years: Writings, 1918–1921*. Princeton: Princeton University Press.

Dziobek, Otto. 1888. *Die mathematischen Theorien der Planeten-Bewegungen*. Leipzig: J. A. Barth.

———. 1892. *Mathematical Theories of Planetary Motions*. Register Publishing Company. Reprinted by Dover in 1962.

Einstein, Albert. 1905. "Zur Elektrodynamik bewegter Körper." *Annalen der Physik* 17, 891–921, (CPAE 2, Doc. 23). English translation in (Lorentz et al. 1923).

———. 1907. "Über das Relativitätsprinzip und die aus demselben gezogenen Folgerungen." V. §17. *Jahrbuch der Radioaktivität und Elektronik* 4: 411–462, (CPAE 2, Doc. 47).

———. 1912. "Gibt es eine Gravitationswirkung, die der elektrodynamischen Induktionswirkung analog ist?" *Vierteljahrschrift für gerichtliche Medizin und öffentliches Sanitätswesen* 44: 37–40, (CPAE 4, Doc. 7).

———. 1913a. "Zum gegenwärtigen Stande des Gravitationsproblems." *Physikalische Zeitschrift* 14: 1249–1266, (CPAE 4, Doc. 17). (English translation in this volume.)

———. 1913b. "Physikalische Grundlagen einer Gravitationstheorie." *Vierteljahrsschrift der Naturforschenden Gesellschaft Zürich* 58: 284–290, (CPAE 4, Doc. 16).

———. 1914. "Die formale Grundlage der allgemeinen Relativitätstheorie." *Sitzungsberichte der Preussischen Akademie der Wissenschaften*, 1030–1085, Part 2, (CPAE 6, Doc. 9).

———. 1917. "Kosmologische Betrachtungen zur allgemeinen Relativitätstheorie." *Sitzungsberichte der Preussischen Akademie der Wissenschaften*: 142–145, (CPAE 6, Doc. 43).

———. 1918a. "Prinzipielles zur allgemeinen Relativitätstheorie." *Annalen der Physik* 55: 241–244, (CPAE 7, Doc. 4).

———. 1918b. "Dialog über Einwände gegen die Relativitätstheorie." *Die Naturwissenschaften* 6: No. 48, 697–702, (CPAE 7, Doc. 13).

———. 1919. "What is the Theory of Relativity?" *The Times,* 29 November 1919. Republished in (Einstein 1954).

———. 1921. "Geometrie und Erfahrung." *Sitzungsberichte der Preussischen Akademie der Wissenschaften* Vol. 1, 123–130.

———. 1923. "Grundgedanken und Probleme der Relativitätstheorie." In *Nobelstiftelsen, Les Prix Nobel en 1921–1922*. Stockholm: Imprimerie Royale.

———. 1926. "Nichteuklidsche Geometrie in der Physik." *Neue Rundschau,* January.

———. 1933. "Notes on the Origin of the General Theory of Relativity." In *Albert Einstein*. *Ideas and Opinions,* 285–290. Translated by Sonja Bargmann. New York: Crown, 1954.

———. 1949. "Autobiographical Notes." In P.A. Schilpp (ed.), *Albert Einstein, Philosopher-Scientist*. Evanston, Illinois: The Library of Living Philosophers, Inc., Illinois, 29.

———. 1954. *Ideas and Opinions*. New York: Crown Publishers.

Frege, Gustav. 1891. "Über das Trägheitsgesetz." *Zeitschrift für Philosophie und philosophische Kritik* 98, 145–161.

Friedlaender, Benedict and Immanuel Friedlaender. 1896. *Absolute oder relative Bewegung?* Berlin: Leonhard Simion. (English translation in this volume.)

Hall, Alfred R. and Marie B. Hall. 1962. *Unpublished Scientific Papers of Isaac Newton*. Cambridge: Cambridge University Press.

Helmholtz, Hermann L. F. 1968. "Über die Tatsachen, die der Geometrie zum Grunde liegen." *Nachrichte von der Königlichen Gesellschaft der Wissenschaften zu Göttingen,* No. 9, June 3rd.

Hofmann, W. 1904. *Kritische Beleuchtung der beiden Grundbegriffe der Mechanik: Bewegung und Trägheit und daraus gezogene Folgerungen betreffs der Achsendrehung der Erde und des Foucault'schen Pendelversuchs*. Wien und Leipzig: M. Kuppitsch.

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

Lagrange, Joseph-Louis. 1772. "Essai sur le problème des trois corps." Republished in: *Oeuvres de Lagrange*, Vol. 6, Paris: Gauthier-Villars, 229 (1873).

Lange, Ludwig. 1884. "Über die wissenschaftliche Fassung des Galilei'schen Beharrungsgesetz." *Philosophische Studien* 2: 266–297.

———. 1885. "Über das Beharrungsgesetz." *Berichte der Königlichen Sächsischen Gesellschaft der Wissenschaften, Math.-Physik*. Klasse 333–351.

———. 1886. *Die geschichtliche Entwicklung des Bewegungsbegriffs und ihr voraussichtliches Endergebnis. Ein Beitrag zur historischen Kritik der mechanischen Prinzipien*. Leipzig: W. Engelmann.

Laue, Max von. 1948. "Dr. Ludwig Lange. 1863–1936. (Ein zu unrecht Vergessener.)" *Die Naturwissenschaften* 35, 193.

———. 1955. *Die Relativitätstheorie,* Vol. 1. *Die Spezielle Relativitätstheorie*. Braunschweig: Vieweg.

Lorentz, Hendrik Anton. 1895. *Versuch einer Theorie der electrischen und optischen Erscheinungen in bewegten Körpern*. Leiden: Brill.

Lorentz, Hendrik Antoon et al. 1923. *The Principle of Relativity.* London: Methuen.

Mach, Ernst. 1872. *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit*. Prague: Calve.

———. 1883. *Die Mechanik in ihrer Entwickelung. Historisch-kritisch dargestellt*. Leipzig: F.A. Brockhaus.

———. 1911. *History and Root of the Principle of the Conservation of Energy*. Chicago: Open Court.

———. 1960. *The Science of Mechanics. A Critical and Historical Account of Its Development*. LaSalle: Open Court.

Neumann, Carl. 1870. *Ueber die Prinzipien der Galilei-Newtonschen Theorie*. Leipzig: Teubner.

Norton, John. 1995. "Mach's Principle before Einstein." In (Barbour and Pfister 1995).

Poincaré, Henri. 1898. "La Mesure du Temps." Republished in Poincaré, Henri (1905). *La Valeur de la Science*.

———. 1902. *La Science et l'Hypothèse*. Paris: Flammarion.

———. 1905. *Science and Hypothesis*. London: Walter Scott Publ. Co.

Reissner, Hans. 1914. "Über die Relativität der Beschleunigungen in der Mechanik." *Physikalische Zeitschrift* 15: 371–75.

———. 1915. "Über eine Möglichkeit die Gravitation als unmittelbare Folge der Relativität der Trägheit abzuleiten." *Physikalische Zeitschrift* 16: 179–85.

Riemann, Bernhard. 1867. "Über die Hypothesen, welche der Geometrie zu grunde liegen." *Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen* 13.

Tait. Peter G. 1883. "Note on Reference Frames." *Proceedings of the Royal Society of Edinburgh,* Session 1883–84: 743–745.

Thomson, James. 1883. "On the law of inertia; the principle of chronometry; and the principle of absolute clinural rest, and of absolute rotation." In *Proceedings of the Royal Society of Edinburgh,* Session 1883–84, 568 and 730.

Thomson, William and Peter G. Tait. 1867. *Elements of Natural Philosophy.* Cambridge: Cambridge University Press.