The Relevance of Irrelevance: Absolute Objects and the Jones-Geroch Dust Velocity Counterexample, with a Note on Spinors

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Abstract

James L. Anderson analyzed the conceptual novelty of Einstein's theory of gravity as its lack of "absolute objects." Michael Friedman's related concept of absolute objects has been criticized by Roger Jones and Robert Geroch for implausibly admitting as absolute the timelike 4-velocity field of dust in cosmological models in Einstein's theory. Using Nathan Rosen's action principle, I complete Anna Maidens's argument that the Jones-Geroch problem is not solved by requiring that absolute objects not be varied. Recalling Anderson's proscription of (globally) "irrelevant" variables that do no work (anywhere in any model), I generalize that proscription to locally irrelevant variables that do no work in some places in some models. This move vindicates Friedman's intuitions and removes the Jones-Geroch counterexample: some regions of some models of gravity with dust are dust-free, and there is no good reason to have a timelike dust 4-velocity vector there. Eliminating the irrelevant timelike vectors keeps the dust 4-velocity from counting as absolute by spoiling its neighborhood-by-neighborhood diffeomorphic equivalence to $(1, 0, 0, 0)$. A more fundamental Gerochian timelike vector field presents itself in gravity with spinors in the standard orthonormal tetrad formalism, though eliminating irrelevant fields might solve this problem as well.

keywords: absolute object, general covariance, dust, spinor, parametrized

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1 Introduction

James L. Anderson analyzed the novelty of Einstein's so-called General Theory of Relativity (GTR) as its lacking "absolute objects" (Anderson, 1967; Anderson, 1971). Metaphorically, absolute objects are often described as a fixed stage on which the dynamical actors play their parts. A review of Anderson's quite precise definitions will be useful. Absolute objects are to be contrasted with dynamical objects. The values of the absolute objects do not depend on the values of the dynamical objects, but the values of the dynamical objects do depend on the values of the absolute objects (Anderson, 1967) (p. 83). Both absolute objects and dynamical objects are, mathematically speaking, geometrical objects or parts thereof; the importance of this requirement will appear later.

Before absolute objects can be defined, the notion of a covariance group must be outlined. Here it will prove helpful to draw upon the unjustly neglected work of Kip Thorne, Alan Lightman, and David Lee (TLL) (Thorne et al., 1973); a useful companion paper (LLN) was written by Lee, Lightman and W.-T. Ni (Lee et al., 1974). The TLL definition differs slightly from Anderson's in its notion of faithfulness. According to TLL,

> A group $\mathcal{G}$ is a covariance group of a representation if (i) $\mathcal{G}$ maps [kinematically possible trajectories] of that representation into [kinematically possible trajectories]; (ii) the [kinematically possible trajectories] constitute "the basis of a faithful representation of $\mathcal{G}$" (i.e., no two elements of $\mathcal{G}$ produce identical mappings of the [kinematically possible trajectories]); (iii) $\mathcal{G}$ maps [dynamically possible trajectories] into [dynamically possible trajectories]. (Thorne et al., 1973) (p. 3567)

One can now define absolute objects. They are, according to Anderson, objects with components ϕ_α such that

> (1) The ϕ_α constitute the basis of a faithful realization of the covariance group of the theory. (2) Any ϕ_α that satisfies the equations of motion of the theory appears, together with all its transforms under the covariance group, in every equivalence class of [dynamically possible trajectories]. (Anderson, 1967) (p. 83)

Thus the components of the absolute objects are the same, up to equivalence under the covariance group,[1] in every model of the theory. It is the dynamical objects that distinguish the different equivalence classes of the dynamically possible trajectories (p.

[1]There seems to be no compelling reason to require a covariance group instead of a mere covariance groupoid, a structure that would be a group if it were meaningful to multiply every pair of elements. Einstein's equations on a background space-time, once one imposes a consistent notion of causality, have a covariance groupoid that is not a group (Pitts and Schieve, 2004).

84). One notices that the components of the absolute object need be the same, up to equivalence under the covariance group, for all *dynamically* possible trajectories, not all *kinematically* possible trajectories. Might this matter have gone otherwise? For most purposes this choice makes no difference, because typically those objects whose components are the same for all dynamically possible trajectories share the same feature for all kinematically possible trajectories. This condition fails, however, in the context of Rosen's deriving the flatness of a metric using a variational principle with Lagrange multipliers, as will appear below.

It has been asserted that the novel and nontrivial sense in which GTR is generally covariant is its lack of absolute objects or "prior geometry" (Misner et al., 1973) (pp. 429-431). John Norton discusses this claim with some sympathy (Norton, 1992; Norton, 1993; Norton, 1995), though technical problems such as the Jones-Geroch dust and Torretti constant spatial curvature counterexamples are among his worries (Norton, 1993; Norton, 1995). Anderson and Ronald Gautreau encapsulate the definition of an absolute object as an object that "affects the behavior of other objects but is not affected by these objects in turn." (Anderson and Gautreau, 1969) (p. 1657) Depending on how one construes "affects," this summary might be serviceable, but only if used very cautiously. On other occasions absolute objects are said to "influence" dynamical objects but not *vice versa* (Anderson, 1971) (p. 169). Such terminology echoes Einstein and implies that absolute objects violate what Anderson calls a "generalized principle of action and reaction" (Anderson, 1967) (p. 339) (Anderson, 1971) (p. 169). Norton has argued, rightly I think, that such a principle is hopelessly vague and arbitrary and that it should not be invoked to impart a spurious necessity to the contingent truth that our best current physical theory lacks them (Norton, 1993) (pp. 848, 849). One might also doubt whether terms such as "affects," "influence" and "act" adequately capture what absolute objects typically do. These terms suggest that the dynamical objects in question would have well-defined behavior if the absolute objects could somehow be 'turned off,' so to speak (perhaps by replacing them with zero in the equations of motion), and that if the absolute objects were 'turned on' again, they would alter the well-defined behavior of the dynamical objects in much the way that an applied electric field alters the motion of a charged particle. But in important examples, such as Newtonian physics or special relativity, turning off many or all of the absolute objects destroys the theory: the equations of motion become degenerate or meaningless. The absolute objects do not so much alter an otherwise happy situation as provide conditions in which the dynamical objects can have well-defined behavior. Perhaps the stage metaphor for absolute objects is deeper than it seemed: presumably actors could put on a play on a stage consisting of a rubbery sheet or a giant pillow, or perhaps act in mid-air while falling freely, but it is easier to act on a firm wooden stage. Thus the claim that absolute objects have some defect knowable *a priori* may be taken too seriously. The fact that it is even possible to do without them, as in Einstein's theory, should be something of a surprise.

In Anderson's framework, an important subgroup of a theory's covariance group is its symmetry group (Anderson, 1967) (pp. 84-88). One first defines the symmetry group of a *geometrical object* as those transformations that leave the object unchanged. If the transformations are infinitesimal space-time mappings, then the Lie derivative of the geometrical object with respect to the relevant vector field vanishes for symmetries. The symmetry group of a physical system or theory—Anderson makes no distinction between them here—is

> the largest subgroup of the covariance group of this theory, which is simultaneously the symmetry group of its absolute objects. In particular, if the theory has no absolute objects, then the symmetry group of the physical system under consideration is just the covariance group of this theory. (p. 87)

Thus, roughly speaking, the fewer absolute objects a theory has, the more of its covariance transformations are symmetry transformations. For the example of a massive real scalar field obeying the Klein-Gordon equation in flat space-time in arbitrary coordinates, the covariance group is the group of diffeomorphisms, while the symmetry group is the 10-parameter Poincaré group corresponding to the ten Killing vector fields of Minkowski space-time. For a massive real scalar field coupled to gravity in GTR, the covariance group is again the diffeomorphisms. The symmetry group is also the diffeomorphisms, because any diffeomorphism leaves the set of absolute objects invariant, trivially, because there are no absolute objects. The fact that the space-time metric in GTR + massive real scalar field has no symmetries in general, though quite true, plays no explicit role in determining the symmetry group of the theory because the space-time metric is dynamical rather than absolute.

Finding Anderson's definition obscure, Michael Friedman amended it in the interest of clarity (Friedman, 1973; Friedman, 1983). Friedman takes his definition to express Anderson's intuitions, so the target of analysis is shared between them. As it turns out, Friedman has made a number of changes to Anderson's definitions, most of which seem to have received little comment by him or others, so some comparison will be worthwhile.

First, though Friedman's and Anderson's equivalence relations are laid out somewhat differently, a key difference between them is that Friedman's equivalence relation, which he calls d-equivalence, comprises only diffeomorphism freedom (Friedman, 1983) (pp. 58-60), not other kinds of gauge freedom such as local Lorentz freedom or electromagnetic or Yang-Mills gauge freedom, in defining the covariance group. But local Lorentz freedom is a feature of the standard version of Einstein's GTR + spinors, for example. Anderson calls such groups besides diffeomorphisms "internal groups" (Anderson, 1967) (pp. 35, 36). I find no argument for Friedman's excluding internal groups from the relevant equivalence relation, so perhaps he was unaware of this departure from Anderson's work. The goal is to distinguish physical sameness from conventional variation in descriptive fluff. Because internal groups involve descriptive fluff as much as diffeomorphisms do, it seems that Anderson was more successful than Friedman on this point. The role of internal groups in Anderson's work seems to have escape Norton's notice (Norton, 1993) (pp. 847, 848).

Second, Friedman's mathematical language is less general than Anderson's and fails to accommodate some useful mathematical entities that Anderson's older component language permits. Anderson, a working physicist, knows what sorts of mathematical structures physicists actually use and need, while Friedman restricts his attention to that narrower collection of entities that all modern "coordinate-free" treatments of gravitation or (pseudo-)Riemannian geometry presently discuss, namely tensors and connections, but not, for example, tensor densities (though a few recent differential geometry texts actually do treat densities of arbitrary weight (Spivak, 1979; Okubo, 1987)). Tensor densities are useful and might be essential in some applications. In the literature on modern nonperturbative canonical quantization of gravity, with Ashtekar's new variables and the like, tensor densities are used routinely. Some authors write densities in a way that makes their weight manifest: a weight 2 density has tildes over it, a weight -1 density has a tilde below it, *etc.* Moreover, the use of a densitized lapse function has proven useful in 3+1-dimensional treatments of the initial value problem[2] in GTR and the dynamical preservation of the constraint equations (Jantzen, 2004; Anderson and York, 1998). Perhaps these uses of densities are matters of convenience rather than necessary, because one might simulate tensor densities of integral weights using tensors, perhaps with great heaps of indices (totally antisymmetric on various sets of three or four at a time), and then invent a notation for multiple 4-forms, multi-vectors, and the like to avoid writing such indices. In practice using densities evidently is easier for many authors. An analogous procedure has not been found (to my knowledge) and might be impossible for general *non-integral* weight tensor densities; it is unclear what a quantity with a third of an index or π indices would mean. But tensor densities of fractional weight are routinely used in the conformal-traceless decomposition of André Lichnerowicz and James York in solving GTR's initial value constraints in numerical general relativity (York, 1972; Brown, 2005). The conformal part of a metric, which has a fixed determinant (usually ± 1) when the components are treated as a matrix, is a tensor density with dimension-dependent and typically fractional density weight, while the conformal factor that converts a conformal metric into a full metric is a scalar density. Thus densities are useful, though perhaps not

[2] It is now customary in numerical general relativity to call the problem of inferring later or earlier states of a system from initial data the "Cauchy problem," while the term "initial value problem" is reserved for the procedure of solving the constraint equations to get a set of initial data. This latter sort of problem exists only for constrained theories like GTR or Maxwell's electromagnetism.

essential, in unimodular variants of GTR (discussed in (Unruh, 1989; Earman, 2003)), for example. Densities with *irrational* weights are, if not essential, at least very useful in work on massive variants of Einstein's GTR (Ogievetsky and Polubarinov, 1965; Pitts and Schieve, 2005). Thus Friedman's mathematical language does not accommodate quantities that physicists use and perhaps require. Friedman's mathematical language is also inadequate to express the techniques used by V. I. Ogievetskiĭ and I. V. Polubarinov in their atypical treatment of spinors coupled to gravity using a "square root of the metric" (Ogievetskiĭ and Polubarinov, 1965). This spinor formalism might be useful in preventing the timelike leg of the orthonormal tetrad, which typically used with spinors, from counting as an unwanted absolute object. Hermann Weyl protested in 1920 against early clumsy efforts at component-free formalisms "which are threatening the peace of even the technical scientist" (Weyl, 1952) (p. 54). Fortunately, recently some authors have accommodated densities in coordinate-free form (Fatibene et al., 1997; Fatibene and Francaviglia, 2003). Presumably a coordinate-free version of any geometric object could be defined if the need arose.

Third, while Friedman considers variously rich and spare versions of what is intuitively one theory (Newtonian gravity) and states a methodological preference for spare theories, his treatment lacks the firm resolve of Anderson's demand that "irrelevant" variables be eliminated. This requirement is also imposed by TLL (Thorne et al., 1973) and discussed by John Norton (Norton, 1993). One can readily adopt the Andersonian proscription of irrelevant variables to express Friedman's intuitions about "natural" choices of variables (Friedman, 1983) (p. 59) in relation to the Jones-Geroch dust counterexample.

A fourth difference pertains to the notion of standard formulations of a theory. Anderson argues (somewhat confusingly) that theories should be coordinate-covariant under arbitrary manifold mappings; this move seems to be offered as a substantive claim rather than a conventional choice. More understandably, TLL stipulate that the standard form of a theory be manifestly coordinate-covariant. Friedman, by contrast, takes as standard a form in which the absolute objects, if possible, have *constant components* (Friedman, 1983) (p. 60) and so have limited coordinate freedom. Friedman implies that one can always choose coordinates such that the absolute objects (a) have constant components and (b) thus drop out of the theory's differential equations, which then pertain to the dynamical objects alone. However, (a) is falsified by the counterexample of (anti-) de Sitter space-time as a background (Rosen, 1978; Logunov et al., 1991) for some *specific* curvature value. These space-times of constant curvature, at least for a fixed value of the curvature, satisfy Anderson's and Friedman's definitions of absolute objects, but the components of the metric cannot be reduced to a set of constants. What holds for space-time curvature also holds for spatial curvature. Anderson makes some effort to identify the 'correct' or best formulation of a theory, a task taken up in more detail by TLL (Thorne et al., 1973). The latter authors' "fully reduced generally covariant representation" of a theory, unlike Friedman's "standard formulation" (p. 60), retains the full coordinate freedom by leaving the absolute objects as world tensors (or tensor densities, connections, or whatever sort of geometric objects they are). Friedman's expectation that absolute objects be expressible using constant components is too strong to apply in every example. (b) is falsified by the example of massive versions of Einstein's theory (Ogievetsky and Polubarinov, 1965; Freund et al., 1969; Babak and Grishchuk, 2003; Pitts and Schieve, 2005). After a lull from the mid-1970s to the mid-1990s, massive variants of gravity have received considerable attention from physicists lately, especially particle physicists. In those theories where the background space-time metric is flat, its components can be reduced to a set of constants globally by a choice of coordinates, but the background metric still does not disappear from the field equations because it appears in them algebraically, not merely differentially as Friedman apparently assumed tacitly. Especially because (a) is false, the Thorne-Lee-Lightman fully reduced generally covariant formulation is therefore preferable to Friedman's standard formulation, which fails to exist in some interesting examples. However, if one's goal is more historical, so that Newtonian gravity and special relativity without gravity are the main theories of interest, then Friedman's standard formulation suffices to illustrate the role of the Galilean and Poincaré groups, respectively.

Friedman's expectation that the components of absolute objects could be reduced to constants in general, though incorrect, usefully calls attention to the role (or lack thereof) of Killing vector fields and the like in analyzing absolute objects. If the (anti-) de Sitter space-time examples show that constancy of components is too strict a criterion, the next best thing is to have a maximal set of 10 Killing vector fields in four space-time dimensions, whether commuting as in the flat space-time case or not as in the (anti-) de Sitter case. One could generalize in various ways (Kramer et al., 1980). Because absolute objects need not be metric tensors, the general notion is not Killing vector fields, but generalized Killing vector fields, fields such that the Lie derivative of the absolute objects vanishes. Certainly some notion of constancy is one of the core intuitions that one has about absolute objects. Thus all standard examples of absolute objects will likely have a fair number of generalized Killing vector fields. In Anderson's terminology, most theories will have fairly large symmetry groups. Typically at least a 7-parameter family of space- and time-translations and spatial rotations will be in the symmetry group, as in classical mechanics (Goldstein, 1980). In GTR (including suitable matter fields), the lack of absolute objects implies a vast symmetry group. This large group of all diffeomorphisms as symmetries of the absolute objects, in turn, leads to an embarrassment of riches concerning local conservation laws, albeit noncovariant and not unique (Anderson, 1967) (pp. 425, 426). From this fact follows the so-called nonlocalizability of gravitational energy. The expectation of time-translation invariance excludes most Robertson-Walker metrics, so this criterion might be invoked to exclude a counterexample of Norton's (Norton, 1993) (p. 848). The most typical and plausible examples of absolute objects do not apply forces that violate conservation laws; those that do, might well be called miraculous. Norton's main point in this example (which is inspired by Torretti), that one can collect various constant curvature spaces together in a single theory and thereby not satisfy Anderson's criterion for absolute objects, will be discussed below.

2 Jones-Geroch Counterexample and Friedman's Reply

With a clear grasp of absolute objects in hand, one can now consider the Jones-Geroch counterexample that claims that the 4-velocity of cosmic dust counts, absurdly, as an absolute object by Friedman's or Anderson's standards. Friedman concedes some force to this objection made by Robert Geroch and amplified by Roger Jones, here related by Friedman:

> ... [A]s Robert Geroch has observed, since any two timelike, nowhere-vanishing vector fields defined on a relativistic space-time are d-equivalent, it follows that any such vector field counts as an absolute object according to [Friedman's criterion]; and this is surely counter-intuitive. Fortunately, however, this problem does not arise in the context of any of the space-time theories I discuss. It could arise in the general relativistic theory of "dust" if we formulate the theory in terms of a quintuple $\langle M, D, g, \rho, U\rangle$, where ρ is the density of the "dust" and U is its velocity field. U is nonvanishing and thus would count as an absolute object by my definition. But here it seems more natural to formulate the theory as a quadruple $\langle M, D, g, \rho U\rangle$ where ρU is the momentum field of the "dust." Since ρU does vanish in some models, it will not be absolute. (Geroch's observation was conveyed to me by Roger Jones, who also suggested the example of the general relativistic theory of "dust."...) (Friedman, 1983) (p. 59)

Here D is the torsion-free covariant derivative compatible with g. Other sources, including what Roger Jones reported hearing from Robert Geroch, indicate a qualification to *local* diffeomorphic equivalence of nonvanishing timelike vector fields (Jones, 1981b) (pp. 167, 168) (Jones, 1981a; Trautman, 1965) (p. 84) (Wald, 1984) (p. 18) (Dodson and Poston, 1991) (pp. 198-200). In any case nothing in my argument will depend on global *versus* merely local equivalence between arbitrary neighborhoods. Jones also distinguishes the local diffeomorphic equivalence of nonvanishing timelike vector fields, which holds in general, from the (local) diffeomorphic equivalence of their covariant derivatives of various orders, which typically does not hold.

Below I will argue that Friedman's response is nearly satisfactory, though it has two weaknesses as he expressed it. First, the statement "ρU does vanish in some models" ought to have said "ρU does vanish in some neighborhoods in some models" to show that he is considering only genuine models of GTR + dust (in which dust vanishes in some neighborhoods in some models), rather than some models with (omnipresent?) dust and some degenerate models which nominally have dust but actually have no dust anywhere. The latter would seem to be a cheat. As it stands, the reader is left to wonder whether such a cheat is doing important work for Friedman (though John Norton correctly read Friedman's proposal as "relying . . . on the possibility that ρ vanishes somewhere" (Norton, 1993) (p. 848)). Clearly some models with dust have neighborhoods lacking dust, and it is these models which will prevent the dust 4-velocity from constituting an absolute object. Second, Friedman's unfortunate notation ρU suggests that the mass current density (which I will call J^{μ}) is logically posterior to ρ and an everywhere nonvanishing timelike U^{μ}. If so, then one has not eliminated the absolute object after all. If a timelike nowhere vanishing U^{μ} exists in the theory, then it is absolute even if ρU^{μ} vanishes somewhere and so is not absolute. Thus the relevance of Friedman's use of ρU^{μ} is left obscure. Instead one can take J^{μ} to be the fundamental variable, while the timelike U^{μ} is a derived quantity defined wherever $\rho \neq 0$. Alternatively, one can take U^{μ} to be meaningful everywhere (and perhaps primitive), but vanishing where there is no dust. If Friedman had said that J^{μ} or U^{μ} "does vanish in some neighborhoods in some models," then these two infelicities would have been avoided. Perhaps these expository imperfections led Roberto Torretti to judge Friedman's reply *ad hoc* (Torretti, 1984) and John Norton to call it "a rather contrived escape" (Norton, 1993) (p. 848). Once these problems are removed, the merit of Friedman's intuition shines brightly.

Below I shall review more discussion of this counterexample in the philosophical literature. Various neglected items from the physics literature will shed light on long-standing philosophical debates about absolute objects. Using the term "variational" for objects which are varied in an action principle (Gotay et al., 2004), one can safely follow Anderson in making "absolute" and "dynamical" mutually exclusive, while leaving open the connection between absoluteness and nonvariationality. It will be shown that there exist theories with variational absolute objects, at least if one does not exclude Rosen's variational principle as somehow illegal. Such a theory can be obtained using Rosen's trick to fulfill Maidens's claim that the absolute special relativistic metric could be obtained variationally. However these theories arguably violate Anderson's demand to eliminate redundant variables. A natural extension of the proscription of irrelevant variables serves to eliminate the Jones-Geroch counterexample: the dust 4-velocity U^{μ} does not count as an absolute object for GTR + dust because U^{μ} does not exist where there is no dust.

The final section will consider briefly some further issues that face the Anderson-Friedman absolute objects program. Among these are the neglected issues of the clock fields of parametrized theories and, more importantly, spinor fields coupled to gravity. While a timelike nowhere vanishing vector field does not exist in every model for the theories that Friedman considers, avoiding such an entity when coupling a spinor field to gravity is widely rumored to be impossible. Thus the connection between spinors and absolute objects is of considerable importance. While absolute objects and dynamical objects are mutually exclusive, it is useful to have the third category of confined objects as well (Thorne et al., 1973); these three categories are mutually exclusive and exhaustive, evidently. Some entities that seemed intuitively absolute but do not satisfy Anderson's definition fit into the category of confined objects. Also Roberto Torretti's example of a theory with spatial metrics of various constant curvatures will be considered briefly.

3 Hiskes's Redefinition of Absoluteness, Maidens's Worry, and Rosen's Answer in Advance

Anne Hiskes proposed amending the definition of absolute objects so that no field varied in a theory's action principle would be regarded as absolute (Hiskes, 1984). Such a move makes use of what *prima facie* seems to be a true generalization about absolute and dynamical objects. This intuition was shared by the master. Anderson wrote:

> In addition to the differences between absolute and dynamical objects discussed in Section 4-3 there is another important difference that appears to be characteristic of these two types of objects. The equations of motion for the dynamical objects can often be derived from a variational principle, especially if these objects are fields. On the other hand, it appears to be the case, although we can give no proof of the assertion, that the equations of motion for the absolute objects do not have this property.... In the following discussion we will assume that the equations of motion for the dynamical objects of a theory follow from a variational principle and that those for the absolute elements do not. (Anderson, 1967) (pp. 88, 89)

Thus Anderson suspected that most or all dynamical objects are variational, while no absolute object is variational. Similar intuitions are manifest in the TLL and LLN papers (Thorne et al., 1973; Lee et al., 1974). Such a requirement also appears in their notion of being "Lagrangian-based" (Thorne et al., 1973) (p. 3573). Recently John Earman has found it convenient to use "absolute" to mean non-variational. (Earman, 2003) Anderson was quite sensitive to the possibility of reformulating what intuitively seems like the same theory using various different sets, and indeed increasingly large sets, of variables in an action principle (Anderson, 1967) (section 4.2). Unlike Hiskes, he strove to define a unique correct formulation that gave the expected answers.

More recently, Anna Maidens has entertained the idea that Hiskes's redefinition could be deployed to remove the Jones-Geroch counterexample (Maidens, 1998). If absolute objects must be nonvariational, while the dust 4-velocity is variational, then the dust 4-velocity is not absolute. Following Hawking and Ellis (Hawking and Ellis, 1973), Maidens indicates how the equations for the timelike vector field can be derived from a variational principle.[3] However Maidens is also sensitive to the large variety of choices of variables and even the number of field components in an action principle for what intuitively counts as a single theory. Thus she expected such a use of Hiskes's redefinition to fail, because it eliminates the Jones-Geroch counterexample at the cost of introducing a new one. More specifically, Maidens has suggested that there might be some way to reformulate special relativistic theories such that the flat metric, which surely ought to count as absolute, is varied in the action principle. If that could be done, then Hiskes's definition of absolute objects would prove to be too strict (the opposite problem from what the Jones-Geroch example suggests about Friedman's), because it fails to count the metric tensor of special relativity as an absolute object. (Maidens presumably should envision a weakly generally covariant formulation of special relativity, though her notation is far from clear on that point.) "At this stage, however, we find a fly in the ointment, for its turns out that given suitable starting assumptions we can derive the Lorentz metric from an action principle." (Maidens, 1998) (p. 262) Supporting such claims would involve actually displaying a suitable Lagrangian density whose Euler-Lagrange equations give the desired results or else citing a source where such work had been done. Surprisingly, she fails to do either one. Success would involve finding an action principle for which the flatness of the metric holds for *all* models (her case (c)), not just some (her case (a), p. 265). A bit later she finds that "it is an open question as to whether the metric of special relativity is derivable from an action principle." (p. 266) Two pages later she once again claims that "some of the physically necessary fixed background, e.g. the Lorentz metric, can also be derived from an action principle." (p. 268) It is not easy to harmonize these fluctuating statements.

Fortunately Maidens's expectation that the flatness of a metric (for all models) can be derived from a variational principle is in fact correct. The question was resolved by

[3]One notices that Hawking and Ellis use a fluid variational principle with constrained variations, not the more familiar unconstrained variations. In some respects this is a disadvantage, though Schutz and Sorkin observe that it keeps one closer to the physical variables (Schutz and Sorkin, 1977). They also observe that in many cases, including this one, one can eliminate the constraints on the variation (not to be confused with constraints in the sense of gauge theories (Sundermeyer, 1982)) using Lagrange multipliers. It seems to me that John Ray's variational principle might be preferable in the present context, because it involves varying U^{μ} itself and uses unconstrained variations (Ray, 1972).

Nathan Rosen's in the 1960s (Rosen, 1966; Rosen, 1973). He used an action principle involving a Lagrange multiplier field with 20 components, a trick recently used also by Rafael Sorkin (Sorkin, 2002). Thus requiring absolute objects to be nonvariational gives an excessively strict definition, so the Jones-Geroch counterexample is not adequately addressed thereby. Some objects that should count as absolute can be variational, as Maidens expected. Rosen includes the following term in an action principle (after a change in notation to $\eta_{\mu\nu}$ for the metric in question, which is *a priori* arbitrary apart from the signature) to force $\eta_{\mu\nu}$ to be flat:

$$S = \int d^4x \sqrt{-\eta} R_{\rho\mu\nu\sigma}[\eta] P^{\rho\mu\nu\sigma}. \tag{1}$$

This term is intended as a supplement to the action for a special relativistic theory, within which now $\eta_{\mu\nu}$ would be subject to variation as well. $P^{\rho\mu\nu\sigma}$, a tensor with the same symmetries as the Riemann tensor for $\eta_{\mu\nu}$, serves as a Lagrange multiplier. Varying $P^{\rho\mu\nu\sigma}$ immediately yields the flatness of $\eta_{\mu\nu}$. Varying $\eta_{\mu\nu}$ takes more work and gives an equation of motion especially involving the second derivatives of $P^{\rho\mu\nu\sigma}$. That equation is not needed here. Rosen seems to make secret use of the Euler-Lagrange equation from $P^{\rho\mu\nu\sigma}$ to discard terms involving $R_{\rho\mu\nu\sigma}[\eta]$ in his equations 10, 11, and 12; if so, then his equation 12 is not an "identity" as he claims. Alternately, he might be taking the metric to be flat before the variation but curved after it, as Sorkin proposes (Sorkin, 2002), if that is an intelligible alternative.[4] As was noted above, Anderson's requiring component equality (up to equivalence under the covariance group) only for *dynamically* possible trajectories is relevant here. Using Rosen's trick, one has a geometric object such that its components agree for dynamically possible trajectories (on-shell, as physicists say) but not for kinematically possible trajectories (off-shell), because the metric is not flat for all kinematically possible trajectories.

Anderson briefly states that one must remove irrelevant variables from the theory under analysis. He writes:

> It is possible that a subset of the components of the [geometrical object characterizing the kinematically possible trajectories of the theory] do not appear in the equations of motion for the remaining components and furthermore can be eliminated from the theory without altering the structure of its equivalence classes. Such a subset is obviously irrelevant to the theory. We shall assume, therefore, that no subset of the components of [that geometrical object] is irrelevant in this sense." (Anderson, 1967) (p. 83)

Likewise TLL exclude the category of irrelevant variables (Thorne et al., 1973) (p. 3569). Anderson observes that

[4] It is perhaps worth noting that varying $P^{\rho\mu\nu\sigma}$ gives an equation of motion for $\eta_{\mu\nu}$ and varying $\eta_{\mu\nu}$ gives an equation of motion primarily for $P^{\rho\mu\nu\sigma}$. Thus one should avoid expressions like "the equations of motion for $\eta_{\mu\nu}$" or "the equations of motion for $P^{\rho\mu\nu\sigma}$" due to their ambiguity.

> one can always construct a hierarchy of theories all of which have the same equivalence-class structure in the sense that the equivalence classes of these theories can be put into one-to-one correspondence with each other. Two theories of such a hierarchy will differ both with regard to the mathematical quantities that describe their respective [kinematically possible trajectories] and their respective covariance groups. However, the set of mathematical quantities that describe the [kinematically possible trajectories] of a given theory in such a hierarchy will contain, as subsets, those of each theory that precedes it in the hierarchy. Likewise, its covariance group will contain, as a subgroup, the covariance group of each preceding theory.. . .
>
> The question then arises as to which theory of a hierarchy one should use to describe a given physical system. The answer rests, of course, in the final analysis, on the measurements that one can make on the system. It is necessary that each quantity used to describe the [kinematically possible trajectories] of a theory must, at least in principle, be measurable. (Anderson, 1967) (p. 81)

Similar thoughts appear elsewhere in the text (pp. 306, 340). This requirement of observability, an unfortunate whiff of verificationism, presupposes that all the physics resides in the field equations.[5] But typically, fields that do useful work are observable, and Anderson's requirement of observability, if not entirely on target, at least emphasizes the importance of excluding idle fields, such as $P^{\rho\mu\nu\sigma}$ appears to be.

While Rosen's trick vindicates Maidens's assertion that building nonvariationality into the notion of absolute objects is unsuccessful, Anderson appears to have the resources to exclude Rosen's trick as a form of cheating. Anderson's prohibition of irrelevant variables appears to exclude theories making use of Rosen's trick, because the dynamical evolution of the Lagrange multipliers $P^{\rho\mu\nu\sigma}$ has no effect on any other fields, whether gravitational or matter. $P^{\rho\mu\nu\sigma}$ appears to do nothing useful by Anderson's standards. Making $\eta_{\mu\nu}$ variational and yet absolute could perhaps be useful in

[5]This last claim Anderson elsewhere implicitly appears to contradict when he considers boundary conditions (p. 75) and suggests (using "furthermore" on p. 83), surprisingly, that there could exist fields that do not appear in other fields' equations of motion, but which help to determine the structure of the theory's equivalence classes. As it happens, recent work on field formulations of Einstein's equations provides an example: the flat metric does not appear essentially in the field equations, but it plays a role in the boundary conditions, topology, and the notion of gauge transformations (Pitts and Schieve, 2004). Boundary conditions are important in string theory as well (Braga et al., 2005). Thus Anderson is overly hasty in eliminating the background metric after deriving Einstein's equations in flat space-time (Anderson, 1967) (pp. 303-306) in the fashion of Kraichnan (Kraichnan, 1955). While Kraichnan's use of a background metric in no way requires that quantization occur by covariant perturbation theory, historically the two projects have been linked in the minds of many. Anderson critiqued perturbative approaches to Einstein's equations in response to a paper by Richard Arnowitt (Arnowitt, 1963).

that it lets one treat the theory readily using the existing constrained dynamics formalism (*e.g.*, (Sundermeyer, 1982)), which has not made much room for nonvariational fields. Making $\eta_{\mu\nu}$ variational also allows one to define a conserved symmetric stress energy tensor without using the formal trick of the Rosenfeld approach, in which one replaces the flat metric by a curved one for taking a functional derivative and then restores flatness afterwards (Deser, 1970). Whether Rosen's trick or the Rosenfeld trick is preferable is open to discussion, but the Andersonian elimination of the Lagrange multipliers as irrelevant is at least a defensible view.

Where does this dialectic leave us? Maidens proposed and rejected using Hiskes's redefinition of absolute objects to exclude the Jones-Geroch counterexample to Friedman's account of absolute objects. Maidens's missing proof was supplied in advance by Rosen. But Rosen's trick seems not to count against Anderson's version of the intuition that absolute objects are nonvariational, because Anderson wisely has criteria for eliminating irrelevant variables. Does it follow that Anderson's intuition, in the larger context of his project that excludes irrelevant variables, is vindicated? That is, if we accept Anderson's definitions and proscriptions, should we also accept his intuition that fields are variational if and *only if* they are dynamical? As it turns out, Anderson's generalization survives this alleged counterexample but might be threatened by another in which all fields are variational but there is still an absolute object. I have in mind parametrized theories (Sundermeyer, 1982; Kuchař, 1973; Schmelzer, 2000; Arkani-Hamed et al., 2003; Norton, 2003; Earman, 2003), in which preferred coordinates are rendered variational. One often calls the results "clock fields." Perhaps some uses of clock fields could be excluded as irrelevant—not because the fields themselves are irrelevant, but because perhaps their variationality is. On the other hand, if clock fields are used to satisfy an appropriate notion of causality in bimetric theories like massive variants of Einstein's equations (Pitts and Schieve, 2005; Pitts and Schieve, 2004; Schmelzer, 2000), then their variationality is relevant. Parametrized theories require more discussion than is appropriate here, however. Spinor fields also pose a difficulty, one which I intend to resolve on another occasion.

4 Eliminating Local Irrelevance Excludes the Geroch-Jones Vector Field

If Maidens's proposed and rejected use of Hiskes's redefinition is set aside for violation of Anderson's prohibition of redundant variables, then the Jones-Geroch counterexample still remains to be addressed. Now it turns out that Anderson's and TLL's proscription of irrelevant variables, if it does not quite remove the Jones-Geroch counterexample, at least inspires a gentle amendment that does the job. This amendment seems especially appropriate after one notices that TLL replace (Thorne et al., 1973)

(p. 3566) Anderson's notion of geometrical object (Anderson, 1967) (pp. 14-16) with Andrzej Trautman's notion of a geometric object (Trautman, 1965). Presumably both notions aim to capture the same intuition.

Given the relative inaccessibility of Trautman's lectures, it will be worthwhile to quote his definition of geometric objects in detail:

> Let X be an n-dimensional differentiable manifold.... [S]ince tensors are not sufficient for all purposes in geometry and physics, [*sic*] for example scalar densities are not tensors, to avoid having to expand definitions and theorems whenever we need a new type of entity, it is convenient to define a more general entity, the geometric object, which includes nearly all the entities needed in geometry and physics, so that definitions and theorems can be given in terms of geometric objects so as to hold for all the more specialized cases that we may require.
>
> Let $p \in X$ be an arbitrary point of X and let $\{x^a\}, \{x^{a'}\}$ be two systems of local coordinates around p. A geometric object field y is a correspondence
>
> $$y : (p, \{x^a\}) \rightarrow (y_1, y_2, \cdots y_N) \in R^N$$
>
> which associates with every point $p \in X$ and every system of local coordinates $\{x^a\}$ around p, a set of N real numbers, together with a rule which determines $(y_{1'}, \cdots y_{N'})$, given by
>
> $$y : (p, \{x^{a'}\}) \rightarrow (y_{1'}, \cdots y_{N'}) \in R^N$$
>
> in terms of the $(y_1, y_2, \cdots y_N)$ and the values of [*sic*] p of the functions and their partial derivatives which relate the coordinate systems $\{x^a\}$ and $\{x^{a'}\}$.... The N numbers $(y_1, \cdots \cdot y_N)$ are called the components of y at p with respect to the coordinates $\{x^a\}$. (Trautman, 1965) (pp. 84, 85)

Trautman then notes that spinors are not geometric objects. He also notes that some objects that are not themselves geometric objects are nonetheless *parts* of geometric objects. *Pace* Friedman's nonstandard usage (Friedman, 1983) (p. 359), the class of geometric objects is not exhausted by tensors and connections. Geometric objects were considered with great thoroughness by Albert Nijenhuis (Nijenhuis, 1952). A more recent treatment of them using modern differential geometry has been given by Marco Ferraris, Mauro Francaviglia, and Cesare Reina (Ferraris et al., 1983).

The reader will notice that Trautman's geometric objects are defined at every point in the space-time manifold. That fact is of special relevance for the dust example, because it implies that if a dust 4-velocity timelike unit vector field U^μ is used as a variable in the theory, then a dust 4-velocity timelike unit vector must be defined at every point in every model, *even if no dust exists in some neighborhoods in some* *models*. Here one recalls Anderson's and TLL's call for the elimination of irrelevant variables; Friedman also recognizes the value of eliminating surplus structure. It is not clear that existing notions of irrelevance apply strictly to the present case. The dust 4-velocity is locally irrelevant, not globally irrelevant, one might say. Perhaps the authors had in mind fields that satisfy equations somewhat like the Klein-Gordon equation as their primary examples, as physicists often do; for such fields irrelevance is likely to be global. But now that the question is raised, it does seem clear that wherever there is no dust, there ought not to be a dust 4-velocity timelike unit vector either—at least not if the task at hand is testing theories for absolute objects.

There seem to be three initially plausible alternatives concerning the dust 4-velocity where the dust has holes in some model. First, one might retain a timelike 4-velocity vector even in holes in the dust, while expecting the 4-velocity values in the dust holes to be mere gauge fluff. It is noteworthy that at least some perfect fluid variational principles in the physics literature yield timelike unit vector 4-velocities even where there is no fluid (Ray, 1972). Perhaps mathematical convenience commends this option, though I have shown that Ray's variational principle can be modified to lack a timelike 4-velocity in holes in the fluid. Presumably one could show that the value of a timelike 4-velocity vector is in fact gauge fluff in dust holes by using the Dirac-Bergmann constrained dynamics technology (Sundermeyer, 1982), though one might run into technical challenges with changes of rank or with the noncanonical Poisson brackets that can appear in fluid mechanics (Morrison, 1998). In any case, the timelike dust 4-velocity in dust holes has no physical meaning, yet leads one to conclude that the theory has an absolute object. Clearly any absolute object whose existence is inferred only by using physically meaningless quantities is spurious. If one allowed physically meaningless entities into a theory while testing for absolute objects, then one could take any theory and construct an empirically equivalent theory with as many absolute objects as one wants. One could concoct a version of GTR with Newton's absolute space, for example. To permit such a procedure is just to give up Anderson's program of analyzing the uniqueness of GTR, because analysis involves *trying* to get the intuitively known right answer as a consequence of some criteria. Anderson and TLL call for the elimination of irrelevant variables for just this sort of problem. One might call the entities that they reject "globally irrelevant variables" because such entities play no role at any space-time point in any model. The Jones-Geroch example shows, I conclude, that one must also exclude "locally irrelevant variables," entities that play no role at some space-time points in some models.

The two remaining options avoid this spurious absolute object in different ways. One option is to take the mass current density J^μ to be the primitive variable and regard U^μ and the dust density ρ as derived. Then ρ is defined by $\rho = \sqrt{-J^\mu g_{\mu\nu} J^\nu}$.

The 4-velocity U^μ is naturally defined by

$$U^\mu = \frac{J^\mu}{\sqrt{-J^\nu g_{\nu\alpha} J^\alpha}},$$

so U^μ is only meaningful where the denominator ρ is nonzero. That consequence is plausible on physical grounds and blocks the Jones-Geroch counterexample. The theory is thus formulated using a quadruple $\langle M, D, g, J \rangle$, not Friedman's quadruple $\langle M, D, g, \rho U \rangle$ or the quintuple $\langle M, D, g, \rho, U \rangle$. In some models J^μ vanishes at some space-time points in some models of GTR + dust, so U^μ is undefined in such cases. Neither J^μ nor U^μ is a Gerochian nowhere vanishing timelike vector field for all models. By contrast, the mass current density J^μ, which is equal to ρU^μ where $\rho \neq 0$, automatically vanishes where there is no dust and is continuous at the transition from dust to vacuum. Thus Friedman's suggestion that it is more "natural" to use the mass current density, once freed from the two infelicities noted at the beginning, is seen to be very reasonable.

The other option is to take U^μ to be meaningful but vanishing in those places in certain models where the dust has holes.[6] Although the dust 4-velocity exists everywhere, it vanishes in some places in some models, so not every neighborhood in every model has dust 4-velocity that is gauge-equivalent to $(1, 0, 0, 0)$. Anderson's definition of absolute object requires that, for any component ϕ_α of an absolute object in a theory, "[a]ny ϕ_α that satisfies the equations of motion of the theory appears, together with all its transforms under the covariance group, in every equivalence class of [dynamically possible trajectories]." (Anderson, 1967) (p. 83) Even if we drop Anderson's requirement of global equivalence in favor of Hiskes's local equivalence, the dust 4-velocity does not count as absolute. In dust-filled regions in a model, the dust 4-velocity is diffeomorphic (at least in a neighborhood) to $(1, 0, 0, 0)$, but in dust holes the 4-velocity is diffeomorphic to $(0, 0, 0, 0)$ instead. Thus U^μ, like J^μ, is not an absolute object. One might tolerate as harmless the surplus structure embodied in the vanishing dust 4-velocity vectors, though the mathematical discontinuity of the vector field makes it difficult to defend this option on grounds of mathematical convenience.

If one chooses to restrict one's attention to models of GTR + dust that do have dust everywhere and always, such gerrymandering is simply changing the subject to consider a different theory. If one takes a semantic view of theories, then restricting attention to such a proper subset of models is just to discuss some new theory besides GTR + dust, namely GTR + omnipresent dust. The new theory GTR + omnipresent dust has an absolute object. But why shouldn't it? Surely no one has well founded intuitions to the contrary. In any case, the theory of interest was GTR + dust, not GTR + omnipresent dust. Anderson anticipated the fact that one could consider a

[6] One need not commit oneself to J^μ as primitive and U^μ as derived. I am indebted to Don Howard for insightful probing about choices of primitive variables, as well as a suggestion concerning this paper's title.

proper subset of models for which some field would count as absolute without counting as absolute for the full set of models. He wrote:

> We should perhaps emphasize that we are discussing here universal absolute objects, which must appear in the description of every [dynamically possible trajectory] of our space-time description. It is quite possible that, for a subclass of [dynamically possible trajectories], one or more dynamical objects satisfy the criteria of Section 4-3 and so play the role of absolute objects for those [dynamically possible trajectories].. . . The existence of such special subclasses of [dynamically possible trajectories] as those discussed above does not, of course, constitute a violation of the principle of general invariance as we have formulated it. Only the existence of universal absolute objects would do so. (Anderson, 1967) (pp. 339, 340)

Thus Anderson reminds us that absolute objects are universal, not (so to speak) provincial like the dust 4-velocity. While the dust 4-velocity constitutes an absolute object for the theory GTR + omnipresent dust, it does not constitute an absolute object for GTR + dust due to the failure of universality. Thus Friedman's intuition, as modified above, is vindicated. The alleged Jones-Geroch counterexample fails to count as an absolute object for GTR + dust and thus fails to undermine Friedman's analysis after a slight amendment using Andersonian resources.

One might summarize Friedman's reply, as amended above, as follows: Geroch's merely mathematical vector field is irrelevant and eliminable because it does no physical work, while Jones's dust application of the vector field does physical work but violates the condition of being meaningful and everywhere nonvanishing in all models and so violates the diffeomorphic equivalence needed for absoluteness. At this stage a summary might be useful. Physics literature previously unappreciated by philosophers of physics has been shown to shed light on the Jones-Geroch counterexample to Friedman's (and likely Anderson's or TLL's) definition of absolute objects. An old result from Rosen vindicates Maidens's claim that Hiskes's redefinition of absolute objects could not be used to eliminate the Jones-Geroch counterexample without generating a new counterexample. The neglected but valuable papers by TLL and LLN and some infrequently attended parts of Anderson's book proscribe irrelevant variables, a fact with important consequences. This proscription can be used to exclude Rosen's trick for deriving flat space-time from a variational principle. Then Anderson's generalization that absolute objects are variational and *vice versa* would seem to be rehabilitated, at least provisionally, though the clock fields of parametrized theories pose further questions. If variationality cannot be invoked to remove the Jones-Geroch counterexample, then some new move is required. Again the Anderson-TLL proscription of irrelevant variables is helpful, in spirit if not in letter. Excluding locally irrelevant values of the field U^{μ}, which purports to be the 4-velocity field of dust, would imply that U^{μ} is undefined wherever the dust vanishes, while the mass current J^μ vanishes there. Alternatively, U^μ and J^μ both vanish there. Either way, GTR + dust fails to have an everywhere nonvanishing timelike vector field that exists in all models. Thus a slight amendment of the Anderson-Friedman tradition using the Andersonian opposition to irrelevant variables eliminates the Jones-Geroch counterexample.

5 Spinors, Confined Variables, and Conjectures

Addressing the Jones-Geroch vector field does not clear the air of every possible question about the Anderson-Friedman project. I close with a collection of features of some or all spacetime theories that could use further attention and conjectures regarding what form that attention might take. Above two further potential counterexamples to Anderson's generalization were mentioned: the timelike leg of an orthonormal tetrad in a theory of gravity coupled to a Dirac spinor, and the clock fields of parametrized theories. Clock fields in parametrized theories, if not irrelevant, apparently will count as absolute objects despite being variational. Such a result is not unreasonable, but some details need careful attention, which I hope to provide on another occasion. Before discussing the spinor-tetrad issue, it is worthwhile to consider Anderson's treatment of spinors of the Dirac equation in a gravitational field (pp. 358-360). Anderson entertains the worry that γ^μ might be an absolute object in flat spacetime, in fact one with a symmetry group smaller than the Poincaré group (though in this context γ^μ is not a vector under *arbitrary* coordinate transformations, so it is not eligible to be an absolute object by Anderson's standards, it would seem). Turning to curved spacetime, Anderson avoids using an orthonormal tetrad by using variable Dirac matrices γ^μ satisfying $\gamma^\mu \gamma^\nu + \gamma^\nu \gamma^\mu = 2g^{\mu\nu} I$. What follows is a formalism with an internal symmetry group (apparently global) unrelated to the group of spacetime mappings. However, the implicit relationship between γ^μ and $g^{\mu\nu}$ leaves obscure what a suitable action principle might be for deriving the Einstein-Dirac equations and what variables it would involve. Thus one can hardly even test Anderson's formalism for absolute objects; his treatment of spinors is just incomplete. By contrast the spinor-tetrad formalism avoids such difficulties.

One often reads that coupling a spinor field to gravity *requires* an orthonormal tetrad (Weinberg, 1972; Deser and Isham, 1976; Fatibene and Francaviglia, 2003). However, the threat of a counterintuitive absolute object then arises. Given both local Lorentz and coordinate freedom, one can certainly bring the timelike leg into the component form $(1, 0, 0, 0)$ at least in a neighborhood about any point, which is enough for absoluteness by Hiskes's standards. (Aligning the tetrad with the simultaneity hypersurfaces is known as imposing the time gauge on the tetrad (Deser and Isham, 1976).) Unlike the dust case, there cannot be any spacetime region in any model such that the timelike leg of the tetrad vanishes. Thus GTR coupled to a spinor field using an orthonormal tetrad gives an example of a Gerochian vector field: nowhere vanishing, everywhere timelike, gauge-equivalent to $(1,0,0,0)$, and allegedly required to couple the spinor and gravity and thus not irrelevant. Like clock fields, the timelike tetrad leg also appears to be both variational and absolute. The spinor-tetrad example seems rather more serious a problem for definitions of absolute objects than the Jones-Geroch cosmological dust example, because the spinor field is surely closer to being a fundamental field than is dust or any other perfect fluid. Spinors (actually vector-spinors for spin $\frac{3}{2}$) are also required in supergravity, where internal and external symmetries are combined. On another occasion I hope to explain how to remove irrelevant variables here and thus avoid this unexpected absolute object.

Some other features of spacetime theories that fit awkwardly with Anderson's and Friedman's accounts might be addressed using TLL's third category of *confined* variables (Thorne et al., 1973), which does not overlap with the absolute objects but has some similarities. "The confined variables are those which do *not* constitute the basis of a faithful representation of the [manifold mapping group]" (Thorne et al., 1973) (p. 3568), which means (p. 3567) that there exist two distinct elements of the manifold mapping group that produce identical mappings of the confined variables. TLL list universal constants as examples of confined objects. Indeed it is clear that structures that do not change at all under coordinate transformations are confined objects. Some other examples of things unaffected by coordinate transformations that come to mind include the identity matrix, the Lorentz matrix $diag(-1,1,1,1)$, fixed Dirac γ^μ matrices, Lie group structure constants, and Oswald Veblen's "numerical tensors" (though Veblen's "tensors" include tensor densities). The numerical tensors are the Kronecker δ^μ_ν symbol, which is trivially a world tensor, and the Levi-Civita totally antisymmetric ϵ symbol with values 1, -1, and 0; these values are the components of both a contravariant tensor density of weight 1 and a covariant tensor density of weight -1 (Veblen, 1933; Anderson, 1967; Spivak, 1979). It has been suggested by Harvey Brown that the signature of the metric is importantly like an absolute object (Brown, 1997; Maidens, 1998). If the signature were an absolute object in the strict sense, then GTR would have an absolute object, contrary to Anderson's diagnosis of the novelty of GTR. Anderson's and Friedman's works perhaps have no category for expressing this immutable, externally prescribed nature of the metric signature, because absolute objects are supposed to be geometric objects (tensor fields and the like). The fact that the spacetime metric signature is unaffected by diffeomorphisms suggests that it counts as a confined variable in the richer TLL taxonomy. The requirement that absolute objects form a faithful realization of the theory's covariance group is something that TLL carry over from Anderson (Anderson, 1967) (p. 83), though they have different definitions of faithfulness (Thorne et al., 1973) (p. 3577), as was noted above. Another issue worthy of consideration is the global topology of spacetime, which seems rather neglected in the literature on absolute objects, apart from the work of Hiskes (Hiskes, 1984) and Friedman (Friedman, 1983). The topology of spacetime is certainly untouched by diffeomorphisms, so it also might be treated as a confined variable. Thus the TLL amendments to Anderson's project already accommodate some intuitively absolute-like features of spacetime theories that do not fit the traditional definitions of absolute objects.

Finally, one might consider Roberto Torretti's example (Torretti, 1984) of a theory of modified Newtonian kinematics in which each model's space has constant curvature, but different models have different values of that curvature. Because every model's space has constant curvature, such a theory surely has something rather like an absolute object in it. Though contrived, this example is relevantly like the cases of de Sitter or anti-de Sitter background metrics of constant curvature that are sometimes discussed in the physics literature (*e.g.*, (Rosen, 1978; Logunov et al., 1991)), where one often lumps together space-times with different values of constant curvature. The failure of the metrics to be locally diffeomorphically equivalent for distinct curvature values prevents such theories from satisfying Anderson's, Friedman's, or TLL's definition of an absolute object. It is unclear that even the resources of the TLL and LLN papers accommodate the Torretti example under any category. Perhaps absolute objects cannot be analyzed adequately, but still one knows an absolute object when one sees it.[7] On the other hand, sometimes progress is made by modifying vague questions (about fixed background structure in this case) until they admit precise answers. Perhaps the problem lies not in the Andersonian tradition, but in the claim that these models with different curvature values are best regarded as part of the same theory. If Anderson's project is sufficiently attractive, might not the notion of a scientific theory be revisable to accomodate that project? Maybe Torretti's theory is gerrymandered, the result of taking the union of several related but naturally distinct sets of models; similar comments could be made regarding Norton's modification of Torretti's example to Robertson-Walker metrics (Norton, 1993) (p. 848). Perhaps a syntactic view of theories as defined by (nonarbitrary?) postulates, rather than a semantic view of theories as (arbitrary?) collections of models, would be helpful here. One might regard models with different spatial curvatures as belonging to different theories that are instances of a common theory schema.[8] This question should be connected with discussions of the status of those universal constants of nature that are "put in by hand" in the laws of nature, if such constants exist (Duff et al., 2002). It might also be worth considering whether models with differing space-time topology should be regarded as belonging to different theories in a common theory schema.

[7] I thank Harvey Brown for defending such a view in discussion.

[8] I thank Don Howard for suggesting the relevance of the syntactic *vs.* semantic debate and supplying a helpful vocabulary here.

6 Conclusion

The discussion of various supposed counterexamples to the Anderson-Friedman project reveals that progress has been made toward having all and only those intuitively absolute entities count technically as absolute objects. The Jones-Geroch counterexample is resolved by the exclusion of locally irrelevant mathematical entities. The Torretti counterexample might perhaps best be viewed as gerrymandered. Other absolute-like entities find a home in the TLL category of confined objects. Further work on spinors will likely show that orthonormal tetrads are partly irrelevant in GTR + spinors and so do not bring in a Gerochian nonvanishing timelike vector field to count, absurdly, as an absolute object. One would hope for a similar outcome for supergravity. Parametrized theories are also worth some attention.

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