Indiscernibles, General Covariance, and Other Symmetries: The Case for Non-Reductive Relationalism

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Introduction

What is the meaning of general covariance? We learn something about it from the *hole argument*, due originally to Einstein. In his search for a theory of gravity, he noted that if the equations of motion are covariant under arbitrary coordinate transformations, then particle coordinates at a given time can be varied arbitrarily - they are underdetermined - even if their values at all earlier times are held fixed. It is the same for the values of fields. The argument can also be made out in terms of transformations acting on the points of the manifold, rather than on the coordinates assigned to the points. So the equations of motion do not fix the particle positions, or the values of fields at manifold points, or particle coordinates, or fields as functions of the coordinates, even when they are specified at all earlier times. It is surely the business of physics to *predict* these sorts of quantities, given their values at earlier times. The principle of general covariance therefore seems untenable.

It is understandable that Einstein, sometime in 1911, gave up the principle in consequence, but it was an error all the same; four years later, once he had realized his mistake, progress was rapid Within the year, he was in possession of the full field equations of the general theory of relativity (GTR).[1]

Now, I want to draw attention to a much older argument, due originally to Leibniz. Leibniz argued that given the homogeneity of Newtonian space, the overall positions of particles, over and above their relative positions, can be varied arbitrarily; they too are underdetermined. Likewise, given the Galilean symmetries, overall absolute velocities and orientations are underdetermined. Call these *shift arguments*. In fact, an argument exactly parallel to the hole argument can be formulated in Newtonian gravity (NTG), if one is prepared to make use of the symmetry of the theory under time-dependent boosts (equivalently, under time-dependent uniform gravitational fields). In that case the absolute quantities are all underdetermined even given their values at all earlier times.

Evidently these arguments target quantities of a specific sort - *absolute* quantities. The solution in each case is broadly the same: the physically real properties and relations are the invariant ones, which do not include the absolute quantities. Pretty well all the invariant quantities turn out to be relational ones, of certain specified sorts. And in the case of NTG, insofar as Leibnizians and Newtonians were really opposed on the nature of space, the shift arguments surely come down on the side of Leibniz's relationalism.

Now for my principal claim: it is that the form of relationalism underpinned by these arguments has nothing to do with a *reductionist* doctrine of space or spacetime (the doctrine that space or spacetime has no independent existence independent of matter); that further, this form of relationalism is a systematic and coherent doctrine that can be applied to any exact symmetry in physics, including the more contentious cases of gauge symmetry and the symmetries of constrained Hamiltonian systems.[2] It is, in point of fact, a natural expression of Leibniz's philosophical principles, in particular the principle of identity of indiscernibles (PII). Relationalism is usually taken to be a reductionist account of space, that space is *nothing more* than the system of spatial relations between actual or possible distributions of

[1]Some consensus on this history has recently been arrived at by a number of historians, among them Renn, Corry, and Norton, as also by Stachel; see Renn *et al* (2000).

[2]Hence they have a bearing on the so-called "problem of time". See Belot and Earman (2001) for a recent review.

matter. Call this *eliminative* relationalism. Leibniz, in his criticism of the Newtonians, did argue for eliminativist relationalism. The links between Leibniz's philosophical principles and contemporary debates in the foundations of spacetiime theories have recently been much discussed (see e.g. Belot 2001); it is always the eliminativist doctrine that is supposed to follow from his principles. Earman, in his influential study of the absolute-relational debate, uses the term "relationism" to mean exactly this eliminativist doctrine (Earman 1989). The hole argument, in the hands of Earman and Norton (1987), was considered an argument for eliminativist relationalism, on the understanding that space or spacetime in itself is described by the bare manifold, divorced of any fields, metrical or otherwise. They also applied the hole argument to the pregeneral-relativistic theories, to roughly similar ends. Leibniz's principles in their original form certainly do imply the eliminativist version of relationalism; what has been overlooked is the difference to his principles made by modern logic. It is the PII, understood in the context of modern logic, that I am interested in, and it is non-reductive relationalism that follows from it.

A different sort of objection has been made to Earman's agenda, and specifically to his use of the hole argument. It has been objected, most prominently by Stachel, that the hole argument cannot be taken over to the pregeneral-relativistic case; that, in point of fact, GTR differs radically from any other spacetime theory, in that it alone requires that any method for the identification of points of space by dynamical in origin (Stachel 1993). Pregenernal-relativistic theories, in contrast, permit the use of non-dynamical methods for identifying points of space. Now, I am sympathetic to Stachel's concern to distinguish GTR from other spacetime theories, but I do not believe there is any such thing as a non-dynamical method for identifying points of space. To be sure there are practical, operational methods, but these are available whatever one's theory; they do not seem to be what Stachel has in mind. This point is important, because the existence of such methods would bear equally on the shift arguments. It would undercut appeal to the PII as well. It is, in many respects, a restatement of Clarke's point of view, as argued against Leibniz in the debate over Newtonian space (in the *Correspondence*; Alexander (1956)).

First I will state the hole argument in detail, and then go on to see why

Stachel restricts the argument to GTR. After that I will say something about Earman's understanding of relationalism. Only then will I go on to Leibniz's principles, and the PII in its modern guise, and the form of relationalism that follows from it. At the end I will return to Stachel's objections.

1 The Hole Argument According to Stachel

Stachel denies that the hole argument has any bearing on precursors to GTR; he denies, further, that general covariance is a space-time symmetry, on a par with the familiar symmetries of NTG. So let us use the term "diffeomorphism covariance" instead, to denote covariance under diffeomorphisms (covariance under the smooth mappings on the manifold M given by C^∞ functions $f : M \to M$). It is well-known that the equations of motion of pregeneral-relativistic theories can all be written in diffeomorphism-covariant form. From now on let "general covariance" mean what Stachel means: diffeomorphism covariance along with an additional principle, namely that no *individuating fields* are available, other than those provided by solving the dynamical equations.[3] By "individuating field" Stachel means any set of properties that can be used to uniquely distinguish the points of a manifold. He gives as an example a set of colors, as indexed by hue, brightness and saturation, which provide an individuating field for the points of a 3-dimensional manifold. According to Stachel, in pregeneral-relativistic theories one can always suppose that such a field exists *independent* of the dynamical system of equations under study. In principle one can always give meaning to the coordinates used in the equations, independent of solving them. Not so in GTR, where there is no space-time to be individuated, prior to a particular solution to the equations.

Let me state the hole argument more precisely. I shall use the coordinate-dependent method. Consider a manifold M, equipped with an atlas of charts. With respect to one of these charts, write the metric tensor field g along with the sources ρ of the gravitational field as functions of x (denote generically $D(x)$). Suppose this is done everywhere on M except on an open subset H

[3]Stachel (1993 p.142). Many others have thought that general covariance must mean something more than diffeomorphism-covariance; see e.g. Wald (1984 p.57).

(passing from one chart to another as necessary), and suppose that locally a system of equations can be given for these fields, supplemented if necessary by appropriate boundary conditions and rules for passing from one chart to another. Let these equations be diffeomorphism-covariant. Let the boundary of H be ∂H. Then, given a solution D to these equations for g and ρ outside of H and on ∂H, the equations do not determine any unique solution in the interior of H.

The proof is by construction. Recall first that for any tensor field ϕ on M and for any diffeomorphism f on M, one can define a new tensor field $f * \phi$ on M, the drag-along[4] of ϕ under f; and that if a system of equations is diffeomorphism covariant, then the drag-along of any solution is also a solution. Let D be as above; then D and $f * D$ are both solutions to the equations of motion, for any f. But $D(x)$ and $(f * D)(x)$ are in general different tensors at one and the same point x (we are interpreting diffeomorphism symmetries as *active* symmetry transformations). If now we choose f so that it is non-trivial inside H, but reduces to the identity on ∂H and outside H, then $D(x) = (f * D)(x)$ for $x \notin H$, but $D(x) \neq (f * D)(x)$ for $x \in H$.

There is of course a way out of the conundrum. Following Stachel, we should pass to the *equivalence class* of solutions under diffeomorphisms, a view which is by now quite standard in the literature (see e.g. Hawking and Ellis (1972), Wald (1984)).[5] Only the equivalence class is physically real. On this understanding, general covariance is invariably an unbroken symmetry, and the physical world is to be described in a diffeomorphic invariant way. The price, however, is that the values of the fields at manifold points, or as functions of a fixed coordinate system, are not physically real. Only the relations among these field values are invariant, so only these relations are real.

This takes some getting used to: the values of fields at points go the way of particle velocities, positions, and directions; only relations among them remain. But can we adopt this point of view if the manifold points can be

[4]For a scalar field, the drag-along is just $(f * D)(x) = D(f(x))$; we will not need the definition in the case of tensor fields.

[5]Although it is not clear that it is shared by all; see e.g. Weinberg (1989, p.4).

independently individuated? In that case, says Stachel, surely not:

> The fact that the gravitational field equations are [diffeomorphism]-covariant does *not* suffice to allow the physical identification of a class of mathematically distinct drag-along fields. If such an individuating field existed, a relative dragging between metric tensor field and individuating field would be physically significant, and Einstein's hole argument would be ineluctable. (Stachel 1993 p.140-41.)

If there is an individuating field, which can be used to specify the values of fields at points, but which is not itself subject to the diffeomorphism, then these values will be *changed* by the diffeomorphism; if this individuating field is physically meaningful, then so too are the values of fields at points marked out by it. The hole argument then becomes "ineluctable". Stachel is surely right on this.

But there is an additional and crucial feature of GTR, which none of the classical theories shares; namely, that *in principle* there cannot be any individuating field of this kind. For the metrical field necessarily determines *both* the gravitational field structures, the affine connection and the Riemann tensor, *and* the chronogeometrical structure, the spatiotemporal relations. The coordinates figuring in the field equations *cannot* be antecedently understood as space-time coordinates, for there are as yet *no* space-time relations, not even a topology, prior to solving the equations for the metrical field in GTR.

It is now clear what is wrong with Earman and Norton's (1987) argument. In the pregeneral-relativistic theories one always has an *a priori* chronogeometrical structure. One always knows what the geometry is, independent of obtaining any solution to the equations of motion. So it does make sense, from this perspective, to introduce an *independent* individuating field; there will always be this option in such cases. In fact, Stachel goes on to insist, for this reason the pregeneral-relativistic theories are better stated in terms of their rigid, finite-dimensional groups, as in Klein's *Erlangen* program (what he calls their "affine-space-plus" form). This brings out the true nature of space and space-time according to these theories. But once this is done the hole argument cannot even be applied.

In support of Stachel, one might add that Newton explicitly denied that points of space or instants of time could have different spatiotemporal relations than those which they have, and yet remain the same points; as Maudlin has put it, they have their metrical relations "essentially" (Maudlin 1988). Newton would have denied that transformations that change these essential properties could have any physical meaning (one simply wouldn't be talking about the *same points*). The only transformations that Newton allowed to be actively interpreted (in line with Maudlin's argument) were the isometries, which preserve all the metrical relations among points. So it seems we are back to the rigid symmetries, the translations, rotations, and boosts. And if a rigid symmetry transformation acts as the identity on any open set, it *is* the identity; the hole argument cannot even be posed.

But here Stachel makes an important qualification (Stachel, 1993, p.148, and note 10), explicitly limiting his remarks to pregeneral-relativistic theories *other* than NTG - so, essentially, among the fundamental classical dynamical theories, to special relativistic electromagnetism. For as is well known, NTG can be put in a form which reveals a wider class of symmetries. These symmetries were, moreover, recognized by Newton, who saw very well that his theory of gravity could also be applied to the case of uniformly accelerating frames of references - even, in point of fact, that it could be applied when the acceleration varies *arbitrarily* with the time (so long as it is constant in space).[6] Unfortunately this point was not understood by Clarke in the *Correspondence.* There, he supposed this symmetry of NTG only followed given *Leibniz's* principles - and viewed it as a *reductio* of them;[7] nor did

[6]Newton stated only the weaker result (at Corollary VI, Book 1, *Principia*), although the stronger principle (which includes time-dependent "equal accelerative forces", as he called them) is obvious. He used this symmetry to justify the application of his laws of motion to the Jupiter system, notwithstanding the gravitational influence of the Sun. It is evidently a precursor to Einstein's Principle of Equivalence.

[7]Clarke, 3rd reply (Alexander, 1956, p.32); here he talks of moving the "whole material world entire", and "the most sudden stopping of that motion". He returned to this objection in his 4th reply (*ibid*, p.48), only to confuse it with the very different objection that there are dynamical effects when a *subsystem* of the universe is brought to a sudden halt. Leibniz pointed out the confusion in his 5th Paper (*ibid*, p.74); in his 5th reply, Clarke restated the objection in its orignal form, complaining that Leibniz "had not attempted to give any answer" (*ibid*, p.105). In fact Leibniz had responded as do we, identifying the two motions as indiscernible (*ibid*, p. 38).

Leibniz rectify the error. Nevertheless the fact remains: to a clear thinker, familiar with Newton's *Principia*, *circa* the time of the *Correspondence*, the following symmetry principle was readily apparent: the equations of motion for the relative distances of a system of bodies, referred to a non-rotating frame of reference, are covariant under the group of transformations:[8]

$$\overrightarrow{x} \to R \cdot \overrightarrow{x} + \overrightarrow{f(t)} \tag{1}$$

where R is an orthogonal matrix and $\overrightarrow{f}$ is a twice differentiable but otherwise arbitrary vector-valued function of the time. If we now choose R as the identity, and $\overrightarrow{f}$ so that it is zero prior to some instant t_0 but non-zero thereafter, we have a version of the hole argument: the value of the absolute position of any particle, and its derivatives, after t_0, can be varied arbitrarily, even keeping fixed its values at all previous times, and even keeping fixed the values of all *other* particles at all previous times. Call this the *generalized* shift argument.

The rejoinder, presumably, is that here too a non-dynamical individuating field may be antecedently available, so that the symmetries (1) will have to be abandoned (equivalently, equations for the relative particle configurations will not be judged to tell the whole story).[9] Against it, one wonders what these non-dynamical individuating fields can really amount to. It does not appear that in NTG there ever was a method available for determining differences in absolute positions, and differences in absolute velocities, at different times.

We will return to this question in due course. Here I want only to note the alternative solution: evidently, if no such non-dynamical individuating field is available, then the symmetry (1) can be retained, with no consequent underdetermination of the theory, so long as we acknowledge that the only physically-meaningful quantities are comparisons between positions, velocities and accelerations *at a single time*. In terms of Newton-Cartan spacetime,

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[8]Called the *Newtonian group* by Ehlers (1973).

[9]This was not quite Carke's conclusion; as remarked, he supposed the argument was liscensed only by Leibniz's principles, not Newton's; but it is in line with his position in the face of those symmetry arguments that he did acknowledge applied to NTG.

this follows from the fact that in general no meaning can be given to the decomposition of the connection into an inertial part and a gravitational part.

Evidently both arguments, the hole argument and the generalized boost argument, promote the view that only certain kinds of relational structures are physically real. But this is not relationalism as it is ordinarily considered; let us get clear on the difference.

2 Relationalism and Eliminativism

Gravity and geometry are inseparable in GTR. Does this mean that gravity is reduced to geometry? Stachel is as likely to put it the other way round:

> Several philosophers of science have argued that the general theory of relativity actually supports spacetime substantivalism (if not separate spatial and temporal substantivalisms) since it allows solutions consisting of nothing but a differentiable manifold with a metric tensor field and no other fields present (empty spacetimes). This claim, however, ignores the second role of the metric tensor field; if it is there chronogeometrically, it inescapably generates all the gravitational field structures. Perhaps the culprit here is the words "empty spacetime". An empty spacetime could also be called a pure gravitational field, and it seems to me that the gravitational field is just as real a physical field as any other. To ignore its reality in the philosophy of spacetime is just as perilous as to ignore it in everyday life. (Stachel, 1993, p.144).

Stachel is looking to a deflationary view of relationalism, without the usual eliminitivism that goes with it. This does not quite mean that he sees no opposing doctrine; indeed, he is prepared to treat "substantivalism" and "absolutism" as synonymous (Stachel (1993 p.154, fn. 2; I shall not follow him in this). Absolutism, in the context of NTG, is the doctrine that there is a preferred state of rest, the Gallilean symmetries notwithstanding. By extension we may take it to be the view that there are absolute positions and directions as well: relationalism in my sense and in Stachel's is certainly opposed to absolutism.

The term "substantivalism" is due to Sklar (1974, p.161); he used it to mean that space exists independent of matter. The term "substantival" is older, and has a more specific meaning: it was used by Johnson for an account of space in which spatial points themselves have positions, and objects have positions by virtue of occupying points (Johnson 1924 p.79). Earman sees the hole argument as an argument against substantivalism in Sklar's sense, taking the bare manifold to represent space in itself, independent of fields altoghether. Relationalism, as he understands it, is opposed to substantivalism. Space and time, on this view, must be *eliminated*, as independent entities, in favour of material ones. And this would be a highly non-trivial affair:

> Not a single relational theory of classical motion worthy of the name "theory" and of serious consideration was constructed until the work of Barbour and Bertotti in the 1960s and 1970s. This work came over half a century after classical space-time gave way to relativistic space-time, and in the latter setting a purely relational theory of motion is impossible. (Earman 1989 p.166.)

Moreover, substantivalism in Sklar's general sense is essential to almost every other part of physics::

> ....no detailed antisubstantivalist alternative has ever been offered in place of the field theoretic viewpoint taken in modern physics. (Earman 1989 p.166.)

But is either of these positions well-motivated? Stachel is doubtful that there is any longer a well-defined distinction between matter and space in the first place, for they have both been superseded by the field. Einstein said much the same:

> There is no such thing as an empty space, i.e. a space without field. Space-time does not claim existence on its own, but only as a structural quality of the field. (Einstein 1954.)

For a systematic discussion of the various ways in which the distinction between matter and space has been weakened by developments in physics over the last two centuries, see Rynasiewicz (1996a). One has only to consider the ether, the electromagnetic field, the metrical field, and the wave-function of quantum mechanics, to see what he means. And once this distinction is broken down, Rynasiewicz goes on to claim, the old philosophical dispute no longer has any point.

Stachel will hardly be moved by talk of wave-functions, but he will agree with Rynasiewicz in respect of his other examples. Let it be granted that the old distinction between space and matter is no longer clear-cut. Does anything remain to the relationalist position? Is Rynasiewicz right to say that none of the philosophical disputes any longer has bite?

Earman has stated three criteria for relationalism, extracted from the pregeneral-relativistic context. Let us take them one by one.

> R1. All motion is the relative motion of bodies, and consequently, spacetime does not have, and cannot have, structures that support absolute quantities of motion. (Earman 1989 p.13.)

Evidently if material and spatial structures enjoy much the same status, little remains to R1 but the platitude that all motion is relative. We hasten on.

> R2. Spatiotemporal relations among bodies and events are direct; that is, they are not parasitic on relations among a substratum of space points that underlie bodies or space-time points that underlie events. (*Ibid* p.13.)

Here Earman draws on Johnson's criterion. We should not take this to mean that points of space or spacetime do not exist at all; it is the claim - at least in the pregeneral-relativistic case - that we do not first determine the points at which bodies or events are situated, and deduce their spatiotemporal relations from the spatiotemporal relations among points. This is the reading I shall give it. It is backed up by Earman's third condition:

> R3 No irreducible, monadic, spatiotemporal properties, like "is located at space-time point p", appears in a correct analysis of the spatiotemporal idiom. (*Ibid* p.14.)

We do not begin with the locations of bodies at points; there are no such irreducible properties. On the other hand, if there are spacetime points at all, we can surely end up with statements of location. I shall take R3 to be laying down a constraint on how such statements are to be made: there can be no appeal to irreducible monadic properties to specify the point p.

There is therefore a kernal to the relationalist position, as formulated by Earman in the case of NTG and special relativity, which I shall summarize thus:

> R0. Points of space and spacetime, in NTG and special relativity, are specified by their relations to bodies and events.

It is true that R0 does not treat space and matter on an even footing, but it leaves open the possibility that the difference, such as it is, is due to the symmetries of space and spacetime, for these are criteria, I say again, abstracted from the pregeneral-relativistic context. As we shall see, granted such symmetries, R0 does indeed follow from Leibniz's amended principles. It is a consequence of non-reductive relationalism.

Earman anticipates one sort of deflationary move, the suggestion that the substantival-relational debate has no physical consequences, but he expects this move to be made broadly on instrumentalist grounds. He grants that there is a corresponding weakened version of relationalism, but he takes this to be entirely trivial:

> ...the relationist can follow either of two broad courses. One, he can decline to provide a constructive alternative field theory and instead take over all of the predictions of field theory for whatever set of quantities he regards as relationally pure. I do not see how this course is any different from instrumentalism. While I believe instrumentalism to be badly flawed, I do not intend to argue that here. Rather, the point is that relationism loses its pungency as a distinctive doctrine about the nature of space and time if it turns out to be nothing but a corollary of a methodological doctrine about the interpretation of scientific theories.
> Two, the relationist can attempt to provide a constructive alternative to field theory..... (Earman 1989 p.166.)

It will be clear, however, that relationalism as I understand it has nothing to do with instrumentalism; it is on the contrary a form of realism. Neither did Stachel and Rynasiewicz downplay the distinction between space and matter on positivist grounds; they too are realists.

Earman is clearly angling for a connection between relationalism as it presents itself on the basis of the shift and hole arguments, and *Machianism*: his reference to the Barbour-Bertotti theory makes this plain. But all these arguments proceed from *given* symmetries of theories; they are none of them *a priori*. Certainly we do not as yet have any argument for the view that transformations among rotating frames of references should be a symmetry group of a dynamical theory.

That does not mean there are no such arguments. Indeed, Machianism does follow from Leibniz's philosophical principles, in their original form. Let us see how.

3 Leibniz's Principles

In common with the scholastics, Leibniz believed that the description of a thing should describe the thing independent of anything else, not in terms of any actual or possible relations it might have with other things. Call this

his *independence thesis*.

True sentences are those which, albeit via a process of analysis, are of subject-predicate form, where the predicate is already contained in the concept of the subject (the "subject" of a sentence is what the sentence is about). They are, as it were, *definitional*, of the form "gold is a yellow metal"; they follow from the description of the thing. For this to be so the subject mentioned, the thing or natural kind, has to be thought of "completely", as already containing within it all the meaningful physical predicates that can properly be assigned to it. Call this his *containment theory of truth*.

Most important of all, *for everything there is a reason*; nothing is to be arbitrary or unexplained. There is, indeed, no choice to be made, unless a meaningful distinction has been drawn. This is Leibniz's celebrated *principle of sufficient reason* (PSR). It is closely related to another of his principles, the *identity of indiscernibles* (PII). According to this, numerically distinct things must differ in some meaningful way, for otherwise there could be no basis for choice among them.

Finally, Leibniz is committed to a weak form of verificationism: physically real differences, if there are any, had better be experimentally detectable, however indirectly. Indeed, by a symmetric object Leibniz understands an object which can be arranged in nominally distinct ways which do not admit any detectable experimental difference.

Given these principles, Leibniz's views follow quite naturally. Eliminativism is among them. Things cannot be located in space because

> Space being uniform, there can be neither any external nor internal reason, by which to distinguish its parts, and to make any choice between them. For, any external reason to discern between them, can only be grounded upon some internal one. Otherwise we should discern what is indiscernible, or choose without discerning. (Leibniz, Fourth Paper, Alexander 1956, p.39).

External reasons are ruled out by the independence thesis: things have to be thought of independent of anything else (so not in relation to matter).

Granted that neither can there be any internal reason to distinguish one part of space over another (because space is uniform) there can be no reason, period, why matter should be located at one part of space rather than another. If the parts of space are real, the PSR will have to be violated. So space is not real.

Here is one of Leibniz's shift arguments, stated in full:

> I say then, that if space was an absolute being, there would something happen for which it would be impossible there should be a sufficient reason. Which is against my axiom. And I prove it thus. Space is something absolutely uniform; and, without the things placed in it, one point of space does not absolutely differ in any respect whatsoever from another point of space. Now from hence it follows, (supposing space to be something in itself, besides the order of bodies among themselves,) that 'tis impossible there should be a reason, why God, preserving the same situations of bodies among themselves, should have placed them in space after one certain particular manner, and not otherwise; why everything was not placed the quite contrary way, for instance, by changing East into West.[10] But if space is nothing else, but that order or relation; and is nothing at all without bodies, but the possibility of placing them; then those two states, the one such as it now is, the other supposed to be the quite contrary way, would not at all differ from one another. Their difference therefore is only to be found in our chimerical supposition of the reality of the space in itself. But in truth the one would be exactly the same thing as the other, they being absolutely indiscernible; and consequently there is no room to enquire after a reason of the preference of the one to the other. (Leibniz, Third Paper, Alexander 1956, p.26).

Evidently the argument applies just as well to the other Galilean symmetries,

[10]We can also interpret Leibniz here to mean spatial inversion, whereupon - supposing it is an exact symmetry - it would follow that the world and its mirror image must be identified. For a defence of this conclusion, and the conseqeuences of parity violation in quantum theory, see my (2002a).

including boosts, and to the translations.

It is clear that there would be no implication that space is unreal if space did *not* have these symmetries. There would then be internal reasons - inhomogeneities - by which its parts might be discerned. Of course non-homogeneous spaces were not on offer in the early 18th century, but the reasons for that had little or nothing to do with Leibniz's principles. It should also be clear that the argument as stated does need Leibniz's independence thesis. It is this which rules out use of external reasons to distinguish parts of space - relations to material bodies, for instance. If relations to bodies were used Leibniz would say that space would not then qualify as a *bona fide* substance. We would not be considering space as it is in itself, but only in relation to other substances.

Leibniz's independence thesis has a resemblance to essentialism, in contemporary philosophy. Essentialists too insist that there is an important distinction between qualities of objects, namely those used to identify an object, its *essential* qualities, without which it would not be the same object, and those that can change, its *accidental* qualities. We have seen this in play in Maudlin's account of Newton's views on points of space. But the distinction between accidental and essential qualities does not generally line up with Leibniz's distinction, between internal and external relations: the connection is more apparent than real.

Unlike Leibniz's assumptions on the nature of geometry, it is not obvious that the independence thesis can be challenged by any empirical or mathematical discovery. But clearly it *can* be denied; we can allow for a broader notion of "object", whether or not objects will then be things in Leibniz's sense. Neither do we have to insist on a distinction between accidental and essential features or relations. Relationalism, as I shall understand it, is not committed to the independence thesis or to essentialism. It is, further, non-committal on the existence of any *a priori* symmetries to space or spacetime, and on any *a priori* distinction between space and matter. But it is committed to Leibniz's other metaphysical principles, in particular the PSR and PII.

4 The Identity of Indiscernibles

In fact it is hard nowadays to believe in Leibniz's independence thesis. The problem lies not with advances in physics but in logic. Leibniz's logic was based on the subject-predicate form of the proposition. He held that in every meaningful proposition there is a subject of predication, in parallel to the concept of substance as the bearer of properties. Where relations seem to be invoked, in reality one is still attributing a predicate to a single subject of predication: relations, for Leibniz, had to be *reducible* - derivable from the monadic properties of their relata (what Leibniz called *internal*, also sometimes called *intrinsic*, in contrast to *external* or *extrinsic* relations, which hold or fail to hold independent of any properties of their relata)

Leibniz's views on relations provided a clear basis for Leibniz's independence thesis. But whilst Frege, the founder of modern logic, drew a distinction superficially similar to Leibniz's (but between *object* and *concept*, rather than *subject* and *predicate*), in Frege's philosophy nothing of particular significance attached to 1-place concepts; relations were as fundamental to Frege's logic as was quantification theory; propositions no longer have to be cast into subject-predicate form to be meaningful;[11] there is nothing wrong with relations *per se*.

Consider again the PII, the principle, roughly speaking, that distinct objects must differ in some qualitative, predicative respect. This is a substantive thesis; a "qualitative" feature of an object can be common to many others; "qualitative identity" is the limiting case of similarity in all respects; qualitatively identical objects might yet be thought to differ numerically - but this is what is ruled out by the PII. The principle insists that there must be qualitative (and physically meaningful) criteria for numerical difference.

It is easy to say what identity is in set theory. Given a set U, it is the binary relation $\{< x, x >: x \in U\}$. But it is evident, since anyway we identify sets purely extensionally (they are defined by their elements), that

[11]It might appear from this that Leibniz's containment theory of truth is in just as much trouble, in modern logic, as is his independence thesis. In general that is correct, but it is possible to restrict the containment theory so that it only applies to *closed* physical systems and to *complete* descriptions of them. Frege's logic is consistent with this.

the set elements themselves are to be given in advance; in Cantor's words, sets are always "composed of definite well-distinguished objects" (Cantor 1895. p.481). Of course the PII may be violated for some language and for some collection of objects; the point is to find a language and a theory in that language which can say what each of them is (which admits no more and no less than what there is).[12] Then the PII will be satisfied.

What then does logic have to say on the matter? If there are only one-place predicates, as Leibniz thought, then the principle is just that objects with the same properties are identical. It has seemed obvious, in consequence, that admitting predicates in two or more variables, one weakens the principle to allow that objects may be counted as distinct if only they differ in their properties *or relations*. This version of the PII is normally called the *weak* principle; the former is called the *strong* principle. But the weak principle as just stated is not what logic dictates.

To see what does follow, from a purely logical point of view, consider the simplest case, a first order language without identity in which all the predicate symbols are explicitly specified.[13] Now introduce the identity sign, and supplement the laws of deduction accordingly, by the following axiom schema (here 'F' is a letter which can be replaced by any predicate of the language):

$$x = x;\ x = y \rightarrow (Fx \rightarrow Fy). \tag{2}$$

This schema implies *substitutivity*, the "indiscernibility of identity": terms (variables, logical constants, and functions of such) with the same reference can always be substituted without change of truth value. It is complete in the following sense: suppose a complete proof procedure is available for the original language (without identity), meaning that every logically true sentence (true in every model) can be deductively proved; then, supplement-

[12]The EPR completeness criterion is one half of this principle: "every element of the physical reality must have a counterpart in the physical theory" (Einstein *et al,* 1935 p.777).

[13]There is no difficulty in making the same definition in the second-order case, where one quantifies over predicates or properties and relations, so long as they do not include the identity sign or the relation of identity.

ing the proof procedure with the scheme (2), every valid sentence, including those involving the identity sign, can be proved. This is in fact how Gödel proved his celebrated completeness theorem for the predicate calculus with identity (Gödel 1930, Th.VI).

Any definition of identity which implies (2) will therefore be formally adequate for the purposes of deduction. Here then is how identity can be explained: for any terms 'x','y', $x = y$ if and only if for all unary predicates A, binary predicates B,...., n-ary predicates P, we have:

$$A(x) \longleftrightarrow A(y)$$

$$B(x, u_1) \leftrightarrow B(y, u_1),\ B(u_1, x) \leftrightarrow B(u_1, y) \tag{3}$$

$$\cdots \qquad\qquad\qquad \cdots$$

$$P(x, u_1, ..., u_{n-1}) \leftrightarrow P(y, u_1, ..., u_{n-1}), \text{ and permutations}$$

together with all generalizations (universal quantifications) over the free variables $u_1, ..., u_{n-1}$ other than x and y. From this (2) obviously follows.

This definition of identity is due to Hilbert and Bernays (1936), and was subsequently defended by Quine (1960); its consequences have not been widely recognized, however. They are straightforward if the language contains only one-place predicates; we then obtain the strong principle of identity as stated above. In the more general case, call two objects *absolutely discernible* if there is a formula with one free variable which applies to the one, but not to the other. There is another way for (3) to be false, so that $x \neq y$: call two objects *relatively discernible* if there is a formula in two free variables which applies to them in only one order. It should be clear that not all relatively discernible objects are absolutely discernible: the relation $x > y$ is true of any distinct real numbers, taken in only one order, although it is impossible to find finite expressions in a finite or countable alphabet which are in 1:1 correspondence with all of the reals.

But this case too falls under the weak principle of identity, since, for any two relatively discernible reals, at least they bear a different relation to each other. Objects can fail to be identity in a third way, however. To see this, suppose $B(x, y)$ is true, and that B is a symmetric predicate (so that $B(x, y)$ iff $B(y, x)$). Evidently B cannot be used to discern objects relatively. But (3) will still fail to hold if only B is *irreflexive* (so $B(x, x)$ is always false), for then there will exist a value of u_1 such that $B(u_1, x)$ is true but $B(u_1, y)$ is false, namely $u_1 = y$. Hence $x \neq y$. Call such objects *weakly discernible.*[14] This is the PII in accordance with modern logic: objects are numerically distinct only if absolutely, relatively, or weakly discernible.

Most of the classical counter-examples to Leibniz's principle, and all of the really convincing ones, turn out to be examples of weak discernibles (for example, Max Black's two iron globes, a certain distance from each other, each exactly the same, in an otherwise empty space) - so are not counter-examples to the PII as just stated. It is true that there is a quantum mechanical counter-example to it, namely elementary bosons all in exactly the same state, but fermions are always at least weakly discernible. Even in the most symmetric case, where the spatial part of the state has exact spherical symmetry, and the spin state is spherically symmetric too (as in the singlet state of two spin $\frac{1}{2}$ particles), fermions are weakly discernible: they satisfy the symmetric but irreflexive relation "...opposite component of spin to....". As for elementary bosons, with the exception of the Higgs particle, they are all gauge particles: the objects in such cases may well be better considered as the modes of the gauge field, with the number of quanta understood as excitation numbers instead.

Weak discernibles certainly could not be objects that we encounter in any ordinary way. By assumption, there is no physically meaningful predicate that applies to one of them, rather than to any other, so one cannot *refer* to any one of them singly. Call them *referentially indeterminate.*[15] But

[14]Quine missed this category earlier (Quine 1960 p.230), where he introduced the distinction between absolute and relative discernibles. He subsequently spoke of grades of *discriminability*, rather than discernibilty (Quine 1976), but I will not follow him in this.

[15]Not to be confused with Quine's doctrine of *indeterminacy of reference*, which applies to objects whether or not they are weakly discernible. Connections between the symmetries of model theory, as exploited by Quine, and the ones that we have been considering, have been drawn by Liu (1997) and Rynaziewitz (1996b); they have been found wanting by

apart from objects at the microscopic scale, there seems to be little possibility of encountering *macroscopic* weak discernibles, not at least if they are impenetrable; for in that case, in any universe with large-scale asymmetries, spatiotemporal relations with other objects will always differ among them (such objects will invariably be absolutely discernible).

5 Non-Reductive Relationalism

The new PII differs from the old entirely through the unrestricted use of relations - any that are physically real. Admitting relations in this way, we are clearly abandoning Leibniz's independence thesis. This changes the debate between Leibniz and Clarke. Consider for example Clarke's objection:

> Why this particular system of matter, should be created in one particular place, and that in another particular place; when, (all place being absolutely indifferent to all matter,) it would have been exactly the same thing *vice versa*, supposing the two systems (or the particles) of matter to be alike; there could be no other reason, but the mere will of God. (Clarke, Second Reply, Alexander 1956, p.20-21).

Clarke denied that the PSR could have the fundamental status that Leibniz ascribed to it. We have seen that in a uniform space the PSR is violated if particles have positions, for there can be no reason why particles should be in one position rather than another. Leibniz took this to be a *reductio* of the view that particles have positions, over and above their relations with each other; but that does not solve the problem just posed by Clarke, given that permutations are symmetries (given that the particles are all exactly alike), for the permutation changes not only the positions of particles, but which particle is related to which. Call it the *permutation argument.* Leibniz's response to it was to deny that there *could* be such a symmetry; it was to reject

Stachel (2001).

atomism altogether.[16] But an alternative response *is to allow that bodies can be identified by their relations to one another*; then a particular body is no more than a particular pattern-position. This is the modern, relationalist description of atoms; the price is that we abandon the independence thesis.

In fact, in this application, Clarke was just as committed to the independence thesis as was Leibniz. Neither could allow that the numerical identity of atoms could be settled by appeal to their external relations. And indeed a reduction of sorts has taken place: particles have been replaced by pattern positions, by nodes in a pattern. Taken independent of their relations with one another, one might have thought to still have a collection of objects, whether substances or bundles of properties or whatever; but under the PII that cannot be so.

In the case of diffeomorphism covariance, there is a reduction of a lesser sort: positions in space have been replaced by positions in patterns of values of fields. This time it is not so clear that these positions cannot be thought of as objects in their own right, independent of the patterns of field-values. Manifold points do, after all, still bear relations to one another, even when the metric and other fields are removed, for there remains the differentiable structure of the manifold, as defined by the atlas of charts. The local topology, the open sets and their relations under set membership, as inherited from the usual topology of R^4, is preserved by diffeomorphisms. It is the smoothness of the manifold which is independent of any fields on it; manifold points can be counted as distinct if and only they are contained in disjoint open sets (of course we are assuming the manifold is Hausdorff), and this too is a diffeomorphism-invariant condition, independent of any metrical structure. Were invariance under diffeomorphisms a sufficient condition for a relationship to be real, the manifold points would be counted as objects independent of their arrangement in patterns of fields.

In the case of the rigid symmetries, there is a reduction of yet another sort: the positions of objects in space have been replaced by the positions of objects relative to other objects - in the first instance, to positions of parti-

[16]It should be evident from this that "particle identity" figures just as much in classical statistical mechanics as quantum statistical mechanics (see e.g. Hestines 1970). That raises the question of just why quantum statistics differ at all from the classical case. The answer, of course, lies in the discreteness of the available energies.

cles. Absolute positions disappear; under the PII, points in space, considered independent of their relations with other points in space and with material particles, all disappear. But points in space considered independent of matter, but in relation to other points in space, are perfectly discernible (albeit weakly), for they bear non-reflexive metrical relationships with each other. There is no problem for the PSR in consequence; there is no further question as to which spatial point underlies which pattern-position, for they are only weakly discernible. Only in the case of absolute discernibles - in fact, only in the case of objects absolutely discernible by a subset of all the predicates available in a language - can their be any further question as to which object has which attribute.

In the case of a homogeneous space, spatial points cannot be absolutely discerned by the subset of predicates that apply to them alone. But since we have rejected the independence thesis, there is no reason in principle not to make use of other predicates as well, specifically those which apply to matter and events. Hence R0 follows: points of space and spacetime, in the symmetric case, are to be specified (absolutely discerned) by their relationships with matter and events respectively.[17]

Finally, I come back to the further symmetry principle urged by Mach: transformations to rotating frames of reference. At the end of the *Correspondence*, Clarke did at last challenge Leibniz to give an account of rotations. Rotational motion in NTG is possible even when all the relative distances at each time are exactly the same; this does in fact pose a problem for Leibniz's original principles. We can, indeed, recover a similar perspective to Leibniz's in modern, relationalist, terms, if only we assume that temporal relations are *reducible* - that time is in fact an internal or intrinsic relation (a relation that follow from monadic properties of its relata). For then it would follow that if two spatial configurations of particles or fields are not discernible in themselves (if they are not absolutely discernible), then they cannot be

[17]Leibniz might be taken to agree with me on this, when he says: "The parts of time or place, considered in themselves, are ideal things; and therefore they perfectly resemble one another, like two abstract units. But it is not so with two concrete ones, or with two real times, or two spaces filled up, that is, truly actual" (Fifth Paper, Alexander, p.63). (But of course, Leibniz did conclude from the fact that the parts of space and time in themselves perfectly resembled one another, that they are ideal, and hence not real; he did not grant that they were still weakly discernible.)

relatively or weakly discernible either. They would then be indiscernible, period, so counted as numerically one. So there could then be no such thing as a rotating system of particles or fields, in which the spatial configuration at each time were exactly the same. In other words, relationalism implies Machianism, but only if time is not an external relation.[18]

For Leibniz, of course, no real physical relation can be external, so he was committed to Machianism. Today we may allow that this question - of whether time is an external relation - is open; but it is no longer a logical thesis. Machianism is no longer grounded in logic.

6 Individuating Fields

A rose by any other name will smell as sweet. Earman has sketched a theory similar to mine; he calls it "resolute substantivalism". In his more recent collaborations with Belot (Belot and Earman 1999, 2001), it is called "sophisticated substantivalism". In their usage, substantivalism means realism. But according to Earman it faces a severe difficulty: if there are "multiple isomorphisms", cases where an object i in one spacetime model (or "world") Σ can be mapped onto distinct objects i_1, i_2, in a second world Σ', by one of two distinct isomorphisms ψ_1, ψ_2, then there is a problem if "identity follows isomorphism", as he puts it. If $i \in \Sigma$ is mapped onto $\psi_1(i) = i_1$, but is also mapped onto $\psi_2(i) = i_2 \neq i_1$, we have a contradiction, for surely identity is an equivalence relation. If so i_1 would be identical to i_2 as well, contrary to hypothesis (Earman 1989 p.198-99).

According to Earman the problem is quite intractable; all the most straightforward ways of making sense of this position are, he says, "indefensible". But on our framework there is no such difficulty. Nominally distinct worlds are in 1:1 correspondence with the elements of the symmetry group. If there are two distinct isomorphisms - group elements - then there are two nominally distinct worlds as well. The formal properties of identity parallel the formal properties of the relationship $\sim$, defined as:

[18] Julian Barbour, arch-Machian, has in recent years moved towards just this view of Machianism; see Barbour (1999).

$$\Sigma_1 \sim \Sigma_2 iff \exists g \in G such that g(\Sigma_1) = \Sigma_2. \quad (4)$$

The two distinct isomorphisms therefore map the object i into two nominally distinct worlds: $i_1 = g_1(i) \in \Sigma_1$, $i_2 = g_2(i) \in \Sigma_2$. There is indeed a map $i_1 \rightarrow i_2$, since (4) is an equivalence relationship (so long as the symmetry transformations form a group), but is a map between worlds: $g_3 : \Sigma_1 \rightarrow \Sigma_2$ (where $g_3 = g_2 \circ g_1^{-1}$). It is worlds, and objects across worlds, which are identified, not objects within a single world (unless of course they are indiscernible).

Hoefer (1996) has defended a view similar in some respects to mine. He likewise accepts that spatial points may be referentially indeterminate (that "primitive identity" may fail, as he puts it). Considering space independent of matter, there is no underdetermination as to where matter is to be located (no violation of the PSR), for if there is no primitive identity of spatial points then there is no matter of fact as to which point is to be occupied. With this I agree; but Hoefer denies that spatial points even considered together with matter can be referred to uniquely (by their relation to the matter distribution). He tacitly embraces Leibniz's independence thesis, whilst explicitly denying the PII. He believes his position is opposed to relationalism: his denial of primitive identity appears *ad hoc* in consequence. Certainly he is not in a position to motivate it on logical grounds, as I do, by the PII.

Relationalism, I say again, rejects the independence thesis and is diametrically opposed to absolutism. But it is neutral with respect to the contrast between matter and space. On an even-handed approach to matter and space, if we can use relations to space to individuate material bodies, then surely we can use relations with bodies to individuate parts of space.

Earman and Hoefer will at least agree with me on this: the hole argument is continuous with the arguments used by Newton, Leibniz and Clark. Stachel has argued otherwise, however. I come back to the nature of general covariance. Does the hole argument apply to NTG, written in diffeomorphism covariant form? Do not exactly the same conclusions follow as for GTR?

There are of course certain differences. In GTR, so long as symmetries are lacking (the physically realistic case), we can follow Stachel's suggestion (1993 p.156) to specify points in spacetime in purely chronogeometrical terms. We always have available at least four of the fourteen invariants ξ_k of the Riemann tensor (ten of them vanish *in vacuuo*); in the absence of symmetries, these quadruples of real numbers will generally differ at distinct points of M, and we can refer macroscopic objects in space-time to their values. The usual way of speaking of fields at points can be recovered as well. Say "the field ρ has value λ at the point $(\xi_1, \xi_2, \xi_3, \xi_4)$", and write "$\rho(\xi_1, \xi_2, \xi_3, \xi_4)=\lambda$", just when ρ has value λ where the Riemann scalars have values $(\xi_1, \xi_2, \xi_3, \xi_4)$; or, more parsimoniously, that the λ-value for ρ coincides with the ξ-values for the Riemann invariants.[19] Evidently in NTG this construction will not be of much use. The invariants built out of the chronogeometrical tensors will all be constant along the integral curves of the Killing vector fields. In this situation we will have to individuate points of these curves by reference to values of the gravitational field or, equivalently, by reference to material particles - assuming, of course, that these do not in turn have further symmetries. We will have to solve the dynamical equations of motion, and use these solutions to define a dynamical individuating field for space-time points. Throughout we will have to abide by the relational, qualitative approach to predication, tolerating weak discernibles should they arise.

But with all of this Stachel disagrees. He claims that on the contrary (i) The space-time structure of pregeneral-relativistic theories is most simply and economically written in terms of an affine geometry; it is to be defined by the linear transformations of an inhomogeneous affine space, not by a differentiable space-time manifold. (ii) An individuating field is required, granted, but it can be defined completely independent of the dynamical field equations of these theories. (iii) One can still rewrite NTG in diffeomorphism-covariant form, but when one does this the hole argument is completely trivial, and the general covariance employed is a sham (it is "trivial general covariance", corresponding to what I have been calling diffeomorphism covariance). (iv) One cannot apply the hole argument for pregeneral-relativistic theories written in affine-space-plus form (for the reasons we have already considered).

Let me give what ground I can. (i) is perfectly reasonable, but it turns out

[19]Such methods have been discussed at length by Rovelli (1991), and in the quantum case as well as in classical GTR.

- with Stachel's qualification when it comes to NTG - that he is speaking here only of electromagnetism, among the fundamental theories (I have made this point already); and even in that case there remain the various shift arguments (but admittedly not the generalized shift argument of NTG): his definition of the individuating field mentioned in (ii) will have to be consistent with these. (ii) is clearly the sticking point; Stachel flatly denies the claim that I am making: that as a matter of course the dynamical equations (for the matter fields) will have to be used to define an individuating field. On (iii) we can guardedly agree, but there remains the generalized shift argument. With (iv) I agree, but with the same proviso as with (i).

(ii) is the decisive point of disagreement; otherwise, given (i), (iii) and (iv), I wish only to see, in Stachel's account, how the shift and permutation arguments are to be dealt with, in accordance with his definition of the individuating field in (ii) (and in the case of NTG, how the generalized shift argument is to be dealt with). How does Stachel define a non-dynamical individuating field? In fact he makes use of the familiar quasi-operationalist approach due to Einstein, in terms of a collection of clocks and rigid bodies (although he disavows any commitment to operationalism thereby). By parallel transport of their direction vectors, as defined by the affine geometry, one sets up a unique tetrad field in spacetime, with its associated coordinate system. He adds that many alternative procedures are possible, for example the use of test particles and light rays "to mention only two other possibilities" (Stachel 1993 p.149). Which is used is not the important point, however; rather:

> The important point for present purposes is that the spacetime structures, as well as the individuating field mapped out with the help of these methods, are *independent* of any dynamical fields that are subsequently introduced in the spacetime. (Stachel 1993, p.149.)

There are points of contact here with Einstein's view on the matter:

> It is ...clear that the solid body and the clock do not in the conceptual edifice of physics play the part of irreducible elements,

> but that of composite structures, which must not play any independent part in theoretical physics. But it is my conviction that in the present stage of development of theoretical physics these concepts must still be employed as independent concepts; for we are still far from possessing such certain knowledge of the theoretical principles of atomic structure as to be able to construct solid bodies and clocks theoretically from elementary concepts. (Einstein 1921, 236-37).

Einstein is saying that we do not know how to provide a dynamical model of the individuating field, and that in this situation, as a stop-gap, we must treat rulers and clocks as "independent concepts" for determining spacetime intervals. Stachel, by contrast, denies that in pregeneral-relativistic theories it is a stop-gap. In such theories there is never any need for such a dynamical model. So long as we confine ourselves to Newtonian or special relativistic theories, we need never inquire as to the dynamical structure of the individuating field; we do not have to solve for the equations of motion, prior to determining what the coordinates mean.

But what exactly are these "independent concepts"? Stachel elsewhere calls them "ideal elements" (Stachel 1983 p.256); he is critical of the "empiricist, operationalist or instrumentalist spirit", according to which "...the ideal elements initially introduced...must be immediately... identifiable with objects used in laboratory tests of the theory". Evidently Stachel is not appealing to any concrete operational procedure, or any given technology. But now one wonders if, like the absolutist, it is a *purely* mathematical coordination that he has in mind. One wonders if they are ideal elements in *Leibniz's* sense. If the procedure has no operational definition, nor is it a matter of using the dynamical theory, then what does it consist in, so as to make any physical sense? One could of course make use of the dynamical individuating field of *some other* theory; this point I freely grant; but it is historically idle, if our concern is with NTG, and it is inconsequential, for surely our interest switches to this other theory. It is the other theory that will be the foundational one for our understanding of space and spacetime.

Another alternative is to make use of a dynamical individuating field defined by one application of the theory, to give content to the use of coordinates in the context of a *different* application of that same theory. This is, in fact, of methodological importance, because it makes clear why, if available, a non diffeomorphic-covariant formulation of a theory is to be preferred. We may grant that it is more convenient, more simple, to first define a dynamical individuating field, by providing a dynamical model for a given physical system, and then to make use of that particular physical system (a particular system of clocks and physical bodies) in other applications of the theory. (The policy of "divide and rule" is well-named.) What is required, of course, for this to be possible, is that the theory not be *generally* covariant, in Stachel's sense. Such a procedure is *not* applicable in the case of GTR. Is this Stachel's point, in sharply distinguishing other theories from GTR? If so one should not say the individuating field so introduced is non-dynamical; one should say, to coin a term, that the field is *external* to an application. The pregeneral-relativistic theories permit of external individuating fields; they will be dynamical fields all the same.

The treatment of referential indeterminateness is a litmus-test for the meaning of an individuating field more generally. Stachel has no comment to make on indeterminateness in affine-space-plus theories, but he does comment on the symmetric case in GTR:

> If a particular metric tensor field does have some symmetry group, the values of the four invariants [of the curvature tensor] will be the same at all points of an orbit of the symmetry group, so that additional individuating elements have to be introduced to distinguish between such points. (For example, the preferred parameters of the symmetry group, one of which is associated with each of the Killing vector fields that generates the symmetry group, may be used.) However chosen, such additional elements cannot be independent of the metric tensor field since the latter serves to define the orbits in question. (Stachel, 1993, p.143.)

As it stands these preferred parameters are purely mathematical artifacts. Stachel does not attend to their physical definition. Nowhere, so far as I know, does he acknowledge the possibility of indeterminateness of reference, in the extreme case where likewise the material distribution has the same symmetries (which is likely to follow in GTR, given exact global symmetries of the metric).

7 Individuating Time

Stachel sees no virtue in a purely operational non-dynamical individuating field, but others might. It was, in point of fact, a method of great use to astronomy, and to much of 19th century physics. Surely it has a bearing on our topic. I will close with some remarks on this option.

The issues are more straightforward in the case of time, so I will confine myself to that. For the notion of a purely operationally-defined clock, we can do no better than turn to the definition of Heinrich Hertz, that exemplar of 19th century experimental electromagnetism

> Rule 1: We determine the duration of time by means of a chronometer, from the number of beats of its pendulum. The unit of duration is settled by arbitrary convention. To specify any given instant, we use the time that has elapsed between it and a certain instant determined by a further arbitrary convention.
>
> This rule contains nothing empirical which can prevent us from considering time as an always independent and never dependent quantity which varies continuously from one value to another. The rule is also determinate and unique, except for the uncertainties which we always fail to eliminate from our experience, both past and future. (Hertz 1894 p.298.)

But there is nothing determinate and unique about the time kept by a pendulum clock, except and insofar as it is under theoretical control - exactly, that is, when the uncertainties in its behavior *are* eliminated from our experience. Evidently a good clock is one whose construction is guided by theory - in Hertz's time, mechanical theory. With that, we are on the road towards a dynamical individuating field after all.

The point is clearer if one is talking of the very best clocks, of the sort needed by astronomers. Here one might think that Hertz's parallel definition, of a spatial individuating field, was on the right lines, for he defined position and orientation by reference to the fixed stars. What then of Sidereal Time, time as defined by the diurnal motion of the Earth with respect to the stars? This is a big improvement on any pendulum clock. Of course, we know this will be a good clock because we have good reasons to view the rotation of the earth as uniform, which derive ultimately from NTG; so Sidereal Time has a dynamical underpinning of sorts. But we need hardly solve any equations of motion for it, in order to define this time standard. It is not much of a dynamical individuating field; and it is anyway an external field, in the sense that I have just defined. It is surely grist to Stachel's mill.

But good as Sidereal Time is, it is not good enough for astronomy - so long as we do not model it more precisely, and so long as we insist on treating it as an external individuating field. When we do model it more precisely, we find that the rotation of the earth is *not* perfectly uniform, and that it varies in very complicated ways with respect to a true measure of time. The effect is known as *nutation*. The main contribution to this was discovered by Bradley, in the early nineteenth century, with an amplitude of 9 seconds of arc every 18.6 years. This wobble of the Earth's axis shows up in a periodic shift in all stellar coordinates (declination and right ascension), so as long as the variation is periodic corrections to Sidereal Time can easily be made; the difficulty is that there are many other harmonics (the component studied by Bradley is due to the regression of the nodes of the moon's orbit). To isolate these, there is no alternative to using the full NTG, and thereby a dynamical individuating field. And, because one has to consider the Moon's orbit, and in principle the other celestial bodies as well, it is hardly an external individuating field that we end up with.

By the early 20th century, it was clear that Sidereal Time would not do for astronomy. What then replaced it? The answer was already to be found in Newton's procedure in the *Principia*. Newton's clock was the Solar System, specifically the Earth-Moon system, Jupiter, and the Sun. The *Principia*, as Newton said, was written to distinguish the true from the apparent motions, to tell us how this clock should be read. His procedure made use of no other data but relative distances and the angles of intersection of lines (rays of light). What other data are there? He modeled a part of the system under study to define an individuating field (therefore a dynamical individuating field), to which the coordinates of other quantities could be referred. In this he but followed Galileo. How is one to test the equation $h = \frac{1}{2}gt^2$ for a freely-falling body, when the only clocks available were hour-glasses, sundials and candles? Galileo's answer was simplicity itself. Conclude, from Galileo's mechanics, that this equation is unaffected if the body is given a horizontal component of velocity v; conclude that the horizontal distance d travelled is vt; then h is proportional to d^2. Galileo used a dynamical individuating field.

Newton's procedure was, of course, much more complicated, and it was very poorly understood. It was only with Lagrange's work, a century later, that the theoretical problem was solved completely; and it was another century, following the work of Simon Newcombe in the 1880s, before astronomers were making use of Lagrange's techniques. The result is *Ephemeris Time*; it is this which defined the SI unit of time until very recently. It is time defined as that parameter with respect to which Newton's equations hold good of the observed celestial motions. Other quantities, as functions of time, are then referred to those very motions. It is a dynamical individuating field *par excellence*.

There is a twist to this story. In 1976 Ephemeris Time was replaced by the atomic clock standard.[20] The two are, in fact, fully comparable in accuracy. With that, and for the first time, we are really in a position to do astronomy using a non-dynamical individuating field for the time coordinate. It is non-dynamical, of course, only with respect to *classical* theory; it is a dynamical individuating field from the point of view of quantum electrodynamics.[21]

If Hertz was oblivious to all of this, immersed as he was in a study of

[20]Known as *Temps Atomique International*, based on a free-running, data-controlled timescale (Échelle Atomique Libre), formed by combining data from all available high-precision atomic clocks (principally cesium beam standards and hydrogen masers). The accuracy is presently of the order of 1 part in 10^{14}, approximately the same as in Ephemeris Time, but it is likely to be considerably improved on by moving to optical frequencies. For more on astronomical time standards, see Seidelman (1992).

[21]Any discrepancies now, between these two dynamical individuating fields, will require a dynamical model of both kinds of clocks, therefore a theory of quantum gravity.

the foundations of mechanics,[22] it is hardly a surprise to find that the young Einstein was ignorant of it too. As Stachel says, the idea of a non-dynamical individuating field was one that Einstein had painfully to unlearn. But here he only followed tradition: from ancient times, when Hipparchus and Ptolemy first doubted that the stars were really fixed, the lesson had to be repeatedly learned that there is no perfect individuating field given to us observationally; and from Leibniz's time, that it cannot be given to us as an ideal thing, either. The history of dynamics is in large part the story of how we are to proceed in this situation, and define such a field all the same. A dynamical individuating field.

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[22]For the defects of Hertz's mechanical principles, see my (1998). Because of the equivalence principle, experiments in electromagnetism and, more generally, microscopic physics (including the definition of ETA) are insensitive to the choice of frame, but Hertz could not easily appeal to it; unlike Newton's principles, his did not imply it.

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